THE

JOURNAL OF SCIENCE,

AND ANNALS OF

BIOLOGY, ASTRONOMY, GEOLOGY, INDUSTRIAL ARTS, MANUFACTURES, AND TECHNOLOGY.

(MONTHLY, FORMERLY "THE QUARTERLY JOURNAL OF SCIENCE.")

EDITED BY

WILLIAM CROOKES, F.R.S., &c.

VOL. I. (THIRD SERIES.)
VOL. XVI. (O.S.)
1879

LONDON:
3, HORSE-SHOE COURT, LUDGATE HILL.

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NOTICES OF SCIENTIFIC WORKS.

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THE QUARTERLY

JOURNAL OF SCIENCE.

JANUARY, 1879.

I. ON THE THICKNESS OF THE ANTARCTIC ICE, AND ITS RELATIONS TO THAT OF THE GLACIAL EPOCH.

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IN a recent work* I have endeavoured to show that the thickness of the ice on the Antarctic continent must be far greater than is generally supposed; that whatever be its depth at the edge of the continent, where it breaks up into bergs, the thickness at the Pole or centre of dispersion must be enormous.

Since the publication of my work, however, Sir Wyville Thomson, Director of the Scientific Staff of the *Challenger* Expedition, in a lecture on the condition of the Antarctic regions, delivered at Glasgow, has come to a totally different conclusion. His conclusion is based chiefly on considerations relating to the principle of regelation and the physical nature of ice; and the following quotation from his lecture will show his views on the subject:—

"There is one point in connection with the structure of icebergs which is of great interest, but with regard to which I do not feel in a position to form a definite judgment. It lies, however, especially within the province of a distinguished professor in the University of Glasgow, Dr. James Thomson, and I hope he will find leisure to bring that knowledge to bear upon it which has already thrown so much light upon some of the more obscure phenomena of ice. I have mentioned the gradual diminution in thickness

* Climate and Time, Chap. XXIII.

of the strata of ice in a berg from the top of the berg downwards. The regularity of this diminution leaves it almost without a doubt that the layers observed are in the same category, and that therefore the diminution is due to subsequent pressure or other action upon a series of beds which were at the time of their deposition pretty nearly equally thick. About 60 or 80 feet from the top of an iceberg the strata of ice, a foot or so in thickness, although of a white colour, and thus indicating that they contain a quantity of air and that the particles of ice are not in close apposition, are still very hard, and the specific gravity of the ice is not very much lower than that of layers not more than 3 inches thick nearer the water-line of the berg. Now it seems to me that this reduction cannot be due to compression alone, and that a portion of the substance of these lower layers must have been removed.

"It is not easy to see why the temperature of the earth's crust, under a widely extended and practically permanent ice-sheet of great thickness, should ever fall below the freezing-point, and it is a matter of observation that at all seasons of the year vast rivers of muddy water flow into the frozen sea, from beneath the great glaciers which are the issues of the ice-sheet of Greenland. Ice is a very bad conductor, so that the cold of winter cannot penetrate to any great depth into the mass. The normal temperature of the crust of the earth at any point where it is uninfluenced by cyclical changes is, at all events, above the freezing-point, so that the temperature of the floor of the ice-sheet would certainly have no tendency to fall below that of the stream which was passing over it. The pressure upon the deeper beds of the ice must be enormous; at the bottom of an ice-sheet 1400 feet in thickness it cannot be much less than a quarter of a ton on the square inch. It seems therefore probable that, under the pressure to which the body of ice is subjected, a constant system of melting and regelation may be taking place, the water passing down by gravitation from layer to layer until it reaches the floor of the ice-sheet, and finally working out channels for itself between the ice and the land, whether the latter be sub-aërial or submerged.

"I should think it probable that this process, or some modification of it, may be the provision by which the indefinite accumulation of ice over the vast nearly level regions of the 'Antarctic Continent' is prevented, and the uniformity in the thickness of the ice-sheet is maintained; that, in fact, ice at the temperature at which it is in contact

with the surface of the earth's crust within the Antarctic regions, cannot support a column of itself more than 1400 feet high without melting."*

The subject is one of very considerable importance, not merely in relation to the Antarctic regions at the present day, but also in its bearings on the condition of things generally during the Glacial Epoch. For if Sir Wyville Thomson's conclusions in reference to the thickness of the Antarctic ice be true, they must hold equally true for the ice of the Glacial Epoch, and consequently would modify to a large extent prevailing conceptions regarding the physical condition of our country during that epoch.

They are therefore conclusions worthy of discussion, and, as they are diametrically opposed to those arrived at by myself, I have thought of considering the subject in somewhat fuller detail, the more so as new elements in the question have since been introduced.

At the very outset of the inquiry it must be observed that the question of the thickness of the ice covering the Antarctic continent is one which cannot be determined by direct observation. No one, as yet, has ever been able to set his foot on that continent; and the perpendicular wall forming the outer edge of its icy mantle is nearly all that has been seen of it. Direct measurements, and some other facts to which we shall shortly refer, show with tolerable certainty what is the probable average thickness of the ice-sheet at its outer circumference; but observation can tell us nothing whatever about the thickness of the ice in the interior, which is the question at issue. This has to be determined by purely physical and mechanical considerations, based, it is true, on data derived from observation. A visit to the Antarctic regions may indeed enable us to become acquainted with data which we might not have known otherwise, but this acquaintance would not aid us in drawing the proper inference from those data.

It fortunately happens, however, that the very circumstances that render the region so difficult to get at are those which at the same time tend to simplify the problem. The Antarctic region is the most inaccessible on the globe, but of all regions it is the one where the physical conditions are most uniform and least under the influence of contingent circumstances, such as those resulting from the presence of warm ocean currents in one place and cold currents in another, or of great masses of land in one part

* Condition of the Antarctic Region, p. 23. Nature, vol. xv., p. 122.

and an open sea in another. We have not in the Antarctic, as in the Arctic region, well-marked warm and moist aërial currents and cold and dry winds blowing athwart different areas. Surrounding the South Polar continent lies an unbroken ocean, in an almost uniform climatic condition. This region also, as Sir Wyville Thomson remarks, is "continuously solid,—that is to say, it is either continuous land or dismembered land fused into the continental form by a continuous ice-sheet." In this case we can treat it as one continuous continent. The South Pole being safely assumed to be in the centre of the sheet, we have here what we perhaps never had on the northern hemisphere even during the Glacial Epoch—a polar *ice-cap*. We have the pole in the centre of the cap; therefore, at equal distances from the pole or centre, the conditions in every respect, both as to climate and the thickness of the ice, may be assumed to be the same; for no reason can be assigned for supposing the conditions in separate areas upon the same parallel of latitude to differ. Thus, as a purely physical and mechanical problem, the conditions could hardly be more simplified.

We shall now enter into the consideration of the question. In the first place, the conclusion that "ice at the temperature at which it is in contact with the surface of the earth's crust within the Antarctic regions cannot support a column of itself more than 1400 feet high without melting" is in direct opposition to known facts.

The immense tabular icebergs found in the Southern Ocean, which have been so well described by Sir Wyville Thomson, are of course portions broken off the edge of the ice-sheet, and the thickness of the bergs represents the thickness of the ice-sheet at the place where they broke off. Now, some of these icebergs have been found to be more than three times the limit assigned by Sir Wyville. The following are a few out of the many examples which might be adduced of enormous icebergs, taken from the Twelfth Number of the "Meteorological Papers" published by the Board of Trade, and from the excellent paper of Mr. Towson on the "Icebergs of the Southern Ocean," published also by the Board of Trade.

Sept. 10th, 1856.—The *Lightning*, when in lat 55° 33′ S., long. 140° W., met with an iceberg 420 feet high.

Nov., 1839.—In lat. 41° S., long. 87° 30′ E., numerous icebergs 400 feet high were met with.

Sept., 1840.—In lat. 37° S., long. 15° E., an iceberg 1000 feet long and 400 feet high was met with.

Feb., 1860.—Captain Clark, of the *Lightning*, when in lat. 55° 20′ S., long. 122° 45′ W., found an iceberg 500 feet high and 3 miles long.

Dec. 1st, 1859.—An iceberg, 580 feet high and from 2½ to 3 miles long, was seen by Captain Smithers, of the *Edmond*, in lat. 50° 52′ S., long. 43° 58′ W. So strongly did this iceberg resemble land that Captain Smithers believed it to be an island, and reported it as such, but there is little or no doubt that it was in reality an iceberg. There were pieces of drift-ice under its lee.

Nov., 1856.—Three large icebergs, 500 feet high, were found in lat. 41° S., long. 42° E.

Jan., 1861.—Five icebergs, one 500 feet high, were met with in lat. 55° 46′ S., long. 155° 56′ W.

Jan., 1861.—In lat. 56° 10′ S., long. 160° W., an iceberg 500 feet high and half a mile long was found.

Jan., 1867.—The barque *Scout*, from the West Coast of America, on her way to Liverpool, passed some icebergs 600 feet in height and of great length.

April, 1864.—The *Royal Standard* came in collision with an iceberg 600 feet in height.

Dec., 1856.—Four large icebergs, one of them 700 feet high and another 500 feet, were met with in lat. 50° 14′ S., long. 42° 54′ E.

Dec. 25th, 1861.—The *Queen of Nations* fell in with an iceberg in lat. 53° 45′ S., long. 170° W., 720 feet high.

Dec., 1856.—Capt. P. Wakem, ship *Ellen Radford*, found, in lat. 52° 31′ S., long. 43° 43′ W., two large icebergs, one at least 800 feet high.

Mr. Towson states that one of our most celebrated and talented naval surveyors informed him that he had seen icebergs in the southern regions 800 feet high.

March 23rd, 1855.—The *Agneta* passed an iceberg in lat. 53° 14′ S., long. 14° 41′ E., 960 feet in height.

Aug. 16th, 1840.—The Dutch ship *General Baron Von Geen* passed an iceberg 1000 feet high in lat. 37° 32′ S., long. 14° 10′ E.

From the fact that the ice forming the upper layers of the iceberg is less dense than that of ordinary ice, Sir Wyville Thomson estimates that as much as one-seventh part of the berg may be above water-line. But for the following reasons I am unable to agree with this estimate. It is true, as he remarks, that the white ice which forms the upper portion

of the berg is less dense than ordinary ice, being composed of recent snows; but, on the other hand, this will be counterbalanced by the greater density of the lower portions of the berg which have been subjected for ages to enormous pressure. I hardly think that there is any good reason to conclude that the *mean* density of the bergs is much under that of ordinary ice, namely, 0·92.*

But even if we admit that as much as one-seventh of the berg is above water, still a berg 500 feet in height would be 3500 feet in thickness, and one 600 feet would be 4200 feet thick, while one 720 feet high, of the tabular form, would be 5040 feet, or nearly a mile in thickness.

It would not, of course, be safe to conclude that the thickness of the ice below water bears always the same proportion to the height above. If the berg, for example, be much broader at its base than at its top, the thickness of the ice below water would bear a less proportion than that indicated by the difference of specific gravity of ice and water. But a berg such as that recorded by Captain Clark, 500 feet high and 3 miles long, may be relied upon as having the proportionate thickness under water. The same may be said of the one seen by Captain Smithers, which was 580 feet high, and so large that it was taken for an island.

It may be here remarked that a berg does not stand higher out of the water because the lightest side happens to be uppermost. The height above water is determined by the *mean* density of the berg, and is the same no matter how the various densities may be distributed through the mass. It would be the same though the berg were turned upside down. This follows as a necessary consequence from the fact that the amount of water displaced by the berg is equal to its weight, and of course it is the same whatever side be uppermost.

To evade the force of the evidence derived from the testimony of the icebergs, it is asserted by some that the heights thus recorded are mere guesses, and not the result of actual measurement. But such an opinion is in direct contradiction to the express declaration of Admiral FitzRoy, who

* It is true that, from observations made (Quart. Journ. Geol. Soc., Feb., 1877) on the density of ice in Disco Bay, Mr. Amund Helland found that, in consequence of the amount of air-bubbles contained in the ice, its density was only 0·886, and from this he concluded that one-seventh of the bergs was above water. But he does not state at what part of the berg his specimens were taken. If they were taken from near the top, or even at the water-line, it might have been expected that the density would be very considerably under that of ordinary ice.

collected the evidence on the subject. He states that "by angular and reliable measurements some of them have been found to be six or eight hundred feet high and several miles in circumference."

But more than this, if Capt. Smithers, for example, did not actually measure the iceberg to which we have referred, he could not have known that its height was 580 rather than 600 feet. The very fact that he stated it to be 580, and not 500 or 600, or even 550 feet, surely implies that he really measured it. The assertion that a person is, for example, 5 feet 6 inches or 5 feet 8 inches in height would not imply that this was his measured height; but if we asserted that he was 5 feet 6½ inches or 5 feet 8¾ inches high, we should necessarily convey this impression. In like manner Captain Smithers, by assigning to the iceberg an altitude so particular as that of 580 feet, distinctly conveys the impression that such was the height obtained by actual measurement. Similarly we conclude that the captains of the *Queen of Nations* and the *Agneta* actually measured the icebergs which they respectively declared to be 720 and 960 feet high.

In reply it may perhaps be asserted that no record of an iceberg 500 or 600 feet in height is to be found in the log-books of the Navy, and that all those instances of enormous icebergs have been given by masters of merchant vessels, who as a rule are not so competent to make accurate measurements. It is doubtless true that the latter are generally not so well qualified for such work as naval officers; but it is hardly credible that they should all have gone so far astray in their measurements as to estimate heights at 500 and 600 feet which in reality were only 200 feet. Now, if but one berg 500 feet high has ever been seen in the Southern Ocean, it is proved that even twice 1400 feet is not the limit of the thickness of the Antarctic ice.

But it is not true that no naval officer has met an iceberg of those enormous dimensions; for, as we have already seen, Mr. Towson states that one of our most celebrated and talented naval surveyors informed him that he had met icebergs in the southern regions 800 feet high. It is, however, not to be wondered at that so few naval officers have seen such bergs, for they are of very rare occurrence. They have been met with chiefly in latitudes that are traverssd by thousands of merchant ships for one vessel belonging to the Navy. And perhaps not one out of every ten thousand merchantmen has ever fallen in with one of the great ice-islands we now speak of.

The testimony from icebergs may therefore be regarded as decisive against the opinion that the Antarctic ice cannot be more than 1400 feet thick.

That 1000 or 2000 feet cannot be the limits of thickness attained by continental ice is amply proved by the geological evidence, which goes to show that during the Glacial Epoch the ice in some places much exceeded 1400 feet. Prof. Dana, for example, has proved that during the period in question the thickness of the ice on the American continent must in many places have been considerably above a mile. He has shown that over the northern border of New England the ice had a mean thickness of 6500 feet, while its mean thickness over the Canada water-shed, between St. Lawrence and Hudson's Bay, was not less than 12,000 feet, or upwards of 2¼ miles (see "American Journal of Science and Arts" for March, 1873). Prof. Erdmann and Mr. Amund Helland have shown that the ice in some parts of Scandinavia was at least 6000 feet thick. It has been proved, by M. Guyot and others, that the Great Valley of Switzerland was formerly filled with a mass of ice between 2000 and 3000 feet in thickness. Mr. Jamieson found that the isolated mountain of Schiehallion, in Perthshire, 3500 feet high, is marked near its top as well as on its flanks, and this not by ice flowing down the side of the hill itself, but by ice passing over it from the north. Dr. James Geikie has shown* that the ice between the mainland and the Outer Hebrides was as much as 3700 feet in thickness. The great mass of ice from Scandinavia, filling the Baltic and the North Sea during the Glacial Epoch, must have been over 3000 feet thick at least.†

The Temperature of the Antarctic Ice.

In examining the physical reasons which have been advanced for the limit assigned to the thickness of the Antarctic ice-cap, we must first consider the probable temperature of the ice; for not only does the thickness of the sheet depend, as we shall see, to a considerable extent on the temperature of the ice, but misapprehensions on this point will tend to vitiate all our reasoning on the subject.

There are but three quarters from which the ice-cap can receive an appreciable amount of heat, viz., (1) the air above; (2) the earth beneath; and (3) the work of compression. Other sources can yield little, if any at all. For

* Quart. Journ. Geol. Soc., vol. xxxiv., p. 861.
† Climate and Time, Chap. XXVII.

instance, the amount carried inward horizontally from the outer edge of the cap by conduction must be infinitesimal, and indeed can never affect the interior, as the ice moves outward more rapidly than the heat can possibly travel inward.

Heat derived from Beneath.—We shall begin with the consideration of the heat derived by the bottom of the sheet from the earth's crust. The researches of Sir William Thomson enable us to determine with a tolerable degree of certainty the amount received from this source. He tells us that through every square metre of the earth's surface 220 metre-tons, or 1,613,700 foot-pounds, of underground heat pass upwards annually. Through every square foot, therefore, there must come 149,600 foot-pounds. This amount is sufficient to melt a layer of ice, already at the melting-point, one-fifth of an inch in thickness. But underground heat would probably be insufficient to melt even so small a layer, since a portion of the heat must, doubtless, be expended in passing through and maintaining at the melting-point a few inches of the ice at the bottom of the sheet.

At first we might be apt to suppose that underground heat ought to travel up through the ice in the same way as through the strata of the earth below, and to make its presence sensibly felt at no great distance from the surface of the sheet. This, however, is impossible; for (1) the greater part of the heat is spent not in raising the temperature, but in melting the ice; and (2) the ice when melted immediately runs off, carrying the heat along with it. But it will be replied, that, notwithstanding this, if the temperature of the ice be much below the freezing-point, the heat constantly passing into the solid layers at the bottom of the sheet, though trifling, ought in course of ages to pass up through the ice, affecting its temperature not for a few inches, as I have supposed, but for a thickness of a great number of feet. Were the ice, like the ground underneath on which it rests, to remain immovable, this would no doubt be the case; but the sheet is in a state of constant motion outwards from the centre of dispersion, and no sooner is a particle of the ice heated than it moves away, and its place is supplied by another particle from behind, which in turn requires to be heated. Besides, the ice has always a downward as well as a horizontal motion; for all the ice found at the bottom comes primarily from the top, and that removed from below is replaced from above. Hence not only

is internal heat from below carried away by the horizontal flow of the ice, but the upward motion of the heat is checked by a downward flow of the ice from above; and the ice is, in all probability, moving downwards more rapidly than the heat is travelling upwards. We must therefore conclude that underground heat is confined to a very thin layer of the cap at the bottom, and that its effects, either in melting the ice or in raising its temperature, are so trifling that they may be practically disregarded in our present inquiry.

It must further be observed that when it is stated that underground heat will maintain at the melting-point the ice in contact with the ground, it is not meant that it will maintain it at the temperature of 32°, for, as Prof. James Thomson discovered, the temperature at which the ice melts is lowered by pressure at the rate of about 0·0137° F. for every atmosphere of pressure. In the present case the pressure depends upon the thickness of the ice: so that, if the sheet be 1400 feet deep, the melting-point will be 31·5°; if half a mile deep, it will be 31°; if 1 mile deep, 30°; and so on.*

Heat derived through the Upper Surface.—It follows, from what has already been shown, that the greater part, if not nearly all, the heat possessed by the ice must have been received through the upper, and not the under, surface of the sheet. But what we are at present concerned with is not so much the amount of heat received by the ice as the temperature at which the heat can maintain the ice.

* The melting-point does not, however, vary uniformly with the pressure; for Mousson (Ann Chim. et Phys., 3rd series, lvi., p. 257, 1859) found that it required a pressure of 13,000 atmospheres to lower the melting-point to zero, F., whereas if the melting-point had decreased in proportion to the increase of pressure, a pressure of 2337 atmospheres would have been sufficient. Boussingault succeeded in lowering the melting-point 11° below zero, F., but the amount of pressure employed was not determined (Ann. Chim. et Phys., xxvi., p. 544, 1872).

The fact that the melting-point of ice would be lowered by pressure, or rather that pressure would prevent freezing, was suggested nearly a century ago by Dr. Charles Hutton, Professor of Mathematics in the Military Academy of Woolwich. From certain experiments on the expansive force of ice, made in Canada by Major Williams, in the year 1784-85, Dr. Hutton makes the following remarks:

"From these ingenious experiments we may draw several conclusions:—First. We hence observe the amazing force of the expansion of the ice, or the water in the act of freezing; which is sufficient to overcome perhaps any resistance whatever; and the consequence seems to be, either that the water will freeze, and, by expanding, burst the containing body, be it ever so thick and strong; or else, if the resistance of the body exceeds the expansive force of the ice, or of water in the act of freezing, then, by preventing the expansion, *it will prevent the freezing, and the water will remain fluid, whatever the degree of cold may be.*" (Trans. Roy. Soc. Edin., vol. ii., p. 27.)

In short, the question to be determined is—What is the temperature of the Antarctic ice? Now, if nearly all the heat possessed by the ice has been received from the upper surface of the sheet, the temperature of the mass must be mainly determined by that of the surface, and cannot be far above the mean temperature of the surface. If so, the temperature of the ice must evidently be very considerably below the freezing-point.

(1.) If we suppose the heat to be transmitted from the surface downwards by *Conduction*, we must necessarily conclude that the surface is at a higher temperature than the ice below; for conduction can only take place from a hot to a colder body, and this process could not possibly maintain the mass of the ice below at a temperature equal to the mean temperature of the surface. The general tendency of conduction would, therefore, be to keep the ice beneath at a lower temperature than that at the surface.

(2.) The work of *Radiation*, however, would probably have the opposite tendency. The heat received by direct radiation from the sun could not possibly raise the temperature of the ice above 32°, but the heat lost by radiation might lower the temperature to far more than 32° below zero. If the heat received from the sun's rays should keep the surface of the ice at, say, 32° during the summer, and the heat lost by radiation should keep the surface at −32° during winter, which is not an extravagant supposition, the mean temperature of the surface would then be 0°. But the mean temperature of the underlying ice would not be so low; for the low mean temperature of the surface is almost wholly due to loss by radiation into stellar space during winter, and this loss would be chiefly confined to the surface. Had the surface been rock instead of ice the rise of temperature during summer would have been about as great as the decrease during winter, and consequently the mean temperature would have been 32° instead of 0° as in the case of ice. Hence the difference between the mean temperature of a rock surface and that of the rock below would not be so great as in the case of ice. The tendency of direct radiation, therefore, is to maintain the surface of the ice-sheet at a lower temperature than that of the underlying mass.

(3.) This tendency is strengthened by another circumstance which comes into operation. During summer a large portion of the direct heat from the sun is spent in melting the surface ice. The melted ice passes down through crevasses and openings in the sheet, thus carrying the temperature along with it. The heat of summer is by

this means carried down below the surface, but not so the cold of winter.

The melting of the ice on the Antarctic continent will be greatly retarded, however, by the coldness of the air, the temperature of which, even during summer, is considerably below the freezing-point. A wind, a few degrees below the freezing-point, blowing on the icy surface would probably re-freeze the ice as rapidly as the sun's rays could melt it. These conditions differ entirely from those that obtain in the Arctic regions. In the latter the air in summer is above the freezing-point, and consequently assists the sun in melting the ice, whereas in the Antarctic regions it is below the freezing-point, and tends to prevent the sun from melting the ice. This circumstance explains the fact, which so much surprised Sir James Ross, that no streams of water flow off the Antarctic ice, similar to those that escape from the great ice-fields of Greenland.

Such water as formed on the surface could not penetrate to any considerable depth, for the ice, as we shall presently see, being much below the freezing-point, the water would be re-frozen before it could descend to any great depth.

It therefore follows that the great mass of ice, up to within a short distance of the surface, can be very little affected by heat transmitted either by conduction, or by radiation, or by water from ice melted at the surface.

Heat derived from Work of Compression and Friction.—I shall now consider the third and last source from which the ice can obtain heat, viz., *Work of Compression and Friction.* We are fortunately able to come to a pretty definite conclusion in regard to the total amount of heat derivable from this source. The force employed is Gravity, and we can thus determine with certainty the greatest amount of work which that can possibly perform on the ice. Mere pressure, however great, cannot of course generate heat unless it perform work, and the heat thus generated is not proportionate to the pressure, but to the work performed, and the amount of work done by pressure is proportionate to the space through which the pressure continues to act. When the pressure is gravity, the work is measured by the distance that the body is allowed to descend. A pound weight descending 1 foot performs 1 foot-pound of work; descending 2 feet it performs 2 foot-pounds; and so on in proportion to the number of feet of descent. In estimating the total amount of work which gravity can perform in the descent of a glacier down the side of a mountain, we measure the work by the vertical distance the glacier

descends; but in the case of the Antarctic ice-cap, the slope of the ground does not enter as an element into our calculations, for the ground is assumed to have no slope, the continent being regarded as flat. The surface no doubt may have great irregularities, such as hills and mountain ridges; those irregularities, however, do not assist gravity, but rather act as obstructions to the general flow of the ice.

Nevertheless, just as in the case of a glacier, the amount of work that gravity can perform is determined by the distance the ice can descend; and this distance is determined not by the slope of the ground, but by the thickness of the sheet. If the Antarctic ice-sheet be 1400 feet in thickness the greatest distance to which a pound of ice can descend is of course 1400 feet. Gravity acting on this pound of ice can therefore perform only 1400 foot-pounds of work. But, in order that gravity may do so, the pound must descend the whole distance from the surface to the bottom of the sheet. In estimating the total amount of heat which could possibly have been conferred on the ice by gravity, we must find the mean vertical distance to which the ice has descended. This of course, in the present case, is equal to half the thickness of the sheet, viz., 700 feet. If 1400 feet, as Sir Wyville Thomson supposes, be the thickness of the Antarctic ice-cap, 700 foot-pounds per pound is the utmost quantity of work that gravity can have performed on the ice. Supposing the whole of this work had been employed in heating the ice by compression, or by the friction of the particles of the ice on one another, or on the rocky floor of the sheet, the heat generated would not have amounted to one thermal unit per pound of ice. The specific heat of ice being about one-half that of water, the total work of compression—assuming that it had all been converted into heat, and the heat equally distributed through the entire mass of the cap—would not have raised the temperature of the ice by 2°.

The foregoing considerations do not afford a means of determining what the actual temperature of the great mass of the ice below the surface is. They show, however, that whatever that temperature may be it is not very materially affected either by the heat of compression, or by underground heat, or by that transmitted from the surface either by conduction or by melted ice.

On what, then, does the temperature of the ice mainly depend?

Temperature of the Ice determined by the Temperature of the Surface.—The temperature of the great mass of the ice is

mainly determined by the mean temperature of the upper surface of the sheet. All the ice down to the bottom of the sheet originally came from the surface. It once existed a the surface in the form of a coating of snow, which, be coming consolidated into ice, was afterwards covered over with fresh layers of snow, while these in turn passing into ice, were buried under succeeding snows, and so on. The ice that formed the surface a century ago now lies buried below the ice of a hundred years, and a hundred years hence its present position will be occupied by the surface ice of to-day. There is not only a constant motion of the ice from the pole outwards, but a constant downward motion as layer by layer is successively formed on the surface.

From what has been proved regarding the small quantity of heat which can be directly transmitted through the ice, it follows that the superficial layers will carry down with them pretty much the same temperature which they possessed at the surface at the time when they were covered up by succeeding snows. Any heat which they can derive from the work of compression, as has been shown, is but trifling. Heat transmitted by conduction could not possibly raise the temperature of the underlying ice above that of the surface; neither could the heat from direct radiation, nor that derived from melted ice.

As the temperature of the ice, then, cannot be much above the mean temperature of the surface, which is far below the freezing-point, it follows that the underlying mass must also be below the freezing-point. The very low temperature of the superficial layers is due to the fact that the mean temperature of the air above the surface is far below the freezing-point—a temperature which the icy surface cannot much exceed. The sun during summer may possibly heat the air sometimes above the freezing-point, but it cannot, of course, so raise the temperature of the ice without melting it.

Again, as solid ice is a better radiator than gaseous air, the surface of the sheet during winter would probably have its temperature lowered by radiation to a greater extent than the air. The probability is that the mean annual temperature of the surface is as low as, if not lower than, that of the air over it. And although the mean temperature of the regions around the South Pole has not been ascertained by direct observation, yet it certainly cannot be higher, but is probably much lower* than, that of those around the North Pole, which we know is but a few degrees above zero, F.

* See Climate and Time, p. 63.

Now if the mean annual temperature of the air over the Antarctic ice-sheet be about zero, F., then that of the surface of the sheet cannot be much higher; and if this be so, it follows, from what has been already advanced, that the temperature of the great mass of the ice down to near the bottom of the sheet must be considerably below the freezing-point.

Temperature of the Ice in some Regions determined by Pressure.—In regions such as Switzerland, where the mean temperature is above the freezing-point, the temperature of the ice in the interior of a glacier is not determined by the air above. The tendency of the air in this case is to keep the entire mass of the glacier at the melting-point. But as the temperature of the melting-point depends upon the pressure, the temperature of the glacier at any depth from the surface will depend upon the pressure to which the ice at that depth is subjected. At and near the surface, where the pressure is small, the temperature of the ice will be 32°. At the depth of a quarter of a mile, where the pressure would be equal to about 36 atmospheres, the temperature would be, not 32°, as at the surface, but 31°.5. And if the glacier were half a mile thick the temperature at the bottom would be 31°, and so on in proportion to the thickness of the glacier. This lowering of the melting-point could not, however, go on without limit, for in a country like Switzerland a point would soon be reached where the ice could no longer retain the solid form. If, for example, owing to the heat of the climate, we could not have ice at a lower temperature than say 30°, then a glacier over 1 mile in thickness would be an impossibility, for the bottom of a glacier of greater thickness would not remain solid at that temperature.

Having considered the various circumstances affecting the temperature of the Antarctic ice, and the sources from which it derives its heat, we have found that the temperature of the ice must be considerably under the freezing-point. We are now prepared to examine the reasons which have been adduced for concluding that about 1400 feet is the probable limit to the thickness of the Antarctic ice-sheet.

Examination of the Reasons for supposing that the Antarctic Ice-cap is not of Enormous Thickness.

1. *Limit to the Thickness of the Ice resulting from Melting produced by Pressure.*—Pressure will produce a melting of the ice in two totally different ways, viz., either by lowering the melting-point or by the work of compression. I shall consider both cases, and see if any ground is afforded by either in support of the conclusion that the Antarctic ice can be only one or two thousand feet in thickness.

(*a*.) *Melting produced by the Lowering of the Melting-point.*—The pressure exerted by a column of ice of half a mile in height would, as we have seen, lower the melting-point 1°; consequently, if the column were at the temperature of 32°, its base, being 1° above the melting-point, would not remain in the solid condition. To prevent the ice melting, the temperature of the base would require to be as low as 31°. Therefore, if 32° were the temperature of the Antarctic ice, Sir Wyville Thomson's conclusion, that the sheet cannot be more than 1400 feet in thickness, would follow as a matter of course.

But his supposition that, owing to internal heat coming through the earth's crust, the bottom of the Antarctic ice-sheet is kept at the temperature of 32°, cannot be sustained. It is this fundamental error, as I conceive it to be, which has led Sir Wyville astray, and induced him to believe that the ice cannot be of excessive thickness.

"The normal temperature of the crust of the earth," says Sir Wyville, "at any point when it is uninfluenced by cyclical changes, is, at all events, above the freezing-point, so that the temperature of the floor of the ice-sheet would certainly have no tendency to fall below that of the stream which was passing over it. . . . In fact, ice at the temperature at which it is in contact with the surface of the earth's crust within the Antarctic regions, cannot support a column of itself more than 1400 feet without melting."

In the question under consideration we are directly concerned with the temperature of the *surface* of the earth's crust only,—the floor on which the ice-sheet rests,—and not with the temperature below the surface. It is pefectly true that at considerable depths below the surface internal heat maintains a temperature above the freezing-point; but it is not true that it does so in reference to the surface. Under-

ground heat produces scarcely any sensible influence on the temperature of the surface, which is determined almost wholly by that of the air and other external agencies. The temperature, in short, is determined from *above*, and not from *beneath*. In warm countries, where the temperature of the air is high, that of the surface is high also. And so likewise in cold climates, the low temperature of the air gives a comparatively low temperature to the surface. Suppose our globe to be enveloped for some thousands of years with a covering at the uniform temperature of say 100°; and suppose, further, that 5000° should represent the temperature of the earth's mass; then, in such a case, there would be a gradual decrease of temperature from 5000° at the centre to 100° at the surface. Let us suppose now the warm covering is removed, and replaced by one at −100°. In the course of some thousands of years there will be a gradual decrease of temperature from 5000° at the centre, as before, to −100° at the surface. *Internal* heat limits the temperature at the centre, but *external* heat limits in every case the temperature at the surface.

To maintain, as Sir Wyville Thomson does, that 32° is the temperature of the floor on which the Antarctic ice-sheet rests, is virtually to beg the whole question at issue. It is the temperature of the ice that determines that of the floor on which it rests, and not the latter that determines the former.

What the temperature of the ground under the Antarctic ice-sheet may be is a question which at present we have no means of determining with certainty; we know only that it must be far below the freezing-point, for the ice resting on it is considerably under that point.

Although the temperature of the ice must impose a limit to the thickness of the sheet, underground temperature cannot do so, for the temperature of the ice is not determined by underground heat.

But supposing we knew the temperature of the Antarctic ice, yet this knowledge would not enable us to determine with certainty the limit imposed by temperature on the thickness of the ice. For, excepting in cases where the temperature is but very little below 32°, we are at present, in the absence of experiments, unable to say what would be the amount of pressure necessary to lower the melting-point to any assigned temperature. The experiment of Mousson shows that Prof. Thomson's formula, $t = 0{\cdot}0137°n$, does not hold true when the pressure is excessively great. A pressure of 73 atmospheres will lower the melting-point from 32° to

31°; but if Mousson's experiment is to be depended upon, a pressure of 400 atmospheres would be necessary to lower the melting-point from 1° to 0°. That is to say, were sufficient pressure applied to lower the melting-point to 1°, it would require an additional 400 atmospheres to lower it to 0°. The *rate* at which the melting-point is lowered by pressure is evidently not uniform, but decreases with the increase of pressure. Were the temperature of the ice at the South Pole as low as 32° below the freezing-point, which doubtless it is not, it would according to Mousson's experiment support a thickness of not less than 90 miles. But if the rate did not diminish with the pressure, but remained uniform, a pressure of 16 miles would be the limit.

From what has already been proved I think we may safely assume that the ice at the South Pole may be at least ten or twelve degrees below the freezing-point. We are unable to say what thickness of ice this temperature could support, but we know that it must be not under 6 nor over 30 miles.

But whatever the actual temperature of the Antarctic ice may be, if the sheet be as thick as the temperature will admit, then underground heat can never raise the temperature of the surface under the sheet sensibly above that of the ice. This is evident, because it cannot raise the temperature of the ice above the melting-point corresponding to the pressure, and the ice will always keep the floor at sensibly the same temperature as itself. In short, in determining the thickness of the Antarctic ice, underground heat does not enter as an element into our calculations, and, so far as the melting of the ice produced by the lowering of the melting-point is concerned, the Antarctic ice at the Pole may be a dozen of miles in thickness as readily as 1400 feet.

(*b.*) *Melting produced by Work of Compression and Friction.*— "The pressure upon the deeper beds of ice," says Sir Wyville Thomson, "must be enormous; at the bottom of an ice-sheet 1400 feet in thickness it cannot be much less than a quarter of a ton on the square inch. It seems therefore probable that, under the pressure to which the body of ice is subjected, a constant system of melting and regelation may be taking place, the water passing down by gravitation from layer to layer until it reaches the floor of the ice-sheet, and finally working out channels for itself between the ice and the land, whether the latter be subaërial or submerged."

I need hardly state, that no amount of pressure, however great, has the least tendency whatever to produce a melting of the ice by heat unless this pressure performs work, and the quantity of ice melted will then be not in proportion to the pressure, but to the work performed by the pressure. The pressure here referred to, which is supposed to produce the melting, is the *weight* of the ice, or, in other words, the force of gravity.

When considering the amount of heat derived from work of compression it was proved that, in the case of the Antarctic ice-sheet, the total amount of work which can possibly be performed by gravity is determined by the thickness of the sheet. It was shown that if 1400 feet be the thickness of the sheet, 700 foot-pounds per pound of the sheet is the greatest amount of work that gravity can perform. It follows therefore that, supposing the whole of the work is employed in heating the ice by compression and friction the heat thus generated would amount to only 0·9 of a thermal unit per pound of ice. It must be obvious that in the case of a flat and tolerably uniform sheet like the Antarctic, in which the pressure must of course be pretty evenly distributed, little or no melting can take place from this cause, as it requires not 0·9 of a thermal unit, but 142 thermal units, to melt a pound of ice already at the melting-point. The total work of 158 pounds of ice would need to be concentrated upon 1 pound in order to melt it. But such an unequal distribution of force in a sheet so uniform is at least extremely improbable. The tabular form of the southern icebergs, with their stratification parallel to their upper surface, shows the flat character of the ground on which they have been formed. This circumstance appears to have particularly struck Sir Wyville Thomson, as well as all who have visited the Antarctic regions. "The stratification," says Sir Wyville, "in all the icebergs which we saw was, I believe, originally horizontal and conformable, or very nearly so. I never saw a single instance of deviation from the horizontal and symmetrical stratification which could in any way be referred to original structure. . . . . As I have already said," he continues, "there was not, so far as we could see, in any iceberg, the slightest trace of structure stamped upon the ice in passing down a valley, or during its progress over *roches montonnées*, or any other form of uneven land; the only structure, except the parallel stratification, which we ever observed which could be regarded as bearing upon the mode of original formation of the ice-mass was an occasional local thinning out of some of the layers and

thickening of others,—just such an appearance as might be expected to result from the occasional drifting of large beds of snow before they have time to become consolidated."

The comparative absence of stones, gravel, or earth on the southern icebergs shows likewise the flat nature of the Antarctic ice-covering. "We certainly never saw," says Sir Wyville, "any trace of gravel or stones, or any foreign matter necessarily derived from land, on an iceberg."

But supposing we should make the extravagant assumption that in this comparatively flat and uniform sheet the pressure by some unexplained means is not evenly distributed, but that, on the contrary, it is all brought to bear on certain points and consumed in melting the ice, and that the total quantity of ice melted is the exact equivalent of the work performed by gravity; and let us further assume that the entire mass of the ice is already at the melting-point, and that, therefore, no work is required to raise its temperature; then the total quantity of ice melted would be of course $\frac{\cdot 9}{142}$ or $\frac{1}{158}$ of the entire mass. Gravity could perform only $\frac{1}{158}$ of the amount of work required to melt the entire sheet. If we suppose the sheet to be 1400 feet thick, then $\frac{1}{158}$ of this thickness will be equal to 9 feet. A layer of ice about 9 feet in thickness, therefore, is the total amount that gravity could under any circumstances have melted.

But more than this, it must be borne in mind that these 9 feet represent the total quantity which could be melted during the whole time the sheet was being formed; that is, from the time the bottom layer fell in the form of snow on the surface down to the present day. We have no means of ascertaining the length of this period. If we assume it to be 10,000 years, and this is probably an underestimate, then 9 feet of ice melted during that period would amount to only 1 inch in ninety-two years, or $\frac{1}{92}$ of an inch annually. But whether the period be 10,000 or 5000 years the quantity is so trifling that it may be practically disregarded in the present inquiry.

Nor is this all, for if the great mass of the ice be as much as 2° below the freezing-point, which it undoubtedly is, the total amount of heat generated by compression and friction during the 10,000 years would not suffice to raise the temperature of the ice even to the melting-point.

The Great Diminution in the Thickness of the Ice-strata from the Top downwards not due, as supposed, either to Compression or to Melting.—The thinness of the lower as compared with the more superficial strata of the ice-sheet is considered by Sir Wyville Thomson to be mainly, if not altogether, due to two causes, Compression and Melting of the Ice, particularly the latter. "The regularity of this diminution," he says, "leaves it almost without a doubt that the layers observed are in the same category, and that therefore the diminution is due to subsequent pressure or other action upon a series of beds which were at the time of their deposition pretty nearly equally thick. About 60 or 80 feet from the top of an iceberg the strata of ice are a foot or so in thickness, although of a white colour, and thus indicating that they contain a quantity of air and that the particles of ice are not in close apposition, are still very hard, and the specific gravity of the ice is not very much lower than that of layers not more than 3 inches thick nearer the water-line of the berg. Now it seems to me that this reduction cannot be due to compression alone, and that a portion of the substance of these lower layers must have been removed."

If the layers 3 inches thick near the water-line were once a foot in thickness, as no doubt they were, then this great diminution in thickness cannot have been due to compression, for had it been so the density of those layers would be more than double that of water. But Sir Wyville has found that the specific gravity of the layers 3 inches thick is not much lower than of those a foot in thickness, which proves, as he has pointed out, that compression cannot account for their thinness; but it does not, as we shall presently see, necessarily prove "that a portion of the substance of these lower layers must have been removed."

Assuming that the lower layers were all originally of the same thickness as the upper, it can nevertheless be shown that the gradual diminution in thickness of the layers from the top downwards follows independently altogether of compression or of the removal of any portion of the substance of the layers, either by melting or by any other means.

Ice radiating from a Centre of Dispersion becomes thinner, because the space over which it is spread becomes greater.—There is this peculiarity in Continental ice, that there is a centre of dispersion from which the ice radiates in all directions. This is particularly true in reference to the Antarctic ice-cap. It does not necessarily follow that the centre of

dispersion is the centre of the sheet. In the case of the Antarctic sheet the centre of dispersion cannot, however, be far from the Pole, and the Pole in all probability is not far from the centre of the sheet. We may therefore in our inquiry safely assume the Pole to be the centre of dispersion. It is obvious that, if the Antarctic ice be radiating in all directions from the Pole as a centre, a portion of a layer which in say latitude 85° covers 1 square foot of surface will on reaching latitude 80° cover 2 square feet. At latitude 70 it will occupy 4 square feet, and at latitude 60° the space covered will be 6 square feet. Then if the layer was 1 foot thick at latitude 85°, it would be only 6 inches thick at latitude 80°, 3 inches thick at latitude 70°, and 2 inches at latitude 60°. Had the square foot of ice come from latitude 89° it would occupy 30 square feet by the time it reached latitude 60°, and its thickness would be reduced to 1-30th of a foot, or 2-5ths of an inch.

Now the lower the layer the older it is, and the greater the distance which it has travelled. A layer near the bottom may have been travelling from the Pole for the past 10,000 or 15,000 years, whereas a layer near the top may perhaps not be twenty years old, and may not have travelled the distance of a mile. The ice at the bottom of a berg may have come from near the Pole, whereas the ice at the top may not have travelled 100 yards. It follows therefore that, other things being equal, the lower a layer is the thinner it should be, and that this is perfectly sufficient to account for the decrease in the thickness of the layers from the top downwards, without assuming any of the ice to have been removed by melting or by any other means.

Continental Ice radiating from a Centre of Dispersion must be thickest at the Centre. and gradually diminish in Thickness towards the Circumference.—Whatever theory we may adopt as to the cause of the motion of ice, it will follow as a necessary consequence that the sheet must be thickest at the centre and thinnest at its edge. In a continental sheet like that covering the Antarctic regions we are not warranted, as has already been noticed, in assuming that the surface of the ground under the sheet slopes persistently outwards from the centre or Pole to the edge; in other words, we cannot infer that the Antarctic ice, like an ordinary glacier, rests on an inclined plane.

Now if we adopt the generally accepted theory, that Gravity is the force impelling the ice forward, we must assume the sheet to be thickest at the centre; for unless it

were so gravity could have no tendency to produce motion, because the force which moves the ice must not only act horizontally, but act more in one direction than in another; and this it could not do were the ice of uniform thickness. Were the sheet of this uniform thickness the forces acting on it would balance each other, and no motion could result. If the sheet is to be forced out horizontally along the flat surface by *its own weight*, then there must be a piling up of the ice in the interior. If the ice comes from the centre, then the pressure must be greatest there; but in order to this the sheet, of course, must be thickest at the centre.

Supposing it should be asserted that it is not the pressure of the particle *a* that moves the particle *b* in front of it, and the pressure of the particle *b* that moves the particle *c*, and so on, but that each particle moves by its own weight, we are nevertheless led to the same conclusion. The weight of the particle (the force of gravity) will not move the particle unless the particle is allowed to descend. If a particle moves by its own weight from the centre of the sheet to the circumference, it must *descend*: it must pass from a *higher* to a *lower level*. It must move down an inclined plane from the centre to the circumference, but to allow it to do so the sheet must be thickest at the centre.*

If, on the other hand, we adopt the "Molecular" theory, or the "Dilatation" theory of the motion of the ice, or any other theory whatever which attributes the motion of the ice not to gravity, but to some expansive force acting in the interior of the mass, we are equally led to the same conclusion as to the greater thickness of the sheet at the centre. Although such a force will of course tend to push the ice as powerfully inwards in the direction of the Pole, or centre, as outwards in the direction of the circumference, yet the motion of the ice will always take place in the latter direction, and never in the former, for the latter will always be the direction of least resistance. The tendency of such a force is to produce an outward motion of the ice on the outer side, but to hold back or prevent such a motion taking

* That the entire mass of the Antarctic ice down to the bottom is in a state of motion, and not simply the upper layers, as some suppose, is demonstrable from the fact that icebergs are stratified down to their base. The iceberg is simply a piece broken off the edge of the sheet, and the stratified face of the berg is the counterpart of the edge from which it broke off; and as the icebergs are known to be stratified to their base, it proves that the sheet from which they were derived is likewise stratified to the bottom. The fact, therefore, that stratified icebergs are continually breaking off the Antarctic sheet, and have been for ages, proves that the sheet down to its bottom must have been in a state of outward motion.

place in the ice on the inner or poleward side. As such an expansive force is assumed to act in every portion of the mass, it follows that the nearer the outside of the sheet the more rapidly will the ice move, and consequently the thinner will the sheet become.

The Greater Thickness of the Sheet at the Pole independent of the amount of Snowfall at that place.—It has been proved that unless the Antarctic ice were thickest at the pole and thinnest at the edge, motion could not take place. It follows, therefore, that however much the snowfall at the edge and other places may exceed that at the Pole, or centre of dispersion, the ice must always be thickest at the centre. For however small may be the snowfall, and consequent amount of ice formed annually at the Pole, snow and ice must of necessity continue to accumulate year by year till the sheet becomes thickest there. The ice at the Pole could not move out of its position till this were the case. Supposing there were no snow whatever falling at the Pole, and no ice being formed there, still the sheet would be thicker there than at the edge. For in this case the ice forming at some distance from the pole all around would flow back towards the centre, and continue to accumulate there till the resistance to the inward flow became greater than the resistance to the outward; but this state would not be reached till the ice became thickest on the poleward side.

We have no reason to believe, however, that the quantity of snow falling at the Pole is not great. "One thing we know," says Sir Wyville Thomson, "that the precipitation throughout the Antarctic area is very great, and that it is always in the form of snow." Lieut. Wilkes, of the American Exploring Expedition, estimated the snowfall to be 30 feet per annum, and Sir James Ross says that during a whole month they had only three days free from snow. The very fact that perpetual snow is found at the sea-level at lat. 64° S. proves that the amount of precipitation in the form of snow in those regions must be great.

But there is one circumstance which must tend to make the snowfall near the Pole great, and that is the inflow of moist winds in all directions towards it; and as the area on which these currents deposit their snow becomes less and less as the Pole is reached, this must, to a corresponding extent, increase the quantity of snow falling on a given area. Let us assume, for example, that the clouds in passing from lat. 60° to lat. 80° deposit moisture sufficient to produce, say, 30 feet of snow per annum, and supposing that by the time they reach lat. 80° they are in

possession of only one-tenth part of their original store of moisture,—still, as the area between lat. 80° and the Pole is but one-eighth of that between lat. 60° and 80°, this would notwithstanding give 24 feet as the annual amount of snowfall between lat. 80° and the Pole.

Rate of Motion of the Antarctic Ice.—If we knew the rate at which the edge of the Antarctic ice-cap is advancing outwards, we could form a rough estimate of the amount of snowfall on the continent. Or, conversely, knowing the amount of snowfall, we could tell approximately the rate at which the ice is moving outwards.

Dr. Rink calculates that the yearly precipitation on Greenland in the form of snow and rain amounts to about 12 inches. About 2 inches he considers is carried off by ice into the sea, and the remaining 10 inches is carried to the sea in the form of sub-glacial rivers. He believes that the quantity disposed of by evaporation is trifling.

The amount of precipitation on the Antarctic continent is probably much greater than on Greenland. On the Antarctic continent it is all in the form of snow or hoar-frost, whereas in Greenland a considerable portion of it—in summer at least—is in the form of rain. For reasons already stated the proportionate amount carried off the Antarctic continent in the form of water to that of ice must be much less than on Greenland. The quantity of ice melted in the Antarctic regions from all causes, we have seen, cannot be great; and of that quantity the greater part must be re-solidified long before it can reach the sea. I can hardly think that it will be regarded as an over-estimate to affirm that at least one-half the precipitation must reach the sea in the form of ice. Assuming the annual precipitation to be no greater than that of Greenland, viz., 1 foot per annum, the quantity carried off in the form of ice would in this case be 6 inches. At what rate, then, would the edge of the cap require to be advancing outwards in order to discharge this 6 inches of ice? If we assume the cap to extend on an average down to latitude 70°, its area will be about 5,940,000 square miles, or 165,611,000,000,000 square feet. A layer 6 inches thick covering that area would contain 82,805,500,000,000 cubic feet of ice. The circumference of the cap is 45,300,000 feet, and its thickness at the edge is assumed of course to be 1400 feet. Were the ice, therefore, to move outwards at the rate of 1300 feet per annum, and to break up into bergs as it advanced, the quantity of ice discharged annually in

the form of icebergs would be 82,446,000,000,000 cubic feet, an amount equal to the layer of ice 6 inches in thickness covering the area. Consequently, if 6 inches of ice be carried annually off the Antarctic continent, the edge of the cap must be moving outwards at the rate of about a quarter of a mile annually. Even supposing there were only 2 inches of ice discharged, the rate of motion would require to be between 400 and 500 feet per annum.

A quarter of a mile per annum cannot be regarded as an improbable rate of motion for continental ice, when we reflect that the Greenland ice has in some places a velocity ten times greater. Mr. Amund Helland, for example, found that the glacier of Jakobshaven has a velocity of about 20 metres *per diem*, which is upwards of 4 miles annually. The exceptional high velocity of the Greenland glaciers is no doubt owing to the fact that the ice-sheet covering that continent has to force its way through comparatively narrow outlets. If the sheet moved off the land in one unbroken mass, like the Antarctic sheet, its rate of motion would be much less.

It is the immense extent of the Antarctic continent which demands such a high velocity to get rid of the ice. To enable it to discharge the annual amount of ice, either the sheet must be excessively thick or its rate of motion excessively great. If, for example, the ice were only 700 feet instead of 1400 feet thick, its motion would require to be half a mile annually in order that the 6 inches of ice should be got rid of; while, if it were only 100 feet in thickness the rate of motion would need to be $3\frac{1}{2}$ miles per annum.

It is this difficulty in getting away which is the chief cause of the enormous accumulation of ice on the Antarctic continent. And it is just this great thickness in the interior that enables the sheet to get rid of its superabundant ice. This is effected in two ways:—1st. The greater the thickness of the ice in the interior, the greater is the force by which it is impelled outwards, and, other things being equal, the greater is the velocity of the ice. 2nd. The thicker the sheet becomes, the greater is the quantity discharged corresponding to a given velocity. The velocity being the same, the quantity discharged is in proportion to the thickness of the sheet.

With the present rate of snowfall on the Antarctic continent it is physically impossible that the ice can be otherwise than of great thickness. Were not the sheet enormously thick the quantity of ice annually discharged would not equal that being formed, and consequently the ice would

of necessity increase in thickness year by year, till the rate of discharge became equal to that of growth. We have just seen that it would require a thickness of not less than 1400 feet at the very edge of the cap to make the two rates equal, even although the ice was moving outwards with a velocity of a quarter of a mile per annum, a rate of motion greater than that of an Alpine glacier; and, on the other hand, to produce such a rate of motion as this, a thickness in the interior enormously greater than 1400 feet is required. If, from an increase in the snowfall, or from a decrease in the quantity of snow and ice melted, or from both combined, the annual amount of ice requiring to be discharged were doubled, the velocity remaining the same, the thickness of the sheet would ultimately become doubled also. Or, if the thickness of the sheet remained the same the velocity would be doubled. The actual result in such a case, however, would be that a restoration of equilibrium between supply and discharge would take place, by an increase both of thickness and velocity. As the quarter of a mile per annum of velocity would only be sufficient to discharge one-half the amount of ice being formed, the sheet would increase in thickness year by year. But this increase in thickness would produce an increase of velocity, and the increase both in thickness and velocity of motion would continue till the quantity of ice discharged would be equal to the 12 inches over the whole area, instead of the 6 inches as before. Equilibrium being now established, no further increase would take place either in the thickness of the sheet or in the velocity of its motion. If, on the contrary, the amount of ice being formed on the Antarctic continent were to become less than at present, both the thickness of the sheet and the velocity of its motion would become less.

The following conclusions have now been established :—

1. The Antarctic ice-sheet must be thickest at the centre of dispersion and thinnest at the edge.

2. The rate of motion of the ice must be least at the centre of dispersion and greatest at the edge.

3. The mean thickness of the edge of the sheet, other things being equal, must be proportional to the area of the sheet, and inversely as the rate at which the edge is moving outwards.

4. The area of the sheet, the thickness of its edge, the velocity of its motion outwards, the amount of snow-fall, and the temperature of the climate are so related to one another that the value of any one of them can be determined approximately in terms of the rest.

The probable Thickness of the Ice at the Pole.—The point which now remains to be determined is, What is the thickness of the ice at the Pole, or centre of dispersion? The thickness of the sheet at the edge is admitted to be about 1400 feet, and this, as has been demonstrated, must be the thinnest part of the sheet. It must gradually thicken inwards towards the Pole as centre of dispersion, where the thickness becomes a maximum. How much thicker, then, must the sheet be at the centre than it is at the circumference? The question to be determined, stated in another form, is, What is the thickness of ice at the Pole required in order to impel the cap outwards in all directions at the rate of a quarter of a mile per annum, or even half that rate per annum? The upper surface of the sheet must slope upwards towards the centre or pole. What is the amount of this slope?

The Antarctic continent is generally believed to extend on an average from the South Pole down to about, lat. 70° or so. In round numbers we may take the diameter of the continent at 2800 miles. The distance from the edge of the ice-cap to its centre, the Pole, will therefore be 1400 miles. A slope of 1 degree, continued for 1400 miles, will give 24 miles as the thickness of the ice at the Pole. But would a slope of 1 degree be sufficient to produce the required amount of motion? If the generally accepted theory of the cause of glacier motion be correct, it certainly would not. But supposing we assume that one-half or even one-quarter that amount of slope would suffice, still we have 6 miles as the thickness of the cap at the Pole.

To those who have not been accustomed to reflect on the physical conditions of this problem, this estimate may doubtless be regarded as somewhat extravagant; but a slight consideration will show that it would be even more extravagant to assume that a slope of less than half a degree would be sufficient to produce the necessary outflow of the ice. In estimating the thickness of a sheet of continental ice of one or two thousand miles across, our imagination is apt to deceive us. We can easily form a pretty accurate sensuous representation of the thickness of the sheet, but we can picture to ourselves no adequate representation of its superficial area. We can realise with tolerable accuracy a thick-

ness of a few miles, but we cannot do this in reference to a superficial area 2800 miles across. Consequently, in judging what proportion the thickness of the sheet should bear to the superficial area, we are apt to fall into the error of under-estimating the thickness. We have a striking example of this in regard to the ocean. That which impresses us most forcibly in regard to the ocean is its profound depth. A mean depth of (say) 3 miles produces a striking impression; but if we could represent to the mind the vast area of the ocean as correctly as we can do its depth, *shallowness* rather than *depth* would be the impression produced. A sheet of water 100 yards in diameter, and only 1 inch deep, would not be called a *deep* but a very *shallow* pool or thin layer of water. But such a layer would be a correct representation of the ocean in miniature. Were we, in like manner, to represent to the eye in miniature the Antarctic ice-cap, even as 12 miles in thickness at the Pole, we should call it a *thin crust of ice*. The mean thickness of the sheet would be about 4 miles, and this would be represented by a carpet covering the floor of an ordinary-sized dining-room. Were those who consider the above estimate of the Antarctic ice-cap as extravagantly great called upon to sketch on paper a section of what they should deem a cap of moderate thickness, ninety-nine out of a hundred would draw one of much greater thickness than 12 miles at the centre.

The accompanying diagram represents a section across the cap drawn to a natural scale, the upper surface of the sheet having a slope of half a degree. No one looking at the section would pronounce it to be too thick at the centre unless he were previously made aware that it represented a thickness of 12 miles at that place. The section, of course, is not intended to represent the actual thickness of the sheet, but to show how liable we are to over-estimate a thickness proportionate to an area so immense. It may here be mentioned that had the section been drawn upon a much larger scale—had it, for instance, been made 7 ft. long instead of 7 inches—it would have shown to the eye in a more striking manner the thinness of the cap.

SECTION ACROSS ANTARCTIC ICE-CAP, DRAWN TO A NATURAL SCALE.

Length represented by section = 2800 miles. Thickness of centre (South Pole) = 12 miles.
Slope of upper surface = half-degree.

At the close of the reading of Dr. James Geikie's paper "On the Glacial Phenomena of the Long Island," before the Geological Society, in May last, His Grace the Duke of Argyll stated that he doubted whether ice could move on a slope of 1 in 211. But a slope so small as 1 in 211 would give a thickness of 7 miles at the Pole. Consequently we have no alternative but to admit that a slope of 1 in 211 is sufficient, or the cap must be over 7 miles thick at the Pole.*

But to avoid all objections on the score of over-estimating the thickness of the cap, let us assume that a slope of an eighth of a degree would be sufficient to produce the necessary motion; the thickness of the sheet would of course be one-fourth that represented in the diagram, *but still it would be 3 miles thick at the Pole!*

There is another cause which tends to mislead us in forming an estimate of the actual thickness of the Antarctic ice. It is not in consequence of any *à priori* reason that can be urged against the probability of such a thickness of ice, but rather because it so far transcends our previous experience that we are so reluctant to admit such an estimate. If we never had any experience of ice thicker than what is found in England, we should feel startled on learning for the first time that in the valleys of Switzerland the ice lay from 200 to 300 feet in depth. Again, if we had never heard of glaciers thicker than those of Switzerland, we could hardly credit the statement that in Greenland they are actually from 2000 to 3000 feet thick. We, in this country, have long been familiar with Greenland; but till very lately no one ever entertained the idea that that continent was buried under one continuous mass of ice, with scarcely a mountain top rising above the icy mantle. And had it not been that the geological phenomena of the Glacial epoch have for so many years accustomed our minds to such an extraordinary

* Dr. J. Geikie writes me as follows:—"I have given the height of the glaciation in the North-west Highlands as 3000 feet or thereabout, which, taken in connection with the glacial phenomena of the Outer Hebrides, implies a slope for the surface of the ice-sheet of 1 in 211, or about 25 feet in the mile. It is not improbable, however, that a more detailed examination of the mainlands may compel up to admit a still greater thickness for the ice-sheet of the North-west—the surface of which may have reached to a height of 3500 feet in Rosshire. This would yield a slope of 35 instead of 25 feet in the mile. After my paper had gone to press I received, through the kindness of Mr. George H. Cook, State Geologist of New Jersey, a copy of his Annual Report for 1877, in which the slope of the ice-sheet that flowed into the northern part of that State is estimated at 34 feet in the mile. Prof. Dana, you will remember, comes to the conclusion that the surface of the ice-sheet attained a height upon the Canadian water-shed of 12,000 feet, on the supposition that the ice sloped southwards at the rate of 10 feet in the mile,—if the slope were greater, the Canadian ice, of course, must have been thicker. The inclination of the ice-sheet in the area of the North Sea I estimate at about 12 or 13 feet in the mile."

condition of things, Dr. Rink's description of the Greenland ice would probably have been regarded as the extravagant picture of a wild imagination.

Greenland Ice-sheet.—There is no reason to conclude that the northern hemisphere possesses, or perhaps ever possessed, an ice-cap like the southern. Greenland, however, possesses on a smaller scale a condition of things similar to that of the Antarctic continent. The same general principles which we have been considering in relation to the Antarctic ice-sheet must hold true in regard to the ice of Greenland.

The Greenland ice-sheet, like the Antarctic, must be *thickest at the centre of dispersion and thinnest at the edge.* Greenland is more accessible than the Antarctic continent, yet—with the exception of Dr. Hayes and Prof. Nordenskjöld, and a small party under the command of Lieut. Jensen, R.D.N., recently sent out by the Danish Government—no one, so far as I am aware, has ever succeeded in penetrating to any great distance into the interior over the inland ice. Nevertheless, the state of things as recorded by these explorers affords us a glimpse into the condition of the interior of that continent. They all found by observation, what we have already seen follows as a necessary result from physical considerations, that the upper surface of the ice-plain under which hills and valleys are buried *gradually slopes upwards towards the interior of the continent.*

Professor Nordenskjöld states that at 30 geographical miles from the coast, the extreme point reached by him, he had attained an elevation of 2200 feet, and that the inland ice *continued constantly to rise* towards the interior, so that the horizon towards the east, north, and south was terminated by an ice-border almost as smooth as that of the ocean.*

Dr. Hayes and his party penetrated inwards to the distance of about 70 miles. On the first day they reached the foot of the great *Mer de Glace;* the second day's journey carried them to the upper surface of the ice-sheet. On the third day they travelled 30 miles, and the ascent, which had been about 6°, diminished gradually to about 2°. They advanced on the fourth day about 25 miles, with the temperature 30° below zero. "We had attained," he says, "an altitude of *five thousand feet above the sea-level*, and were 70 miles from the coast, in the midst of a vast frozen Sahara immeasurable to the human eye. There was neither hill, mountain, nor gorge anywhere in view. We had completely sunk the strip of land between the *Mer de Glace* and the sea, and no object met the eye but our feeble tent, which bent to the storm."†

* Geol. Mag., 1872, vol. ix., p. 360.
† Open Polar Sea, p. 134.

Dr. Rink, referring to the inland ice, says that the elevation or height above the sea of this icy plain, at its junction with the outskirts of the country, and where it begins to lower itself through the valleys to the friths, is, in the ramifications of the Bay of Omenak, found to be 2000 feet, from which level *it gradually rises towards the interior.**

Dr. Robert Brown is of opinion that Greenland is not traversed by any range of mountains or high land, but that the entire continent, 1200 miles in length and 400 miles in breadth, is covered with one continuous unbroken field of ice, the upper surface of which, he says, *rises by a gentle slope towards the interior.*†

Lieut. Jensen and his party, after great toil, succeeded in reaching a mountain 50 miles from the border of the ice-fields. The height of the mountain which they ascended was ascertained to be about 5000 feet above-sea-level. From the summit they found the ice-fields beyond stretching as far as the eye could see, the plateau apparently rising *higher and higher towards the interior.*

There is little doubt that an expedition into the interior of Greenland would throw more light on the physical nature of continental ice than one to the North Pole.

The Ice of the Glacial Epoch.—The same general principles which we have been considering hold equally true in reference to the ice of the Glacial epoch. Misapprehensions regarding the magnitude of continental ice lie at the very root of the opposition with which the Land-ice Theory of the chief phenomena of the Glacial Epoch has had to contend. One of the main objections urged against that theory is the magnitude of the ice-sheet which it demands. For example, to explain the glacial phenomena by the theory of land ice, we are compelled to infer that the whole of Scotland, Scandinavia, and the greater part of North-western Europe, were not only covered with ice, but covered to a depth of one or two thousand feet. But not only are the mainlands glaciated, but the islands of the Baltic, the Orkneys, the Shetlands, and the Hebrides, bear equal evidence of ice having passed over them. To explain this by the theory, we have further to assume that the ice-sheet which covered the land must have filled the Baltic, the German Ocean, and the surrounding seas; in short, that all these regions were buried underneath one continuous mass of ice.

To one with inadequate conceptions of the nature of con-

* Journal of the Royal Geographical Society, 1853, vol. xxiii.
† "Physics of Arctic Ice," Quart. Journ. Geol. Soc. for February, 1871.

CHART SHOWING THE PROBABLE PATH OF THE ICE IN NORTH-WESTERN EUROPE DURING THE PERIOD OF MAXIMUM GLACIATION.

The lines also represent the actual direction of the striae on the rocks.

inental ice such a condition of things as this may appear ncredible; but if the principles we have been considering e correct, it follows as a necessary consequence. If, during he Glacial Epoch, the quantity of ice annually formed in North-western Europe was much in excess of the quantity nelted, enormous ice-sheets must of necessity have been ormed.

The thickness of the sheet or sheets covering that region vould depend, as has been shown, upon the area covered and he rate of snowfall, or, rather, the rate at which the ice was eing formed. The sheet, as has also been shown, must ave been thickest at the centre or centres of dispersion—if here were more than one—and thinnest at the edge. The xtent of area covered by ice on North-Western Europe nust have been great; so also must have been the amount f snowfall. If we assume that the Glacial epoch was due o an increase in the eccentricity of the earth's orbit, it ollows that the amount of snowfall on the ice-sheet, during ummer at least, must have been excessive. Owing to the earness of the sun in perihelion at that season, the amount f evaporation in the Atlantic would be greater than at resent. The aqueous vapour thus raised would be carried orth-eastward by the south-west winds, which would then —for reasons which have been stated at length elsewhere—be nuch stronger than they are at present. The vapour on reaching he ice-sheet would be condensed by the cold, and fall as snow. Owing to the rapid accumulation of the ice over so large an rea, and the consequent difficulty in getting rid of it, a conition of things similar to that which now obtains in the ntarctic regions would ere long result.

That such a condition as this, to which we are led by heoretical considerations, did actually prevail during the Glacial epoch is now established by the facts of observation. Norway we know was the great centre of dispersion of the ce, and here it has been found that the sheet attained its reatest thickness. It has been shown by Mr. Amund Helland that its thickness there was over a mile. Scotland vas also a subordinate centre of dispersion, and we know hat the ice moving off it was sufficient to prevent that ountry from being overridden by the great mass of ice owing outwards in all directions from the Scandinavian entre. It was sufficient, but little more, for the Scandinaian ice filling the German Ocean, and passing over the Orkney and Shetland Islands, was so powerful as to bend ack the Scottish ice, and force it to turn round, after it had

entered the German Ocean, and pass obliquely over the flat lands of Caithness.*

The accompanying Chart, reproduced from "Climate and Time," shows the probable path taken by the ice in its outward motion from these two centres of depression. The Chart, it is to be observed, is not theoretical, for the lines represent the actual direction of the striæ on the rocks. It was inadequate conceptions of the nature of continental ice which prevented geologists from coming to the conclusion that all those striæ were produced by land ice.

II. GRAVITATION AS A FACTOR IN THE ORGANIC WORLD.

By WILLIAM CROOKES, F.R.S.

IT cannot be denied that in discussing the great questions of the origin of life and the development of species,—in seeking to explain to ourselves why every creature is as we find it, and not otherwise,—we have to deal with two sets of factors.

On the one hand we have certain outward agencies, such as the great cosmic forces, the chemical and physical character of the earth's surface and of its liquid and gaseous envelopes, the varying effects of climate, the quality and quantity of food accessible, the conflict with other species,—everything, in short, which the animal encounters either as an enemy or as an ally in its struggle for existence.

On the other hand, there are doubtless inward tendencies upon which these external forces react. For if we see a roll of calico passing through a dye-bath of alizarin, and coming out not all of one uniform shade, but displaying a particoloured pattern, we know that different mordants must have been imprinted upon it with each of which the dye gives a different result. Just so here; were the outer factors alone present, no variety could result from their action. But of

* An important paper by my colleagues, Mr. B. N. Peach and Mr. J. Horne, on the Glacial Phenomena of the Shetland Islands, will shortly be read before the Geological Society of London. They have found evidence which puts the question beyond all doubt that the whole of those islands have been glaciated by land-ice from Scandinavia.

ese inward factors we know as yet nothing. Nor can e even hope to begin their study till we have first traced it and eliminated the workings of the external agencies hich are simpler in themselves, and whose general aracter we have had the opportunity of learning from the enomena of inorganic nature. When this task is once complished we shall then find certain "residual pheno-ena" not to be accounted for by the outward factors above entioned, and from these phenomena we may hope to gue back to their unknown and doubtless exceedingly com-icated causes.

But very much still remains to be done before we can cide with certainty what part is played by such external rces.

The functions of electricity have indeed not been dis-garded. In semi-scientific circles it fills the post which pular tradition once allotted alternately to Michael Scott d to the devil—doing duty, namely, as the originator everything unaccountable. The influence of light d of heat upon organic beings has been investigated th much care, and though the task is far from com-ete, yet valuable results have been reached. Above all has been established that certain limits of temperature ist, above and below which all our experience declares life be impossible. All organisms, as we find them upon our obe, consist of a combination of solids and liquids. But low a certain degree of heat the liquid constituents become lidified, and thus are rendered incapable of playing their rt in the vital process. If such solidification is temporary may involve merely a suspension of animation. Above a rtain temperature both liquids and solids would be either latilised or resolved into their inorganic constituents; nce it follows that the chemical composition of every ganism must stand in a certain relation to the thermic nditions of the world it inhabits. Very similar is the case regards light, its total absence and its excess being alike favourable to the welfare of plants and animals.

But the effects of gravitation in its several manifesta-ons seem scarcely to have received the attention which ey merit. Yet that this all-embracing force must have t its impress upon the organic world, no less than upon eless matter, can scarcely require any formal demon-ration.

For what is an animal? If we take as the type the ghest organism known to us, the reply must be—a thinking

brain! But this brain, in order to communicate with the external world, requires organs of sensation, organs, too, by which it can react upon surrounding matter, and others by which it can remove itself from place to place. These processes, however, require again the expenditure of energy, which the brain has not the power to create out of nothing. Hence the necessity for what are sometimes known as the vegetative systems of man and animals,—the organs of digestion, assimilation, circulation, respiration, secretion, and excretion,—whose duty is to provide for the waste of tissue of the brain and its more immediate accessories, to place at their disposal a constant store of available energy, and to provide for the removal from the totality thus constructed of all matter which has become effete. Such a complex being obviously cannot be flung at random into space. It demands a world, without which there can be neither scope for its activity nor even the possibility of its existence. With this world it must be in harmony, and thus the animal becomes subject to the dominion of gravitation, with which its organic forces act now in conjunction and now in opposition. Here indeed, just as in the case of heat, we find that there are certain limits within which only it is possible for organic life as we know it to be maintained. Were the earth's attractive force as exerted upon bodies placed on its surface, either greater or less, we should all,—the "we" including every animal and vegetable organism as well as man,—in virtue of that difference alone, be something other than what we now are.

A couple of ideal experiments, the results of which no one can doubt, will make this clear, and will at the same time explain the part played by gravitation in the animal economy. Let us suppose this force doubled. All terrestrial animals would then be compelled to exert a vastly increased amount of strength in order to support themselves in any other than a recumbent position—to rise up from the ground, to run, leap, climb, or to drag or carry any object. In order to do all this several important changes in the animal structure would be necessary. Their muscles must necessarily be more powerful, and the skeleton to which they are attached would need a corresponding modification. To work such limbs a more rapid transformation of matter would be required; hence the supply of nutriment must be greater, involving enlarged digestive organs, and doubtless a larger respiratory apparatus, to allow of the perfect aëration of the increased mass of the blood. In order to keep up the circulation with the necessary force, either the heart would require to be more powerful

or the distance through which the blood would require to be impelled must be reduced, and the vessels widened so that there might be less loss from friction. The increased amount of nutriment required would involve a corresponding increase in the difficulty of its collection, and the struggle for existence would therefore probably be intensified.

These considerations involve a very marked change in the structure of land animals. Their general form, in accordance with their thickened bones, increased muscles, and larger respiratory and digestive apparatus, would be heavier and more massive than what we now observe. The necessity for such a structure would be increased by another circumstance, viz., that the probability of a fall, and the danger, if it should happen, would both be increased. Hence the bipedal form—whether among anthropoids, birds, or hopping lizards—would be attended with much greater drawbacks than is the case with the actual amount of gravitative force. Even quadrupeds would be unsafe in their movements, and it is highly probable that a hexapod, octopod, or decapod structure would be found extending much higher in the animal kingdom than it does in our world. Or if there is some reason why the number of limbs in a vertebrate animal cannot exceed four, we should find the majority of animals furnished with short legs, like those of Saurians, which raise the trunk of the body but little, and allow it to rest easily upon the surface of the ground. The type which we see in serpents would probably be much more common, and would be developed into a great variety of forms. The necessity of keeping the centre of gravity low, and the great demands made upon the system in other respects, would conspire to reduce the size of the head and the brain.

Of all animals those which pass a large proportion of their existence in the air, supported upon wings, would meet with the greatest difficulties. The earth would draw them down with a force hard to resist, and which could only be overcome by the expenditure of a large amount of muscular force, and consequently of the transformation of much matter. Hence with gravitation intensified, as we are supposing, such forms of life as the humming-birds, the swifts, the swallows, &c., could scarcely exist. In their stead would flourish groups approaching to the Gallinaceæ and the Struthionidæ, along with the true woodland birds, whose flight is little more than an extended leap from branch to branch.

Winged insects would fare very similarly to birds. The

difficulty of flight being greatly increased, we might look for a corresponding diminution in the number of aërial species, and an alteration in the habits of such as do exist. The dragonfly, which in order to find a sufficiency of food hawks about through the whole of the day; the tiger-beetle, who seizes his victims by a series of bounds, and short though rapid flights; the butterflies and bees, who have to visit thousands of flowers in order to obtain a sufficiency of honey or of pollen—must all find the struggle for existence rendered more arduous, and would doubtless be less numerous both in species and individuals. Hence the fertilisation of flowers by the intervention of insects must be impeded, and anemophilous species would receive an advantage which they do not now possess, and which would lead to the extinction—or at least to the great reduction—of entomophilous plants, *i.e.*, all those with the showiest blossoms: a somewhat unexpected result to follow from a mere increase of the earth's attraction! Creeping insects would merely find their position deteriorated in the same manner as would land animals. Hence they would have a relative advantage over their winged brethren, and might probably become in consequence more abundant. Such articulate forms as the centipedes, the scorpions, and the ground-spiders—having, by reason of their numerous legs, many points of support—would fare the best, though even such creatures would find it more difficult to run, and much more so to climb and leap, than they do at present. The ordinary cobweb-making spiders would have to make their nets of an increased strength, in order to support their own weight and that of their prey. The true gossamer-spiders, which now are able to mount up in the air to a considerable height, would find their flights at an end.

The terrestrial Mollusca—snails, slugs, and the like—would be little incommoded, so long at least as their journeys were confined to the level ground. Hence they would undoubtedly play a more important part.

Such, then, are the changes which would ensue, for instance, in our own globe, from a mere increase of its attractive force. Numbers of animal and vegetable species would find the balance so far turned against them that they would more or less rapidly succumb. Other species would find increased scope and opening, and, if the Evolutionist view be correct, would doubtless branch out into a multitude of new forms. But all, to survive, would have to be structurally modified to a greater or less degree in the manner I have already pointed out.

Did a man weigh three times as much as he now does—as would be the case were he placed on the surface of Jupiter—the conditions of organic life must be still further altered, and the resulting forms, other conditions remaining equal, would differ still more widely from what we see around us.

Decreased attraction of the earth, or a reduction in the actual weight of plants and animals, would be attended with another set of changes scarcely less remarkable. With the same expenditure of vital energy as at present, and with the same quantity of transformation of matter, we might be able to lift larger bulks, to take longer bounds, to move with greater swiftness, and to undergo prolonged muscular exertion with less fatigue. Hence the transformation of matter required to keep up animal heat, and to restore the waste of tissue, would be smaller for the same amount of duty done. A less volume of blood, smaller lungs and digestive organs would be required. The labour of the heart, in raising the blood to the head and parts of the body above its own level, would be reduced. Hence we might expect a set of structural changes in terrestrial animals of an inverse nature to those we have seen resulting from intensified gravitation. All parts of the body might safely be constructed upon a less massive plan. A slighter skeleton, smaller muscles, and a slenderer trunk would suffice. These modifications would be the more essential from another consideration. At present the cases in which men or other terrestrial animals, are swept away by the wind are exceptional, and occur only in furious cyclones. Were our weight, however, reduced to one-fourth of its present magnitude, whilst our bulk remained unchanged, we should not seldom run the risk of being carried off our feet, with the possibility of unpleasant consequences. Hence one of the characteristics of the "fittest" for survival in such a state of things would be either a minimisation of the surface exposed to the wind, or the utilisation of atmospheric currents as a means of locomotion. On the water this is already done by the paper nautilus, which drives before the wind, using its arms as a kind of sails. On land we may see that most noxious Dipteron the "Harry long-legs" (*Tipula oleracea*) travelling in a brisk breeze, with a kind of motion that is neither running nor flying, but a curious mixture of both. Head-first or tail-first, rolling over and over, it makes a very rapid progress, as we find if we attempt to "stamp it out," though all the while its movements appear exceedingly clumsy. Were the force of gravitation reduced in the manner we are

contemplating, locomotion of this character would probably become common among the higher animals. The separation between the aërial and the terrestrial forms of life would be less marked than we now find. A true aërial life, moreover, would be much easier than at present, as the winged animal, of what kind soever, would require to exert much less force than at present in order to overcome the attraction of the earth. Hence the number, the variety, and probably the size of winged species would doubtless be much increased.

We may safely assume that could gravitation be sufficiently intensified we should ultimately reach a maximum limit beyond which life could not exist. A point is easily conceivable where no force generated within the body of an animal—supposing one placed in such circumstances—would counterbalance the force of which it was, so to speak, *cemented* to the earth's surface. All motion from place to place would then be absolutely impossible, and every movement within the animal system, save in a line tending to the earth's centre, would be impossible likewise. The smallest and simplest animals would doubtless bear a greater gravitative force than the higher and larger. It is evident that they have, even now, a certain advantage in this respect. Though under the influence of gravitation, as decidedly as are larger species, we see them—even when not specially organised for such feats—ascend smooth perpendicular surfaces, and even travel along the lower side of a horizontal plane with their heads downwards. Naturalists have spent much time in explaining this faculty in the common house-fly, which, however, is only one out of thousands of species able to perform the same action.

A fall, even from a height very great in proportion to their stature, seems to have no injurious effect upon wingless insects. This may at first sight seem due to the coat of armour, or so-called exo-skeleton, in which they are enclosed. But if a tortoise is dropped from a corresponding height its armour does not save it from damage. The reason must, then, be sought in the resistance offered by the cohesive power of the atmosphere to the fall of so light a substance.

Even a relatively moderate increase in the force of gravitation might be productive of serious consequences to larger animals. In man, the downward pressure of the blood resulting from gravitation sometimes distends the veins of the feet and legs so as to produce a varicose condition. Were the human stature or the earth's attraction doubled, and still more if both these modifications took place con-

jointly, this disease would be induced earlier in life, and would occur not merely in the lower extremities, but in the regions of the trunk below the level of the heart. The increased action of the latter organ, further, could scarcely fail to bring on aneurism.

If we then admit that gravitation tends to limit the size of organic beings, presenting difficulties which grow with their growth, we shall be in a position to explain certain general phenomena of the animal and vegetable kingdoms which have hitherto been for the most part accepted as ultimate facts. It is a familiar observation that every substance, living or lifeless, weighs less the denser the medium in which it is plunged, or, more accurately speaking, the nearer the specific gravity of such medium approaches its own. If we raise a stone which is lying under water, we find its weight increase suddenly the moment we have lifted it above the surface. This simple experiment may show us that a creature inhabiting the land, and having its body ordinarily immersed in air, experiences greater difficulty in supporting its own weight than would a being of equal size and formed of similar materials, but framed to live in the water. The former has to spend a greater proportion of its energies in fighting against gravitation than the latter. Again, a creature which passes a great portion of its time not merely immersed in air, but suspended in it, has a still harder struggle against gravitation than either of the two former, and will find its vital force still more severely taxed. Hence we might expect to find the inhabitants of the air, in the strictest sense of the term (*i.e.*, *Cheiroptera*, birds, and winged insects), small; the inhabitants of the water largest; whilst the dwellers on land—immersed, indeed, in air, but supported to a great extent by the firm ground—should occupy an intermediate position.

This agrees substantially with what we find on an actual examination of the animal kingdom. If we take the two leading sub-kingdoms, the Annulosa and the Vertebrata, each of which includes a vast number of aquatic, of terrestrial, and of aërial species, we find that in every case their largest-sized members are inhabitants of the waters. The first of these sub-kingdoms includes the insects, the myriapods, the spiders and their allies, and the crustaceans. But everyone knows that certain of the latter group, such as the crabs and lobsters, far surpass the remaining classes in magnitude. Who, for instance, ever found a beetle, a cockroach, locust, scorpion, centipede, or spider weighing 5 lbs.? Yet a crustacean of 15 to 20 lbs. is by no means unexampled.

Turning to the sub-kingdom of the Vertebrates, popularly so much better known, we find in its highest class, the Mammalia, the largest of all known animals, the whales, all strictly aquatic. Several of the seals—such as the bottle-nose, which reaches a length of 25 feet—and the walrus also far exceed the average stature of land-animals. Nay, amongst those ordinarily classed as true terrestrial forms, many of the largest—such as the hippopotamus, the elephants, the various species of rhinoceros, even the true buffalo (*Bubalus bubalis*) are marsh-dwellers, semi-aquatic in their haunts and habits. The largest of the cats, the Bengal tiger, is also the most natatorial.

Among the reptiles we find a state of things closely analogous. Of the Saurians at present living no terrestrial species can at all compare with the crocodiles. Among the gigantic extinct groups the majority at least—such as the Ichthyosauri, the Plesiosauri, and the Pythonomorphæ (if the latter are not rather Ophidian)—must have been aquatic.

If we examine the serpents we find, again, that the largest species are aquatic,—as, for instance, *Eunectes murinus*,—inhabiting the rivers of South America.

Among the Chelonians the same rule prevails, the turtles being certainly much larger than their terrestrial kindred, the tortoises, even those of the Gallapagos and the Mascarenes.

Of the Mollusca only a minority are terrestrial. But here also we find the smaller species inhabiting the land, and the larger forms, especially the gigantic cuttlefishes, confined to the waters.*

I may here remark that Swainson, though setting out from totally different considerations, and though accepting each animal species as an independent ultimate fact, lays a decided emphasis on the magnitude of "natatorial or aquatic types," which he pronounces "as to structure, chiefly remarkable for their enormous bulk."†

In opposition to the vast growth of the aquatic groups, we find the inhabitants of the air for the most part diminutive. The bulkiest birds are puny in comparison with the bulkier members of the other vertebrate sections. Further, even among birds, the largest species—such as the ostrich, emeu, and cassowary—are strictly terrestrial in their habits, as was also the gigantic extinct moa (*Dinornis*) of New Zealand, and in all probability the still larger *Æpiornis* of

* Cephalapods are known to reach the weight of 2 tons.

† Geography and Classification of Animals, p. 249, § 309.

Madagascar. By way of contrast we may point to the humming-birds, perhaps the most truly aërial of vertebrates, some of which do not exceed 20 grains in weight, and which, except when sleeping or engaged in the act of incubation, are ever on the wing.

The largest and heaviest insects,—such as *Goliathus Drurei, Dynastes elephas*, &c.,—though possessing organs of flight, use them rarely and clumsily, and may in this respect be instinctively contrasted with many species of tiny Diptera and Hymenoptera which spend nearly the whole of their adult stage in the air.

An examination of the respective sizes of the various animal groups fully supports, therefore, the assumption with which I started. There are, indeed, multitudes of tiny and even microscopic creatures inhabiting both the salt and the fresh waters. But the largest species, whether still living or extinct, are strictly aquatic.

The vegetable kingdom in its turn presents us with very similar phenomena. The dry land, indeed, produces mighty plants like the Sequoia of California, the Boabab of Senegal, and the Jarra-jarra and the gum-trees of Australia. But according to Prof. Reinsch, the *Macrocystis pyrifera*, a sea-plant found in the North Pacific, surpasses them all. A single specimen of this plant has been found, by actual measurement, to cover three square miles.

The size of extinct organisms, as compared with that of species still surviving, has often attracted attention. Here there certainly exists, in the popular views, a very considerable amount of exaggeration. Ever since it was placed beyond doubt that fossils were truly the remains of plants and animals which once inhabited this earth of ours, and not mere *lusus naturæ*, or superhuman forgeries—we can find no better name—created specially for man's delusion, it has been customary to invest the faunæ of bygone geological epochs with a magnitude most portentous. But, as is the case with recent forms of life, this gigantic character shrinks when the test of careful measurement is applied. We may safely say that no fossil serpent, saurian, fish, or whale of one hundred yards in length has yet been discovered; that no strata have yet disclosed the remains of a terrestrial carnivore of double the size of the royal tiger, or of a proboscidean, ruminant, edentate, or marsupial of more than thirty feet from head to tail. It has even been questioned whether the mean stature of the animal kingdom has greatly decreased. Still we have good evidence for concluding that in almost every group species have once existed

very considerably surpassing in size their nearest representatives still living, and that those orders which now comprise our bulkiest animals, such as the proboscidea and ungulata, were at one time far richer in species than at the present day.

The relative magnitudes of the various animal groups do not seem to have been greatly different from what we now find them. Early geologic epochs certainly display birds larger than any now living. But the largest of these birds did not equal, much less exceed in stature, the large mammalian forms of those days. The *Harpagornis* was smaller than the *Machairodon*, just as the harpy eagle of our time is smaller than the tiger or the jaguar. The mammoth and the mastodon exceeded in bulk the *Dinornis* and *Æpiornis*, just as the elephant surpasses the ostrich. The rocks have yielded up remains of insects larger, perhaps, than insects of the same, or of closely approximating orders existing in the present day. But we find no proof that the *Annulosa*, as a class, made any nearer approach to the average growth of the *Vertebrata* than what we observe in living species. This fact, we submit, powerfully supports the view that the relative as well as the positive sizes of organic beings are not solely dependent upon such circumstances as the greater or less abundance of food, the competition of other species, &c., but are governed by some deeper law. Evolution has no *tabula rasa* upon which to work. We do not, of course, ignore or question the fact that the respective parts played by certain of the leading groups of the animal kingdom have not always been the same; that there has, for instance, been an epoch in the earth's existence which might be pre-eminently considered as the age of reptiles, when the development of the mammalia was only in its infancy.

That in so many cases the largest, strongest, and best-armed species should disappear, whilst the smaller and the weaker have been perpetuated, is a curious case of the survival of the fittest. This superiority demands an explanation. If, as seems not improbable, the density of the atmosphere was greater in the early geological epochs than is now the case, we have a clue to the more luxuriant growth of the terrestrial and aërial species of those days. They would then be placed in conditions somewhat approaching those of the aquatic animals, and might accordingly attain a larger size.

It appears, therefore, that although gravitation sets cer-

tain boundaries to the size of organisms which they are unable to exceed, yet these boundaries vary with the density of the medium in which such organisms are immersed.

A denser atmosphere, just like a reduced gravitative force, would tend to obliterate the sharp boundary line now existing between terrestrial and aërial animals, and would doubtless re-people the world with winged saurians, gigantic birds, and with flying mammals.

The effects of a highly rarefied atmosphere may easily be realised from what is witnessed upon lofty mountains. Great limitation among aërial species, and an equally great debilitation in terrestrial forms, could not fail to result.

There has been no little speculation as to how the world must appear to beings whose whole life-time might be counted by minutes rather than by years. But no one seems to have entertained the idea that our perceptions of external nature and our modes of interpreting phenomena are to a very great extent the result of our size, and would change if it were modified. Such a contemplation may teach us some useful lessons.

Let us in the first place suppose ourselves reduced to the size of the minutest living being, though preserving the same mental faculties, and, in proportion to our bulk, the same physical powers which we now enjoy. In what light would the most ordinary phenomena of outward nature present themselves? We should find an alteration, not merely in magnitude, but in kind. As an example, I set out for a journey of exploration upon a cabbage leaf, which appears as a plain of many square miles in extent. I find it studded with huge, glittering, transparent globes, each in height vastly exceeding my own stature, and resting motionless upon the surface of the leaf. Each of these appears to pour out, from one of its sides, a dazzling light, accompanied by a strong heat. Urged by curiosity, I approach and touch one of these orbs, when suddenly I feel myself seized upon and whirled round and round till I come somewhere to an equilibrium and remain suspended on its surface, utterly unable to extricate myself. In the course of an hour or two I find the globe diminishing, and ultimately it disappears, leaving me at liberty to pursue my travels. Leaving the cabbage leaf, I stray along over the surface of the soil, which has an exceedingly rocky and rugged character, until I see before me a broad surface of the same kind of matter which formed the globes on the cabbage leaf. Instead, however, of rising upwards from its support, I see it now sloping downwards in a vast curve from the brink, and

ultimately becoming apparently level, though as this is at a considerable distance from the shore, I cannot be absolutely certain. Let us now suppose that I hold in my hand a vessel bearing the same proportion to my minimised frame that a pint measure does to that of man as really existing, and that by adroit manipulation I contrive to fill it with water. If I invert the vessel, I find that the liquid will not flow out, and can only be dislodged by violent shocks, or by the removal of the atmospheric pressure from a part of its surface. After these and a few more observations and experiments easily conceivable, I sit down to theorise on the properties of water and of liquids in general. Shall I come to the conclusion that liquids seek their own level; that their surfaces when at rest are horizontal, and that solids placed in a liquid sink or float according to their higher or lower specific gravity? No; I shall think myself justified in inferring that liquids, when at rest, tend to assume spherical, or at least curvilinear forms, whether convex or concave, depending upon circumstances not easily ascertained; that they cannot be poured from one vessel into another, but, unlike solids, resist the law of gravitation, which is consequently not universal, and that such bodies as I can manipulate refuse to sink into liquids, whether their specific gravity be high or low. From the behaviour of a substance placed in contact with the dewdrop I shall even derive plausible reasons for doubting the inertia of matter. All these changes in my interpretation of phenomena arise not from my becoming aware of any forces hitherto overlooked, still less from the disappearance of laws now recognised, but simply from the fact that my supposed decrease in bodily size brings capillarity and cohesion into a relative prominence which they do not now possess. To rational beings of the actual size of man the effects of capillary attraction and of the cohesion of liquids rank among the residual phenomena which only attract attention when science has made some progress. To *homunculi* such as we have been supposing the same effects would be of capital importance, and would be interpreted not as something supplementary to those of general gravitation, but as due to an independent and possibly antagonistic force.

Their vision might possibly surpass, in its power of dealing with minute objects, even our most exquisitely finished microscopes. But this faculty, however invaluable to beings who could use it only when and where they might think desirable, would be rather bewildering than enlightening were it the only mode of vision. To see objects at will with

say a power of one-twentieth inch is exceedingly useful; to see them with such a power only would show merely details which the mind could scarcely combine into an accordant whole.

In the study of heat they would encounter difficulties probably insuperable. In this branch of physical investigation little can be done unless we have the power of raising and lowering the temperature of bodies at pleasure. This requires the command of fire. Actual man, even in a very rudimentary state of civilisation, can heat and ignite certain kinds of matter by friction, percussion, concentrating the sun's rays, &c. But before these operations can produce actual fire they must be performed upon a considerable mass of matter, as otherwise the elevated temperature is conducted or radiated away as rapidly as produced, and the point of ignition is never reached. The physics of such *homunculi* would therefore differ most remarkably from our own.

Nor could it be otherwise with their chemistry, if indeed such a science can be conceived as possible for them at all. It can scarcely be denied that the fundamental phenomena which first led mankind into chemical inquiries are those of combustion. But, as we have just seen, such beings would be unable to produce fire at will, and would have little opportunity for examining into its nature. They might occasionally witness forest-fires, volcanic eruptions, &c. But such grand and catastrophic phenomena, though serving to reveal to our supposed tiny men the existence of combustion, would be ill-suited for quiet investigation into its conditions and products.

Let us pass now from Lilliput to Brobdignag, and consider how nature would appear to rational beings of enormous magnitude. Their difficulties and misconstructions would be of an opposite nature from those committed by the pygmies above imagined. Capillary attraction and the cohesion of liquids, instead of being over-estimated, would be doubtless entirely ignored. The dew-drop and the curvature of collections of liquid, where bounded by some solid body, would be altogether invisible. The behaviour of minute bodies thrown upon a globule of water would escape notice. The *homucunlus* able to communicate but a very small momentum will find all objects much harder than they appear to us, whilst to the colossal beings we are now supposing granite rocks would be a very feeble impediment.

But the most remarkable difference between such enor-

mous beings and ourselves lies here: if we stoop down and take up a pinch of earth between the fingers and thumb, moving these members say through the space of four inches in a second of time, we experience nothing remarkable. The earth offers a little resistance, more or less, according to its greater or less tenacity, but no other perceptible reaction follows.

Now let us suppose the same action performed by a gigantic being, able to move his finger and thumb through four miles of soil in the same lapse of time. He would experience a very decided reaction. The mass of sand, earth, stones, or the like, hurled together in such quantities and at such a speed, would become intensely hot, probably reaching the point of ignition. If his size were still greater, the experiment would be attended with still more striking results. If we can suppose a being vast enough to grasp the earth between his two hands and at once arrest its motion, the consequence would be explosive ignition capable of resolving at once the planet, and the being who thus checked its career into a gaseous condition. Just as the *homunculus* must fail to bring about ignition when he desired it, so the colossus could scarcely move without causing the liberation of a very inconvenient degree of heat, and literally making everything too hot to hold. He would naturally ascribe to granite rocks and the other constituents of the earth's surface such properties as we attribute to phosphorus—of ignition on being a little roughly handled.

Need we do more than very briefly point to the obvious moral? If mere differences of size can cause some of the most simple facts in chemistry and physics to take so widely different a guise; if beings infinitesimally small and immensely large would simply as such be subject to the delusions we have pointed out, and to others that might be detected, is it not possible that we, in turn, though occupying, as it seems to us, the golden mean, may also in the mere virtue of our size fall into misrepresentations of phenomena from which we should escape were we either larger or smaller? May not our knowledge have in it in this respect an element of subjectivity hitherto unsuspected, and we fear scarcely possible to eliminate?

III. SANITARY SCIENCE IN THE UNITED STATES: ITS PRESENT AND ITS FUTURE.

By Albert R. Leeds, Ph.D.

It is generally conceded, I believe, by scientific labourers in this country that we have been more fertile in invention than discovery. We owe to older nations a constantly increasing debt of obligation for those initial germs of thought which have fructified into new sciences, while we may, at the same time, ask a generous acknowledgment of the merits of many inventions which have opened up new fields of employment to thousands. Sciences which promise much for the improvement of the daily condition of mankind, and have in them a side largely practical, are sure of welcome in our midst. Such a science is pre-eminently the one under consideration. It gathers into one the teachings of all other sciences, so far as they bear upon private and public health, and makes these teachings practically operative in the promotion of human welfare in this country. It grew into prominence during the war of the Rebellion, when the work of the Sanitary Commission was made co-extensive with every army camp and army hospital. Its principles have been expounded in sanitary associations formed in many States and in smaller communities. These have led to the formation of State and city boards of health, clothed to a greater or less degree with executive functions.

Every epidemic has fastened popular attention upon the subject, and before it was taught in book or lecture room, has been rehearsed in a thousand forms in the newspaper. In this present yellow fever plague more than twelve thousand people have perished, probably not less than sixty thousand have convalesced, and two hundred millions of dollars would not represent the aggregate pecuniary loss. During its awful course universal interest has been felt in the cause and prevention of this and similar diseases, a homily on private or public hygiene has formed a prominent feature of the daily paper, and this interest has culminated in the offer made by a lady, already widely known by her munificence in the cause of science, to defray the expenses of a commission of enquiry composed of sanitary experts. We believe that this is all as it should be, and that in sanitary science this country is taking a foremost place, because

popular sympathy and popular knowledge are running almost abreast of the science itself. The proper execution of sanitary laws demands the free and intelligent co-operation of the individuals; a system of enactments, however skilfully framed and however supported by a strong central authority, alone will not suffice. Not only would it appear alien to the genius of our institutions, but also a mode ill-suited to attain its object, if a Health Department were added to the other departments of State at Washington. No one would deem it possible for such a department to legislate pure air, pure water, and pure food into use throughout the nation. On the contrary, it would appear far wiser to leave such legislation to each State, and in the State as far as possible to each community, recognising that the popular agitation and knowledge requisite to obtain health laws is the best guarantee that they shall not afterwards remain dead letters. What I have to say concerning the present and future of sanitary science is mostly the record of what such communities have already done, and of what ideas are already growing up in their midst, and which ere long will bear fruition in new laws and movements.

I. *Vital Statistics.*

We shall begin at the foundation-stones of exact sanitary science—vital statistics. To appreciate the advance in this direction we have only to compare the condition of our great city, New York, at the beginning of this century with its present. At that time it had no registration even of deaths. The first "Bill of Mortality," as it was called, extended from November 1, 1801, to January 1, 1803—fourteen months. So little accuracy in the nomenclature of diseases was thought of or expected, that in this report the people are said to have died of "flux," "hives," "putrid fever," "rash," "lingering illness" (which certainly was not a rash performance), "stoppage," "breaking out," "fits," &c. The first reliable report was that made in the year 1866, after the organisation of the Metropolitan Board of Health. In the second annual report in 1867, the beneficent results of the institution of the Health Board, and of the sanitary reforms executed under its superintendence, were shown by the fact that there were 3152 less lives lost during the first year of its administration as compared with the preceding year. The report, moreover, showed that this gain had been mainly in the chances of life at the adult ages, and in the districts where the greatest amount of sanitary work

had been performed. The causes of insalubrity affecting infant mortality were not yet within control. In the year 1868 the work of registration was extended and specialised in such a way that comparisons could be made in the death-rate between portions of the city occupied by a degraded and overcrowded population and those more favourably situated, whether in point of natural advantages or in the character of inhabitants. This specialisation enabled the sanitary inspectors to judge of the value of their labours in the matter of cleansing and disinfection. In fact they had lowered the death-rate in certain of the most wretched wards below that in some of the best, sanitation in these latter having been omitted. The registration extended also to the effect of modes of living upon the death-rate, and in this manner pointed out the necessity of controlling the excessive mortality in tenement houses. That health reports, when promptly and intelligently used, might be effectively employed in the prevention of disease, was shown by the returns made during the last two weeks in July, 1878. The Registrar, apprehending that the infective quality of Asiatic cholera might prove to be present in the rapidly fatal diarrhœas then prevalent, sent warning to the Surgeon-General of the United States army, in consequence of which the General in charge of the recruiting and transportation of troops ordered the immediate suspension of that branch of the army service in New York. Valuable illustrations of the relation existing between damp houses and pulmonary consumption were obtained by selecting certain wards of the city and forming maps in which every death from phthisis for a number of years was marked on the chart opposite the locality of its occurrence. The evidence so obtained pointed to an excess of consumption at the lowest levels, and in two of the wards to a crowding of fatal cases of this disease in localities unusually damp and in rainy seasons flooded, although these dwellings differed in no other respect from the average of the ward. The results obtained in this manner were deemed so valuable that the registration was applied to each house in the city. In this way excessive mortality in any locality or from any special class of diseases became known at once to the sanitary inspectors.

With regard to the registration of marriages, improvement was more difficult. The system of registration, expecting a voluntary support from clergymen and civic officers concerned, could secure very partial returns, and it was only by diffusing information through the press and the lavish distribution of circulars that the accuracy and com-

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pleteness of the returns in this respect could be improved. An enquiry into the number of births registered, as compared with that which the Board had reason to believe occurred, revealed a deficiency in the registry of 65 per cent. It has been stated by an American writer on these topics that "it would be impossible for a large portion of the adult men and women born in the United States to prove by any public records or other legal documents that they were legitimate offspring, with a natural right to the name they bear, or even that their parents were ever married."

The system of mortality registration was gradually improved until the returns made in the year 1871 were probably nearly perfect. When compared with the mortality in other cities, this accuracy, however, told against New York, for while its death-rate was 28·6 per thousand, that of St. Louis in the same year was reported at 17 in a thousand; of Rochester, 16; Buffalo, 14; and Jersey City, only 7. In matter of marriages and deaths, the increased knowledge among clergymen, physicians, and others on whose voluntary co-operation the registration largely depended, had resulted in an apparent increase in the annual marriage and birth-rate, but still the number of births returned was probably less by 10,000 than the true. In the following year the Board instituted suits against these parties, which had a beneficial effect, but it became evident that nothing short of important changes in the law would secure completeness.

I have been thus particular in narrating the history of vital registration in New York because this city was the first to undertake a reform, and because its reports were the first which attempted to keep abreast with the development of sanitary science and to diffuse this knowledge broadcast. The course of legislation on these points is one which every city and State has, or is going through. In reference to New Jersey, the facts are so fresh that I scarcely need recall them. At each meeting of the New Jersey State Sanitary Association since its origin, three years ago, the inaccuracy and worthlessness of the State vital statistics were conclusively shown in the reports of the committees on this subject. The Association formulated a protest, and appointed a special committee to memorialise the Legislature. By these means, and by the efforts of the State Board of Health, public opinion on the subject was awakened and so far educated that during the winter just passed a law was enacted which gives to New Jersey one of the best systems of registration as yet devised in this country. It has incorporated in it two features to which its peculiar

excellence are due, and which should be universally copied: 1st. That of issuing burial permits only after registry has been made by a properly qualified person; 2nd. The returns are made to an expert, who collates them in accordance with the views of the most eminent authorities, and draws from them their most important teachings of immediate and very practical application.

II. *Registration of Disease.*

We must not rest content, however, with the returns of mortality; we should advance to the registration of disease. This is practicable, and if not in all, yet in that large class of diseases in their nature preventable, of universally acknowledged utility. We do not delay until a street brawl becomes a riot before notifying the magistrate and securing police aid; neither should we wait until diphtheria, typhoid, &c., become epidemic before sending intelligence to the custodians of the public health. But this is not all; to make their knowledge of public utility, these custodians must be invested with adequate powers. At present there is little more expended upon the whole work of the Board of Health of the State of New Jersey during an entire year than the pay of two policemen. Its members labour without remuneration for the *sanitas publica*. Their power is mainly the educational impetus of just ideas, forcibly expressed. There are many ways of promoting sanitary reforms, but none, it appears to me, so practical as that of giving to the Health Board the money, means, and men to register diseases, to investigate their causes, to suggest and promote their remedies, and not unfrequently to bring offenders to suitable punishment.

III. *State Sanitary Legislation.*

There is a source of danger, as this last summer has strikingly shown, which cannot be warded off by sanitary legislation when limited to a few of the States. If those States which are the seats of yellow fever year after year do not provide efficient precautions to suppress or control the epidemic, it will annually invade other localities, following the lines of travel, and spreading northward in the Mississippi basin. We have recently seen the alacrity with which more favoured communities came to the relief of those afflicted with the epidemic. Help of every description was sent until the bountiful public was asked to hold its hand.

While the terrible plague lasted was not thought a time for good advice, but for good deeds. Now that the danger is over, the time has arrived to avert similar visitations in future. Does it appear unreasonable to ask for the most skilfully-devised sanitary regulations in localities where such a pestilence may germinate? Recent events have elicited a vast deal of discussion as to the origin of these epidemics and the modes of combating them. There is want of harmony, however, in all points but this: that some of the factors which are concerned in originating the disease are within human control, and prevention, therefore, is the duty of the authorities where the disease germinates.

Those involved in the consequences of neglect of these duties, however remote their homes, have a right to ask for reform. This agitation should not be allowed to die out with the pressure of the calamity which aroused it. It should be continued until every one of the States has an efficient health code. At present the majority have none, or very deficient health laws. Massachusetts has strikingly shown its general enlightenment by being the first State to have an efficient Health Board and a wisely-devised code of sanitary legislation. New York and Pennsylvania have neither, though strenuous efforts have been made by public-spirited individuals to do away with the stigma. In the west, Michigan has been distinguished by the excellence of its sanitary legislation, and the voluminous and valuable publications of its State Health Board. But Arkansas and Missouri are sadly deficient, and the case is even worse in Iowa, Kentucky, and Indiana. Some attempts to supply the most pressing wants have been made in Florida and in North and South Carolina, and health laws are not entirely wanting in the statute-books of New Hampshire, Maine, and Rhode Island. The necessity for educating the people in each State before the requisite legislation is secured, will require a considerable period to elapse before all the States have systems of laws in accordance with modern knowledge. In the meantime, in the name of all those good men who have perished, and as an acknowledgment of the nation's charity, let the plague-stricken States of the Gulf and the Mississippi basin lose not a day in adopting the wisest precautions experience and investigation can offer.

Struggling, as we are in this country, to have the importance of sanitary legislation generally recognised, the progress made in some directions is highly encouraging. It is probable that no community will take steps to learn what is essential to its health before it has suffered from lack of

information. To the distress of London the world owes those great works of the Royal Commissions on Water Supply and the Pollution of Rivers, which are the repertory of the best knowledge on these topics. The manufactories of England have made it necessary for the Government to take cognisance of aërial impurities, and much has been done in that country towards establishing a chemical climatology. Similarly the pollution of the Passaic by the manufacturing towns above has caused enquires to be set on foot akin to those referring to the pollution of the Thames, and has given rise to extended enquiries into the methods and aims of water analysis.* An attempt was made to deprive the inhabitants of New York of some of their public parks and occupy them with buildings devoted to military and other purposes. The more public-spirited citizens came to the rescue, and, through the influence exerted, a Public Parks Association and other means preserved the open squares as breathing places and pleasure grounds. The Association recognised as its principle of action that to preserve the parks they must be improved. The proposition was made and eloquently advocated by Dr. Seguin that the physical as well as the spiritual well-being of the citizens at large would be powerfully augmented by making the public gardens out-door schools, supplementing the in-door school system by that in which they are lamentably deficient—an education in the phenomena of plant and animal life. A beginning in this direction has been made in the Botanical and Zoological Museums of the Central Park of New York, and in the Fairmount Park in Philadelphia; but these are remote from the centres of population, and the objects of study should be placed where they could constantly appeal to the eye. The hygienic value of gratifying the sense of beauty, as well as satisfying the requirements of use, is more and more recognised. The first society on this side of the water organised with this object was the so-called Laurel Hill Association, of the village of Stockbridge, in Western Massachusetts. After twenty-years of activity, the result has been to produce a village of exceeding loveliness. Thousands of trees have been planted out along the road-

* "Report to the Board of Public Works of Jersey City," Profs. Wurtz and Leeds.

"Analytische Beiträge aus dem Laboratori und des Stevens Institute of Technology, Prof. Leeds: Zeitschr. für Anal. Chem., 1878.

"Recent Progress in Sanitary Science," Prof. Leeds: Annals of the Academy of Science, New York, vol. xi., 1878.

"Water Supply of the State of New Jersey," Prof. Leeds: Journ. Franklin Institute, March and April, 1878.

sides. The village cemetery, formerly neglected, has been surrounded by an exquisitely-kept hedge. Monuments have been erected to the memory of villagers whose subsequent achievements have made the place of their birth illustrious. Prizes have been offered for those who laboured most efficiently to improve the health and beauty of their native town, and for these prizes the poor as well as the opulent contend. In fact, the neatly-kept sidewalks, the attractive gardens, the pretty cottages of the poor, are a better indication of what healthy village pride can do for a community than the tunic lawns of the rich. I need not add that in a community where these things which add grace and beauty to the daily life have been done, the more important works of water supply, drainage, sewerage, &c., have not been left undone. Similar associations have sprung up throughout New England. In Williamstown the villagers have thrown down every fence, and this most picturesque of village towns is a beautiful park, through which the houses of the inhabitants are scattered.

IV. *Ventilation.*

In the matter of ventilation, a considerable advance on the whole is to be noted; in other words, the percentage of failures to successes, in cases where methods of ventilation for the time being in vogue have been tried, is slowly growing smaller. The volume of scientific literature, founded on our increasing knowledge of the properties of materials, of gases, and of heat, grows more rapidly than the generally accepted rules by which the art of ventilation is to be practised. It is noteworthy that there are few persons who do not regard themselves competent to arrange the ventilation of an ordinary building, and it has hitherto been left largely to the builder, the vestryman, and the school trustee. This should not be the case. What advance has been made is mainly due to the specialisation of this kind of professional labour—the foundation of a class of engineers who devote themselves exclusively to problems of this character, and who have fought their way into practice by successful work accomplished. The architect submits his plans to these specialists, who adds to them the requisite details of heating and ventilation. It would be a great step in the interests of sanitary science if the school or hospital trustee would not think it devolved upon them, as a portion of their office, to become for the time being an authority upon ventilation, and if they were, as a proceeding of sound economy, to rele-

gate this duty to persons properly qualified. As a matter of fact, these qualifications are obtained at a considerable expense to the commuity, for in this stage of the art of ventilation, there being no authority universally recognised and but few generally-conceded rules, every sanitary engineer goes through a somewhat similar series of experiments and failures before he arrives at a reasonably successful method in practice.

As far as I can learn, there appear to have been great successes as well as great failures, whether the system of ventilation by aspiration has been resorted to, or that by propulsion. At the present time many authorities of note have declared in favour of mechanical ventilation. And yet in a number, I might say in most of the asylums and hospitals in this country where fans have been introduced, they are now standing still. The Roosevelt Hospital, for instance, in New York, where the fan, after having been put in operation, was run backward, and run so for months. It is now stopped. This is one fact of many which would make us chary of affirming positively that either system is the better. Probably both, discreetly applied, yield good results, and in their skilful application, and not less the faithful supervision of the ventilating apparatus after introduction, are good results to be sought.

V. *Physical Education in Schools and Colleges.*

Progress in this direction has been initiated at our highest seminaries of learning, and is slowly working its way downward through our educational system. I do not refer to so-called athletic sports, although these had not attained to much prominence in our colleges prior to the year 1850, but to the introduction of physical exercise and instruction on hygiene as a part of the college curriculum. This, so far as I am aware, was successfully accomplished in Amherst College; and now, after a trial of nearly twenty years, is still regarded as an indispensable adjunct of the college course. The dignity of this department of instruction is emphatically recognised by appointing to it only distinguished members of the medical profession, and including them in the college faculty on the same footing as the other professors. It is made their first duty to know the physical condition of every student, and to see that the laws of health are observed by them. In case of sickness the students apply to this officer for a suitable certificate, which excuses them from college duties, and are put in the way of

obtaining suitable treatment. The statistics of the bodily condition of the students are regularly and frequently secured, and are supplying a collection of "physiological constants," which are of growing interest, and supply practical helps in determining whether physical condition is within the bounds of health, and whether their development from time to time is normal or otherwise.

All the classes are required to attend gymnasium exercises four times a week, and the regularity and faithfulness in this is made an element of collegiate standing. The performances are accompanied with music, and arranged to give full play to the animal spirits. This and the advantages personally experienced by the students have conspired to make the gymnastic fully as popular and well-attended as the literary exercises. Finally, the intelligent co-operation of the student is secured by instruction upon the means of preserving health, physical and mental, with supplementary lectures upon human anatomy and physiology. Prof. Hitchcock,* writing of the chances of life of the young men under this hygienic discipline as compared with men at the same age elsewhere, says it is regarded as an established law that the chances of life grow less and less from about the fifteenth to the twenty-third year, and the rate of decrease is very rapid. But the tables of health, as kept at Amherst College, show that there is an improvement in health from year to year through the course, the ages being from nineteen to twenty-three. For taking the number of sick men in the freshmen class as unity, the number in the sophomore year as 0·912, in the junior 0·759, and in the senior but 0·578, the percentage of sickness during the college course diminished to nearly one-half.

In the light of this successful experiment, continued for a period of twenty years, it is not premature to urge upon colleges generally the formation of a similar department, and for my own part I see no method of raising the character of public school instruction so effectual as that of giving to the physical training of the children a very prominent place.

VI. *Health Resorts.*

The growing facilities each year for travel are steadily increasing the number of citizens who visit the country, the sea-shore, and the mountains. A salubrious village is fre-

* Hygiene at Amherst College, Prof. HITCHCOCK: American Public Health Association.

quently transformed into a centre of pestilence merely by such influx of strangers, the entirely natural modes of disposal of refuse and excreta no longer being adequate, and the artificial methods not being provided until an outcry due to disease is raised. In no way, however, is the growing intelligence on sanitary matters more strikingly shown than by the extreme sensitiveness of pleasure-seekers to the salubrity of summer resorts in respect to water, sewerage, and drainage. Of the multitude of illustrations I need but speak of Bethlehem, in New Hampshire, a beautiful village situated 1700 feet above sea-level, and so renowned for the purity of its atmosphere that of the 40,000 hay-fever patients whom Dr. Beard has calculated exist (and hay-fever patients say that life to them is only a tolerable existence) in this country, several thousands annually spend part of the summer there. Its popularity increased in a few years so rapidly that a crowded village soon arose, and during the summer of 1877 an outcry was made concerning drainage. The towns-people, realising that the reputation of salubrity was the wealth of the town, steps must be taken at once to preserve it. They did so, and during the past summer the influx of visitors has been greater than ever.

These two stages in the growth of a summer resort—its sanitary degradation and subsequent rehabitation—can now be witnessed in every phase of their development, along the entire coast-line of the State of New Jersey. This great sea-side resort, a hundred miles in length, stretching from Sandy Hook to Cape May, is rapidly growing into an almost continuous city. It harbours each summer a vast multitude from our two metropolitan cities, and from the Middle and Middle-Western States. Even as a Sanitarium in winter, the physicians of Philadelphia, during the past lustrum, have recognised the great advantages that were pointed out by Dr. John Torrey nearly a half century ago, and are sending their patients in need of change of air to Atlantic City and neighbouring points. The arid expanses of its sandy shores have become in this way one of the principal sources of income to the State. Would it not, then, be a highly remunerative policy for the State to maintain their attractiveness? As a fact, nothing of a preventative nature was done. The shallow pits, which provided surface water for drinking and other purposes when the population was sparse, were multiplied when the visitors came by thousands. Malarial and typhoid fevers were rife in spots which the sea-breezes visited every day. Only with the consequent suspicion and public alarm which threatened to empty them

of their patronage did these places tardily move in the matter of adequate sanitation; and now the universal introduction of cemented cisterns, and the diurnal removal of garbage under the stringent regulations of local Boards of Health, attest the purpose of the great sea-side Sanitarium to retain its highly profitable reputation.

VII. *Sanitary Advantages of doing away with Illuminating Gas as a means of Illumination.*

Any process of illumination which returns to the confined atmosphere we breathe the products of combustion is theoretically open to objection. All methods of illumination up to the present time have depended on some process of rapid combustion, oxygen being withdrawn from the air, an equal bulk of carbonic acid returned to it, and oftentimes a large amount of heat as compared with the amount of light liberated in the process. If, now, we can illuminate without the subtraction of vital and the addition of mephitic air, and if we can produce an intense light without a corresponding heating of the surrounding atmosphere, we have made two steps of great hygienic value so far as the illumination and ventilation of rooms is concerned.

There is much reason for supposing that this will be soon accomplished in the wholesale introduction of the electric light. By very many roads a crowd of inventors is pushing forward to this end. The rapid destruction of the terminals, with the corresponding need of frequent adjustment, is being obviated by a variety of devices. In some the length of the carbons is made invariable, by a supply of carbon as soon as it wastes away, through deposition of carbon from a hydrocarbon atmosphere in which the electric arc is taken. In another, wasting is prevented by an entire exclusion of oxygen, and the terminals are surrounded by an atmosphere of pure nitrogen. Another experimenter separates his carbons by an intervening material over which the arc is formed, and all parts of this electric candle are burnt away at the same rate. In one of these ways, or in some other, the problem of lighting by electricity more perfectly and as cheaply as by illuminating gas will be solved and we shall have the attendant train of hygienic benefits. In the matter of street illumination the contamination of the atmosphere by gaseous products of combustion is, of course, not a matter of great moment, but in the illumination of those places of public assembly, the church, the theatre, the lecture-room, the improvement will be of much importance.

VIII. *Sanitary Surveys.*

The intimate relation between health and the configuration of the soil has been recognised from time immemorial. In truth there is reason to suppose that more practical weight was given to it in ancient than in modern states of society, for while, in the former, security from enemies and the possible exigencies of a protracted siege made it imperative to select high places capable of good drainage for city sites, the demands of commerce are now best met by towns at the lowest levels, frequently at the estuaries of rivers and marshes formed at the confluence of great streams. While the demands of commerce are inevitable, the care upon sanitary science to avert as far as possible attendant evils is not the less urgent. For this reason the rapidly increasing bulk of statistical information upon this subject is a matter of great gratulation. The geological surveys prosecuted by the State Governments, and latterly extended to the shores of the Pacific by the munificence of the National authorities, have supplied an admirable foundation. The hydrography of the sea-coast and the sea-board estuaries has been executed on a basis so broad and solid that the topography and hypsometry of the whole country can be built upon it. In addition to these we have a number of studies of the relation of topographic and geologic features to one or all the various types of disease.

Even before the inquiry instituted by the medical staff of the Privy Council of Great Britain, the extended research by Dr. Bowditch* demonstrated the intimacy of the relation between wet and retentive soils and the prevalence of consumption, these conditions of surface structure being chargeable with a thousand deaths from consumption in Massachusetts alone. Subsequently the fluviate and pond basins of Massachusetts were surveyed and mapped out by Rickwood. Staten Island having long lain under the ban of insalubrity, a number of gentlemen interested in its occupancy and improvement instituted a sanitary survey of the island, Dr. Elisha Harris and Mr. Frederick Low Olmstead examining into the more general questions involved, and Profs. Newbery and Trowbridge the structural conditions. The influence of the surface-soil and of the underlying rock, its porosity, its bedding and its joints, upon the local climate, the drainage, and the attendant salubrity,

* "Consumption in New England, or Locality one of its Chief Causes."

have served as models for the conduct of similar investigations in various other portions of the United States. Prof. Cook has been engaged for several successive summers in running a series of levels over much of the densely-populated water-sheds of the Hackensack and Passaic, with similar objects; and recently a most minute inquiry has been made into the soil, contour, and drainage of Hudson County, by Mr. L. B. Ward, C.E. He has determined how much of its area—which includes the communities of Jersey City and Hoboken, and the smaller towns of Harrison, Kearney, North Bergen, Union, Weehawken, and Bayonne—is upland, how much is marsh, what portion is rocky, what occupied by soil of various kinds, and, where possible, the nature of the sub-strata, with the population on each tract, and their condition in regard to sewerage and drainage. The ability of each variety of underlying rock, serpentine, sandstone, and trap, to carry off surface water, is considered, with the corresponding influence upon the surface temperature, dryness, and salubrity. When we consider the abrupt changes of habitat over this crowded area, it will be seen that it offers a field peculiarly favourable for the study of the effect of surface condition upon the rates and causes of mortality. Most fortunately the vital statistics of this district have been tabulated with exceptional fulness and accuracy, under the superintendence of the President of the Health Board, Dr. L. H. Elder. These statistics have been investigated by Mr. E. H. Harrison, C.E., of Jersey City, with extreme care. He has plotted upon a working map every case of fatal illness arising from insalubrious environments, each disease being indicated by a distinctive character. Other maps are in course of preparation, showing the relations of surface, contour and drainage of soil, of rock, of sewerage, and relative density of population over the same area.

IX. *Upon the Composition of the Atmosphere.*

In conclusion, I hope I may be permitted to say a few words with regard to one topic of sanitary science which for a long time has more particularly interested me, and which at present is the subject of especial study—the composition and purity of the atmosphere. As communities grow more dense, and factories multiply, the sources of aërial impurity augment in a rapidly growing ratio. In England the Government has been forced to appoint an inspector, the celebrated Dr. Angus Smith, who has made

thousands of examinations of the air in all parts of the country, and directs Government interference where persons and property are too much imperilled by atmospheric contamination. In Glasgow a city analyst has been recently appointed with this special duty. New York is already showing the effect of the sulphurous and nitrous vapours sent out from its myriad chimneys. Recently the U. S. Signal Officer, in his room at the Equitable Insurance Building, took out from a carefully wrapped package an instrument which he desired to show me, and it was hopelessly corroded by the acid vapours sent out from the tall chimney of the United States Assay Office near by. In Philadelphia there is scarcely a house-front which is not disfigured by some stain of magnesia and lime-salts, a result in part due to the acid vapours in the atmosphere. And when rains sweep down and carry with them in solution such agents, they are more powerful to corrode metal, and even stone surfaces, than would at first appear credible. When one of the normal constituents of the atmosphere is wanting—the ozone—it has largely lost its sweetening and disinfecting powers, and there is much reason for believing that the prevalence or severity of certain diseases is intimately connected with the varying amounts of ozone in the atmosphere. Unfortunately there is much difficulty in estimating the percentage of this constituent of the air, and in guarding against the disturbing influences upon our determinations of other bodies possibly present. To overcome these difficulties experiments have been on foot in the laboratory of the Stevens Institute of Technology for many months. Numerous analyses of the atmosphere, collected in various parts of the United States, have been made and recorded. They will serve as contributions towards a beginning of a chemical climatology of this country, and might, with great profit to the physician, the agriculturist, and meteorologist, be vastly extended by Government aid in connection with the Signal Service and the Department of Agriculture. We cannot do unaided as much in researches of this character as can be done in the laboratories of the Old World by Government assistance, but we can at least labour in the hope that the time is not distant when the importance of research of this kind, even if it does not end in a profitable invention, will be generally understood and generally encouraged.

IV. THE COURSE OF NATURE.*

By Prof. SIMON NEWCOMB.

IN imposing on its retiring President the duty of delivering an Address, the constitution of this Society sets no limits to his choice of a theme. Both in these and in the corresponding addresses delivered before the sister society of Great Britain, it has not been uncommon for the speaker to choose for his subject the general progress of scientific research during the year. This course is now less common than formerly, because, owing to the immensity of the field of research, it has become impossible for any ordinary mind to follow its progress in all its branches. I have thought, therefore, that a higher interest would attach to a theme chosen from the field of modern scientific thought, and, by a process in which I have been the follower rather than the leader of my own contemplations, I have been led to present to you some thoughts on the Course of Nature as seen in the light of modern scientific and philosophic research. Though I have but a single central idea to present to you, namely, that of the simplicity and universality of the Laws of Nature, yet so great is the confusion of thought which prevails on the question, What are the Laws of Nature? that it is necessary to approach my idea from more than one standpoint, and to illustrate it in more than one way.

We all know that the history of the Caucasian race, during the last three centuries, has been marked by a kind of intellectual development so entirely without precedent that some might call it miraculous; in fact, by such a development of the understanding of the course of nature as has revolutionised human society in many of its phases. You also know that this development has been marked by frequent collisions of opinion between the investigators of the material manifestations of nature on the one side (if I may be allowed to use the expression), and philosophers and theologians on the other, respecting the true theory of the course of nature. My desire in entering this field is to act the part of the peacemaker rather than that of a combatant, not sustaining any other propositions than those which are

* An Address delivered before the American Association for the Advancement of Science, St. Louis, August 22, 1878. Communicated by the author.

actually believed in by the large majority of educated men at the present time; but the confusion of thought on this subject, to which I have just alluded, is so great that—although I may combat no opinions actually held—it may be necessary to greatly modify their application, and to criticise the forms in which they have found expression.

The key-note of my discourse is found in a proposition which is fundamental in the history of modern science, and without a clear understanding of which everything I say may be entirely misunderstood. This proposition is, that Science concerns itself only with phenomena and the relations which connect them, and does not take account of any questions which do not in some way admit of being brought to the test of observation. The only universe it knows is that made known by the telescope, the microscope, and other appliances of observation. That this is the whole universe we should all be very sorry to suppose, and none more so than he who has the honour to address you. But, should I pretend to a scientific knowledge of what lies behind this visible frame, I should be acting the part of the rash speculator rather than of the cautious thinker. Only into a single field of thought do I dare to venture. When we trace the efforts of men to penetrate the secrets of Nature, we find them clearly divisible into two classes—philosophic speculation and scientific investigation. We find the objects of thought equally divisible into two classes—phenomena and their hidden causes, those unknowable entities out of which they proceed. The great progress which the last three centuries have witnessed has been wholly in the field of phenomena, and it is to this field, and to the results of scientific investigation in it, to which I ask your attention this evening. But, it is to be expected that, in this brief characterisation of our field of thought, I have failed to convey to your minds any clear conception of its boundaries. The progress here alluded to has been rendered possible only by entirely rejecting the mode of thinking about nature which was prevalent in former ages, and into which the untrained mind is almost sure to fall at the present day. The distinction will be evident to one mind at a glance, while another may be unable to comprehend it after all the explanations which it is possible to give. As my whole discourse will be misleading unless all my hearers have a clear conception of it, I shall endeavour to present you with the materials of such a conception, rather in the form of concrete illustrations in familiar language, than in that of abstract general definitions.

As one mode of expression, we might say that modern science introduces into the higher modes of thought about nature that same kind of practical good sense which characterises the successful man of business. Scientific investigation is, in a certain sense, purely practical in both its methods and its aims. There is a mental operation, with which all are well acquainted, under the familiar term "theorising;" to this operation all scientific investigation is so much opposed that the mere theoriser and essayist can never make any real advance in the knowledge of nature. To speak with a little more precision, we may say that as science only deals with phenomena and the laws which connect them, so all the terms which it uses have exact literal meanings, and refer only to things which admit of being perceived by the senses, or, at least, of being conceived as thus perceptible. This purely literal meaning of all scientific language is in strong contrast to the metaphorical and poetical forms of expression into which we are apt to fall in discourse upon abstract subjects generally, where our ideas cannot be at once referred to sensuous impressions.

We might also say that no question is a scientific one which does not in some way admit of being tested by experience. The single object of scientific research is to predict the course of nature, or the results of those artificial combinations of causes which we call experiments; and no question is a scientific one unless its solution will in some way advance this object. I must not, however, be understood as saying that the test of experience can always be immediately applied, because then no disputed question could be a scientific one. For example, the question whether man existed on the earth 50,000 years ago is a scientific one, because it is one respecting actual historic occurrence of scenes evident to the senses. It could at once be settled by simple inspection, could we in any way form a picture of the earth as it then looked, and it may actually be settled in the future by the presence or absence of sensible traces of the existence of man at those times. Should we, however, go farther, and enquire whether such men had souls, our enquiry would not be a scientific one, nor one in which science could in any way concern itself with profit. The soul can neither be seen nor in any way made evident to the senses of others. From the very nature of things, it could leave no material trace of itself to be unearthed by the geologist or antiquarian of a future age. So far are we from forming any conception even of our own souls, as sen-

sible existences, that no question affecting them, even now, is a scientific one; much less can science consider those of past generations.

There is thus a quite well-defined limit between questions which are scientific ones and those which are not scientific, and with which, in consequence, science has no concern whatever. You must not understand me as in any way claiming that questions of this last class are not worth thinking about. They include many which are of the most absorbing interest to the human race, and about which men will think the more as they become more thoughtful. But to mix them with scientific discussions will only introduce confusion of thought respecting sensible things, without in any manner advancing their solutions. The current desires that science shall consider man as something more than an animal are as unreasonable as if we wanted to make algebra a help to moral philosophy.

This limitation of all scientific research to a single specific field is something so little understood that I may have occasion to call it to mind in other connections. But there is another equally essential maxim of science which I must explain in order that you may understand the spirit which animates scientific investigation. It is that the man of science, as such, has no preconceived theories to support, but simply goes to nature to find out and interpret what she has to say according to her exact meaning. What he may desire to be true has no bearing at all on the question what really is true. Here arises the inability of men of science to view theological questions in a light which shall be satisfactory to the theologians, and the corresponding inability of the latter to appreciate the spirit in which men of science discuss the problems of life and being. We hear much at the present time of a supposed conflict between science and religion; but it is rather a conflict between two sets of men who view nature from opposite and irreconcilable standpoints. It is essential to the understanding of our theme that we should see in what this difference of view consists; I shall, therefore, endeavour briefly to explain it.

The theologian looks upon the doctrines he has been taught as something the truth of which is essential to the welfare of humanity, and, we might almost say, to the supremacy of the Creator. He thus invests them with an attribute of moral excellence, implied rather than expressed in the term orthodoxy, and looks upon those who attack them not simply as men who are mistaken, but as men who are seeking to do a great injury to the human race. Hence the

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idea of weighing the arguments on both sides with entire indifference to the result is one which he cannot be expected to receive with favour, or even to readily comprehend as received by others. His idea of truth is symbolised in the pure marble statue which must be protected from contact with profane hands, and whose value arises from its beauty of form and the excellence of the ideas which it embodies. He therefore looks upon those who attack it with feelings not unlike those of the keeper of the statue upon a chemist who refuses to see anything in the statue except a lump of carbonate of calcium of peculiar form, and who wants to handle it, weigh it, determine its specific gravity and its cohesive power, and test its substance with acids. The corresponding idea of the scientific investigator is symbolised by the iron-clad turret, which cannot be accepted until it has proved its invulnerability. Instead, therefore, of being protected from violence as if it were a product of the fine arts, violence is invited. Its weak points are sought out by eyes intent on discovering them, and are exposed to the fire of every logical weapon which can be brought to bear upon them. A scientific theory may thus be completely demolished; it may prove so far from perfect that its author is glad to withdraw it for repairs or reconstruction; or it may be hammered into an entirely new shape. But however completely it may stand the fire, it maintains its position as a scientific theory only by being always in the field ready to challenge every new comer, and to meet the fire of every fact which seems to militate against it. A countless host of theories have thus been demolished and forgotten with the advance of knowledge, but those which remain, having stood the fire of generations, can show us a guarantee of their truthfulness which would not be possible under any other plan of dealing with them.

As a consequence of this way of viewing theories, the scientific man recognises no such attribute as orthodoxy in his doctrines. There is nothing at all which he says you must believe to be true as a condition of scientific recognition. There may, indeed, be many propositions to doubt which would indicate extraordinary incredulity, or downright folly, or even insanity, and he might, therefore, regard a sceptic as possessing a pitiful feebleness of intellect, and in consequence, refuse to listen to him; but he would refuse, not because the man disbelieved something which was undoubtedly true, but because he was not worth listening to. Perhaps the point which I am striving to make clear may be most readily grasped by the reflection that

science offers its highest rewards to him who will overthrow and supplant its best-established and most widely received theories. Thus the names of the men who disproved the theory of epicycles in astronomy, and the doctrine of phlogiston in chemistry, occupy the most honourable positions in the history of science. Of course, no such thing as authority in science has anything more than a provisional recognition. If a man of good repute says that he has investigated a certain subject and reached a certain result, the latter may be accepted on his authority, in the absence of other evidence. But this gives no reason at all why any one else should not reach a different result, and it would be no argument at all to cite the mere authority of the first against the second. In case of a discrepancy of this kind, the whole question would have to be re-investigated. The dictum, "It is written," has no terror whatever for the investigator of nature; he can recognise no authority for any feature in the course of nature, except nature herself as he sees her.

These principles are of so much importance in the philosophy of science that I may be pardoned for viewing them in yet another light. In reading those discussions with scientific men on certain theories recently advanced by the more advanced students of philosophic biology into which the representatives of theology sometimes enter, I have often noticed that if the representative of science propounds, discovers, or brings forward any fact or principle which seems to tell against his side of the question, the other calls it an "admission," or "concession," just as if his opponent had first selected his side for the love of it, and was then unwilling to concede or admit anything which might militate against it. Now, to go into the philosophy of the subject a little deeper than heretofore, allow me to say that the man of science professes no ability to recognise truth on sight, as he would recognise a house or an animal. The question whether any given proposition is or is not true, is necessarily to be decided by the human judgment, co-ordinating all the facts which bear upon it. There is no such thing as a revelation of scientific truths, and even if one should claim that there was, the admission or rejection of the claim would be an act of the judgment, which thus becomes the ultimate arbiter in any case. Hence a proposition is to be proved probable or true, not by anything in itself, but by a more or less long and painful examination of the evidence for and against it. Everything that can be found to militate in favour of it is put into one scale, and

everything that can be found to militate against it is put into the other. If the investigator is imbued with the true spirit of science, his search is equally vigorous for arguments to go into the two scales. When he says that the proposition is worthy of being received as true, he means, not that it bears any recognised seal of truth, but that the evidence in favour of it entirely preponderates over all that can be brought to bear against it.

You will not understand me as maintaining that every individual man of science constantly maintains this spirit of impartiality any more than every Christian constantly lives up to the highest standard of his profession. Hot conflicts have sometimes raged, and there is no reason to suppose that they have entirely ceased, even now, in which each combatant could only see one scale. But the spirit I have described is that in which science exhorts her votaries to approach every question, and in which they will constantly endeavour to approach it if they are worthy of their profession.

Let us now approach our main theme, the course of visible nature. Let me again remind you that of the two universes, the seen and the unseen, I am only going to speak of the former. We find ourselves placed in this world in the midst of a vast theatre of activity. We see an atmosphere agitated by storms; great masses of water rising in the air to form clouds, and, after falling to the earth, flowing as mighty rivers to the ocean; countless forms of vegetation rising from the earth and then returning to it; a sun supporting all life on our planet with its heat; an infinitude of chemical changes going on around us; countless stars moving through space with velocities which transcend all our conceptions. To all appearance these operations have been going on for millions of ages past, and may continue for millions of ages to come. As the thinking man contemplates them he is led irresistibly to the conclusion that they do not go on at random, but that they are joined by connecting links, or are in some way the product of knowable causes. From his earliest infancy he sees connections between them which enable him to foresee results. He finds that fire burns, that the sun warms, that food satisfies his hunger, and that heavy bodies fall with a certainty which shows the forces at play to be invariable in their action. To penetrate the mystery in which these forces are enshrouded, he has exerted the efforts of his intellect from its first dawn until the present time. What general conclusions has he reached?

From the earliest times at which man began to think, two modes of explaining the operations of nature have presented themselves to his attention. Those modes are sometimes designated as the teleological and the mechanical.

The teleological explanation of nature presupposes that her operations are akin to human actions insomuch as they are under the control of, and directed by, one or more intelligent beings having certain ends in view; that the events are so directed as to compass these ends; and, finally, that the relation of the events to the ends admits of being discovered by observation and study. This last condition is a very mportant one, because without it the teleological explanaton of the cause of nature would not be a scientific one. The doctrine that the Author of Nature has certain ends in viw, and directs the whole course of events so as to bring thm about, will not enable us to explain and predict the events unless we know what those ends are. But, as I hav already said, the test of scientific advance is the power of fresight—of foreseeing what result any combination of circmstances will lead to. If we always had to wait for the isult, and could then only say, I know this is the result whic was intended, because it has happened, no actual foresiht would be possible; and however excellent the doctrie might be as a theological one, it would not admit of beig tested by observation and experiment, and the questio of its truth would, therefore, not admit of being settled y scientific investigation.

You ay recall the remark of a satirical philosopher when he saw e gifts which those who escaped the dangers of a certain eacherous and stormy sea offered up to the goddess who hachis sea at her command: "I see no offerings from those wl were lost," said he. It was not till the voyager had got fely to shore that he found himself under the protection othe goddess.

It musbe well understood that the teleological theory of nature, ors it is now familiarly called, the explanation of natural phomena by design, has two distinct forms, the scientific a the theological. These forms are not antagonistic onesthe one held by scientific men and the other by theologis; for, as you may well know, the scientific form is the e in which scientific men almost universally reject the eological theory, while they have nothing to say against e other forms. The forms refer only to the fields to wh the theory may belong, the scientific and the theologic. The distinction turns on whether we suppose the en which the Creator has in view to be dis-

coverable by scientific investigation or to be inscrutable. Only in the former case have we, as scientific investigators, anything to do with the question. The theory, as we have to consider it, is in brief this: that the course of events in inanimate nature is from time to time modified by invisible intelligences just as it is modified by man when he changes the course of a river or plants a forest.

The other explanation of nature is the mechanical one. It assumes that her processes go on in accordance with certain laws which admit of being fully comprehended by the human mind so far as their effects are concerned. Each state of things is the effect of the state which immediately precedes it, and the cause of that which immediately follows it. The course of nature is thus considered as an endless chain, of which the work of science consists in making out the forms of the links, and the modes in which they are connected. In this work we have to be concerned with two things: the general laws of nature, as they are familiarly called, and the facts or circumstances which determine the operation of these laws. This distinction is most clearly seen in human laws. Thou shalt not steal, is a law; that John has stolen, is a fact. The combined result of the law and the fact is that John is locked up in jail. So that all bodies near the earth gravitate towards it with a force directly as their mass, and inversely as the square of their existence from its centre, is a universal law of nature. The Niagara River and the precipice are facts; and the cataract is the result.

But the general explanation of the course of nature, on the mechanical theory, is not of this simple kind, because the laws of nature do not act singly, but in combination; so that the result of each is modified by the action of all the others which come into play. The law of gravitation is not that all bodies must fall, but only that they tend to fall, and, therefore, will fall unless held up by some sufficient opposing force. So long as I support this weight in my hand it does not fall, because the force of gravitation and the resistance of my hand neutralise each other. But the instant I let go the weight drops, according to a certain law known as that of uniformly accelerated velocity.

The doctrine I am endeavouring to elucidate is this: knowing a few simple laws of nature, of which gravitation is one; knowing also the arrangement of material things within the field of investigation; that is, knowing the facts, we can predict with unerring certainty what the result will be: or if we cannot predict it, it is not because of any

quality of the thing itself, but only because of the insufficiency of our powers. Moreover, these results will be, as it were, another layer of facts from which it is possible to predict new results to follow them, and so on without limit, unless some force from without intervene to change the course. If we include the whole of nature in our field, no outside facts can come in, and her course, therefore, admits of being predicted with entire certainty from beginning to end.

Now the point which I wish to bring to your attention is the revolution which modern science has brought to pass, in the opinions of mankind, respecting the relations of the two classes of causes, or supposed causes, which I have described. That all events could be explained on teleological principles, it is not likely that any one ever supposed. That the falling of heavy bodies, the running of rivers, the changes of seasons, and the revolutions of the heavens were all in accordance with mechanical laws, at least so far as the phenomena are concerned, no one ever knowingly denied. But it was thought that the action of these causes was from time to time modified by the introduction of causes of the teleological class, just as a rock might be kept from falling by the force of cohesion. The general rule has been that the more ignorant the age, the more minute and immediate was supposed to be the action of those beings who were modifying the course of nature in order to compass their ends.

As illustrating this, I might commence with the age of image worship, when the fate of the individual is supposed to be at the mercy of certain spiritual entities, symbolised by forms of wood, stone, or wax. But, leaving out of consideration ideas so different from those which prevail among us, let us come nearer home. It is not many generations since men who knew that the regular course of nature went on in accordance with mechanical laws believed, nevertheless, that occurrences of a terrific or extraordinary character were specially brought about to compass some end of Providence. Not only so, but, what is most essential to our theme, this end was supposed to be a scrutable one. The motions of stars and planets had gone on from age to age, until no new aspect of them inspired alarm. But a comet was looked upon as a messenger specially sent to give warning of a coming calamity. The scrutable end was, in this case, the warning of mankind. Ordinary cases of sickness and accident, whatever their result, always have been looked upon as a part of the regular course of events. But it

is not many centuries since the pestilence was believed to be specially sent by Heaven to punish mankind for their wickedness. Punishment and terror were here the ends which Providence was supposed to have in view. The regular daily breezes and showers were supposed to be the result of natural laws. But these laws were not supposed to be entirely adequate to the production of the tornado, which was again a special messenger, and they were suspended or their action was modified in times of extreme drought threatening mankind with famine.

These special messengers of Heaven have, one by one, yoked themselves to the car of natural law, so that I think I can hardly be wrong in saying that the supremacy of mechanical law and its adequacy to account for the whole course of nature, as we see it going on before us, is now the almost universal opinion of educated men. This revolution in human thought is, perhaps, clearly brought out in the different view we now take of certain religious observances introduced by our ancestors, whose ideas would now be considered as approaching the irreverent. Take, for example, the prayers for the right kind of weather, which we find in our prayer-books. When they were first composed and inserted, their object was a purely practical one. As the farmers now sometimes fire off cannon to make the black cloud break and discharge its contents upon the parched field, so the prayers were to be offered up in order that the aqueous vapour in the air might be made to condense and fall. That a much more exalted view of prayer than this is now taken by the more enlightened portion of the religious world, I think we have every reason to believe.

Although we can hardly entertain a serious doubt that the mechanical theory of natural operations, or, as it is sometimes called, the doctrine of the uniformity of nature, is generally acquiesced in by the mature thought of intelligent Christendom, yet objections are frequently made to it because it seems to run counter to some of our most cherished ideas. If it were not paradoxical to make the assertion, it might be said that we hold, or at least express, entirely inconsistent views on the subject. The fact is that we are pupils of two opposing schools, which are, in a certain degree, antagonistic, one of which we cannot, and the other of which we will not, give up. In one of these schools the chief teachers are observation and experience. All sentiment and emotion are banished from its curriculum, which admits only the hard realities of the outer world. The older we grow the more we see and hear of this school, and the

more unreservedly we accept its teachings. It tells us that the whole course of nature takes place in accordance with certain laws capable of expression in mathematical language; that these laws act with more than an iron rigour, and without any regard to consequences; that they are deaf to prayer or entreaty, and know no such thing as sympathy or remorse; that if we would succeed we must study them, and so govern ourselves that their action shall enure to our benefit.

The other school is that of sympathy, emotion, and religious faith. In it, as children, we receive our first teachings. It shows us ourselves placed, as it were, in a forest of mystery, surrounded by forms over which we have no control, and able to penetrate so little into the surrounding darkness that we cannot tell what shall happen to us on the morrow. It has in all ages peopled the thickets with invisible beings having an interest in our welfare or our injury, or with providential interferences designed to compass ends of which we in advance have no conception. Its teachings are nearest and most welcome in times of affliction and fear. Its objections to the teachings of the other school are heard far and wide through the land. Notwithstanding the number of forms which these objections take, their essence may be condensed into a very few sentences. The following will probably be accepted as a fair rendering of their substance.

You take a contracted and unphilosophical view of nature when you say that the world is governed by inexorable laws. These laws are not governors, but only the instruments of government by which the real governor executes his purposes. With them, but without subverting or violating them, he can reward or punish, bring on prosperity or call down disaster, according to the dictates of his sovereign will. The child and the peasant call the thunder the voice of God. The modern philosopher attempts to correct them by showing that it is the product of evaporation and of atmospheric electricity. But the view of the child is really the more correct of the two, because he ascends at once to the first cause, and thus sees further than the philosopher who corrects him, because the latter stops short at the immediate or secondary cause without even trying to raise his eyes to the higher source of power. I think I am not far wrong in giving this as the substance of the most cogent objections which may be anticipated in any quarter against the mechanical theory of the course of nature.

Now, if these views referred only to inscrutable first

causes of things, or to the intelligent but invisible substratum which underlies the whole cause of nature, we should have no occasion to discuss them, because they would lie outside the field I have assigned as that of our contemplation at the present time, and which I have sought to describe as the field of phenomena. The doctrines that all things go on in exact accordance with the will of the Creator; that he has certain ends which the laws of nature are designed to bring about; and that an intelligent cause lies behind the whole universe of phenomena, are of a class which science has no occasion whatever to dispute. If it were made clearly to appear that the field of the teachings in question was thus limited, and was entirely distinct from that of phenomena, with which alone science is occupied, there would be no occasion for dispute between the two schools. I have no disposition to throw a single stone across what I consider the sacred boundary line, nor to enter a field which I am by natural and acquired habits of thought unfitted to cultivate. As men of science let us by no means attempt to penetrate a region in which the eye of science can see nothing but darkness. If we thus subject ourselves to the imputation of being "of the earth, earthy," we may console ourselves that our edifice is firm and durable because it does not seek to rise into regions of serener air, nor to rear its dome above the clouds.

I can hardly be mistaken in saying that the objections to the mechanical theory of nature, which I have just tried to formulate, are not always confined to the field of inscrutable first causes. There is a part of the boundary line over which the stones are flying very thickly. While some of the combatants may profess to make no attack on the doctrine of the uniformity of natural law, I cannot but think that these professions often arise from a misapprehension of the scientific side of the question. Indeed, I must confess that I have met with a difficulty from my inability to form a clear idea of the views really entertained by the school now under consideration. I have made a somewhat careful study of some of the most elaborate works of the writers of the theological school, devoted to this very topic, and I have left them without being able to decide in my own mind whether the writers do or do not hold unreservedly to the mechanical theory of the course of nature. That nearly all intelligent men really believe in this theory, at least so far as the present time and dispensation are concerned, we have abundant reason for believing. Nor is there even among advanced theologians any lack of profession of a belief in the

uniformity and supremacy of the laws of nature. But when thinkers of the other school maintain the doctrine, and trace it to its logical consequences, undisguised by sentimental language or figure of speech, they are met with criticism which I can account for only by supposing that the theologian understands by laws of nature something different from what is understood by the man of science.

Let us try to condense the questions at issue into the smallest possible space. The scientific philosopher maintains that the natural course of events goes on in invariable accordance with certain knowable laws. He asks the theologian in the words of Pope:—

> "Think'st thou like some weak Prince the eternal cause
> Prone for his favourites to reverse his laws?
> Shall burning Ætna, if a sage requires,
> Forget to thunder and recall her fires?
> On air or sea new motions be impress'd,
> O blameless Bethel, to relieve thy breast?
> When the loose mountain trembles from on high
> Shall gravitation cease if you go by?
> Or some old temple, nodding to its fall,
> For Chartres' head reserve the hanging wall?"

To all these questions the other answers no, and thus all occasion for dispute ought to end. But it does not end, by any means; for he proceeds to criticise the views of the questioner on the ground of their narrowness, and to inform him that the Creator can (and, by implication, that he does) so arrange things that any result he may wish shall be brought about by the action of natural laws themselves. "We do not expect Ætna to recall her fires when a sage is near; or the air and ocean to acquire new motions to preserve a saint from danger." . . . "Should these individuals not be rushing recklessly against the known laws of Heaven, or should it be the will of God to preserve them, it will be found that provision has been made for their escape, and that not through the powers of nature disobeying their own laws, but through other powers in nature opportunely interposing to stop, to turn aside, or otherwise to modify their operation."

Now, always supposing that such remarks as these are intended to apply to the domain of sight, hearing, and understanding, they differ fundamentally from the scientific theory in their view of what constitutes the laws of nature. The school seems to look upon causes and effects in nature as belonging to two different classes of things. They see an immense collection of causes, to each of which the appropriate effect is tied. So long as the cause is followed by its

effect, the laws of nature are satisfied. So if the Ruler wants to reward, punish, kill, or rescue, he has only to bring into operation the appropriate cause at the proper moment; the natural effect follows, and His will is executed without any violation of the laws of nature. I am not sure that this is an exact statement of the views to which I refer; but it is the best I can gather from the study of the forms in which they have found expression. Supposing this to be the view really entertained, it is essentially different from that held by the scientific philosophy. The course of nature as it presents itself to the eye of science is not a collection of isolated causes, each with its effect attached to it, but it is rather to be symbolised by a chain in which each link is connected with the link which precedes it and with the one which follows it. At each moment of time the state of the universe is the effect of the state which immediately precedes it, and the cause of the state which immediately follows. There are no such things as distinct causes and effects, but only laws of progress which connect the successive links of the seemingly endless chain.

As an illustration of this, let us take the falling of the rock. To the mere observer there is no evident reason why it should fall at one time rather than another; he may, therefore, feel that there is room for speculation as to the cause which made it fall at the exact moment it did. But science teaches that it will fall at the very moment when the cohesive attraction which binds it to the mountain behind becomes less than the weight of the rock. We might suppose a power to so adjust the causes which effect the cohesion that the rock shall fall at some desired moment. But any such adjustment would be as complete a change of the course of nature as if the power should hold the rock up after it had begun to fall. The natural processes by which the cohesion of the rock is slowly diminished, though largely hidden from our view, are governed by laws as precise in their action as those which regulate the motion of the planets. The water which falls from the clouds slowly percolates through the ground and enters a crack in the supporting mass. It wears it away at a rate dependent on the solubility of the material and the quantity of water which falls. A constant but certain molecular action goes on without ceasing between each molecule of water and each molecule of rock. The strength of the latter is thus weakened according to some law admitting of precise mathematical statement. Thus a mind possessed of sufficient mathematical ability, knowing how much water runs over

the rock from time to time, and knowing also the laws of molecular action between the rock and the water, could determine long in advance the very moment at which the rock would fall.

Going back another step, we see that the quantity of water which runs over the rock depends on antecedent circumstances in the same way, namely, upon the quantity of the rainfall and the arrangement of the crevices in the ground. However the latter may have been produced, the cause is still another link in the endless chain which we can trace back to preceding links as far as we please. Equally is the rainfall a fixed element, determined by the course of the winds and the amount of moisture which they carry. Thus we have a network of causes too complicated for the human mind to trace in detail, but which the philosophy of science teaches us act with mathematical certainty. No tempering, modifying, or adjusting action comes in at any stage of the process, so far as we can see; if we admit such action we have to keep placing it farther back as our knowledge increases.

Now there is one feature of these causes, the admission or rejection of which constitutes the main point of difference between the two schools of thought which I have before indicated. All are agreed that the course of nature is determined by what we may call causes or laws, but all are not agreed as to the scope of action of these laws. The great and distinguishing feature which the school of science recognises, and which the other school does not recognise, is that all the laws of nature act without any scrutable regard to consequences. I qualify my statement by the word scrutable, because it is entirely outside the pale of scientific research to speculate upon possible inscrutable ends in nature. This being a subject of which the man of science, speaking as such, can affirm nothing, so he can deny nothing. Having found that no trace of regard for consequences can be seen in the mode of action of the laws which he investigates, but that the whole course of things, so far as his eye can penetrate, may be explained and predicted without supposing any such regard, the demands of science are satisfied, and he must there stop.

Let me illustrate this by going over the train of thought which has just occupied us in the opposite direction, starting from the rainfall, and tracing the succession of causes to the fall of the rock. The spot at which each drop of rain shall fall is determined by antecedent conditions entirely, by gravitation, and the winds. The drop neither

seeks nor avoids the crevices, never asks in any way what shall be its destiny after it reaches the ground. It strikes the ground wherever gravity and the winds bring it, percolates through the soil according to the law of least resistance, and dissolves the rock according to the laws of chemical affinity, without any respect to the consequences, immediate or remote. At length a moment arrives at which the cohesive force of the rock becomes less than the weight which urges it downward. This moment is fixed entirely by antecedent circumstance, such as the solubility of the rock, and the amount of water which percolates over it. At this very moment the rock begins to fall. It falls sixteen feet the first second, three times that distance the next, and so on, according to the mathematical law of falling bodies, without any respect to the lovely character of the beings it may destroy, or the disasters with which it may crush the fondest hopes of men. The region may be the wilderness; the passer-by may be a babe in its nurse's arms, an angel of charity, fulfilling her mission of good will, or a murderer aiming the deadly blow at his victim; but under no circumstances can we see that these conditions in any way affect the chain of causes which lead to the falling of the rock, or cause it to wait a moment, or swerve a hair's breadth from its inevitable course.

According to the theory of the course of nature, which I am trying to elucidate, the chain of causes which we have described, each cause acting according to antecedent conditions, but without any regard to consequences, is the type of the whole course of inanimate nature, as far in space as the telescope can penetrate, and as far back in time as the geological record can be deciphered. An essential feature of the theory is, that the laws which connect the several links of the chain, and thus determine the progress of events, do not possess that character of inscrutability which belongs to the decrees of Providence, but are capable, so far as their sensible manifestations are concerned, of being completely grasped by the human intellect, and expressed in scientific language. Without this the theory would have no practical bearing whatever; because, to say that the course of events is fixed, but by laws which we can never grasp, would give us no clue at all to learning what that course shall be, and would be equivalent to telling us that it is enshrouded in the same impenetrable mystery with first causes. A very important feature of the progress of science is found in the constant resolution of the laws of nature into more simple and elementary ones, until we reach principles

so simple that it is impossible to analyse them farther. Let us take as an instance of this the laws of the celestial motions. When Kepler discovered that the planets moved round the sun in eclipses, having the sun in one focus, he found what were, for his time, simple and elementary laws. They were entirely comprehensible, admitting of being expressed in mathematical language. They enabled him to predict the motions of the planets, and, so far as the intellect of the time could penetrate, they could not be resolved into more simple expressions.

But when Newton appeared on the scene, he showed that these and other laws could be expressed in the simple and comprehensive form of gravitation of every particle of matter toward every other particle with a force inversely as the square of the distance which separates them. All the laws of planetary motion which had before them discovered, were shown to be reducible to this one simple law, combined with certain facts respecting the directions and velocities of the planetary motions. The most essential of these facts is that the velocities of the planets in their orbits are such that under the influence of the sun's gravitation, these orbits are nearly circular.

By this grand generalisation Newton reduced the laws of the celestial motions to a form so elementary, simple, and comprehensive, that no further reduction seems possible in our state of knowledge. Attempts have been made to show that gravitation is itself the result of discoverable causes, but they appear to me entirely unphilosophical, since the causes into which they would resolve gravitation are more complex than gravitation itself. But for our present purpose it is not necessary to concern ourselves whether gravitation may arise from some more subtile principle as yet undiscovered. The point which I wish you to grasp is the entire comprehensibility of the law as it now stands. There is no mystery surrounding it. When I say that any body left unsupported will fall toward the centre of the earth until it meets with the earth itself, or some other obstacle to its farther fall, you know exactly what I mean, and what are the results of the law which I enunciate. In a certain sense we might say that the laws of nature are simply *general* facts, distinguished from special facts by their dependence upon certain antecedent conditions. Considered as such, there can never be any doubt as to their meaning or results. There is no profound philosophy involved in their action or expression any more than there is in such statements as that all unsupported bodies fall toward the centre of the

earth; that gunpowder, when touched by fire, suddenly changes to an incandescent gas; that water, at ordinary pressure, changes to steam at a temperature of 212°.

Now, scientific investigators are earnestly endeavouring, each in his own sphere, to do for the whole of nature what Newton did for the laws of planetary motion, to find and announce the elementary principles which connect all the links of the endless chain which symbolises her course. The student of chemistry cannot doubt that the innumerable properties of the various compounds which he studies arise from the play of certain attractive and repulsive forces among the elementary molecules of the matter of which these compounds are formed. Could he only learn the law according to which these forces act, chemistry might become very largely a deductive science, and the properties of compounds might be predicted in advance, as the astronomer predicts the conjunctions of the planets. The idea now entertained by those who see farthest in this direction is that all the physical properties of matter depend upon, and may be reduced to, certain attractive and repulsive forces acting among the ultimate atoms of which matter is composed.

It may also be supposed that all the operations of the vital organism, both in men and animals, depend in the same way upon molecular forces among the atoms which make up the organism. The operation of forces unknown to chemistry must, indeed, be presupposed, but there is no reason to suppose that these forces are less simple than chemical ones. Some would even go so far as to explain the facts of consciousness in this way. The philosophy of this explanation belongs, however, to another department of thought—that of scientific materialism—into which we cannot at present enter.

The most startling attempts in the direction I have indicated are those which are designed to show that those wonderful adaptations which we see in the structure of living animals, and which in former times were attributed to design, are really the result of natural laws, acting with the same disregard to consequences which we see in the falling rock. The philosophy of Darwinism, and the theory of evolution, will be at once brought to your mind as forming the modern system of explanation tending to this result. On these theories the eye was not made in order to see, nor the ear in order to hear, nor are the numberless adaptations of animated beings to the conditions which surround them in any way the product of design. Absurd as this theory appears at the first glance, and great as is the anxiety to

secure its rejection, the question of its truth is to be settled only by a careful scientific study of the facts of nature, and the laws of hereditary descent. The principle which is to aid in its settlement is universally admitted in quarters where it is fully understood. We are not to call in a supernatural cause to account for a result which could have been produced by the action of the known laws of nature. The question then is whether these laws of hereditary descent and of natural selection are adequate to account for the gradual growth of such organs as the hand, the eye, and the ear, and for all the adaptations which we see in nature. If they are it would be idle to call in any other cause, except we place it behind the laws, and if we place it behind those laws we must equally place it behind all others. Of course, such a cause lies beyond the field of sight, and does not, therefore, belong to scientific observation. Granting the theory, then, so far as the eye of science can penetrate the whole result is brought about by laws acting in seemingly blind disregard of consequences.

Let us now turn once more to the theory of scrutable design, which supposes, at least, the occasional action of causes which the human intellect can perceive to have been intended to produce certain effects, such as the salvation of the righteous, the punishment of the wicked, the warning of the indifferent, or the preservation of the race. Studying this theory from the purely scientific standpoint, in all the varying forms in which history presents it, we see its distinguishing feature to be the idea of causes acting so as to bring about certain results.

When Pallas inspired Diomed with renewed strength, and gave superhuman accuracy to his aim, it was in order that he might be able to pierce his Trojan enemies. Ordinary investigation might fail to show that his hand trembled less than usual as he raised his javelin, but the goddess took care that the last tremulous motion of his hand, as the weapon left it, should be in the direction to send it into the breast of the foe. The utterances of the oracles were determined, not by the past or the present, but by events still in the future. The blazing comet appeared, not in obedience to a chain of causes commencing with the creation, but in order that man might be warned of the coming calamity. When the prayers of the righteous averted the coming storm, the cloud moved aside in order that their fields and houses might be saved, and when they brought down the gentle rain upon the parched fields, the rain fell in order that famine might be averted.

These supposed causes differed from what enlightened minds now understand by the term Providence, in being amenable to scientific investigation, and in not being included in the regular chain of natural phenomena. The designs of Providence are inscrutable, but those of Pallas and Juno were not. Careful experimental investigation, such as might have been undertaken by a Helmholtz of that time, would have sufficed to show just how Pallas wanted the spear thrown, if the view of the Homeric age was the correct one. When the King died, or the enemy was victorious, men thought they knew exactly why the comet appeared when it did.

These views having so far vanished into thin air, I do not see how we can avoid recognising the reality of the revolution which modern science claims to have made in the views of men respecting the course of nature. And yet, as I have already shown, there are many tendencies in our being which make us unwilling to admit the revolution, and lead many to look upon the old theory as correct, provided it were only considered as tracing causes to the will of the Creator. On what is this view founded at the present time? Entirely, it seems to me, in ignoring the distinction between the scrutable and the inscrutable, between the seen and the unseen worlds. Science has, to a greater or less degree, banished final causes from the visible universe; but they act with undiminished vigour in the invisible one. Such a translation may not be a great revolution in thought, from a theological point of view, but it certainly is from a scientific standpoint, which considers only visible things.

I can readily imagine your asking if teleological causes can be really considered as absolutely banished from the whole domain of visible nature, if, considering how limited our knowledge, and how vast that part even of the visible universe which we do not know, it is not rash to assert that we know the true theory of nature, even in the field of phenomena. This question may lead us to look a little more carefully than we have hitherto done upon the exact standing of the doctrine of the uniform course of nature according to antecedent causes, and the relation of this doctrine to modern scientific investigation. And this leads me to say that it would be entirely unphilosophical to regard the revolution I have described as a scientific discovery or induction. It may be doubted whether the scientific mind is really any less disposed to believe in final causes than the ordinary mind. Nor can the theory that the course of nature is symbolised by the chain of cause and effect, as I

have described it, be considered as a product of modern investigation simply, or as belonging especially to the present age. It is a theory which has been, in a limited sphere, recognised by all men at all times. The reason why modern science has so greatly extended its scope is that modern science has acquired a vastly more extended view of nature than has before been obtained. One of the most curious and suggestive features of the teleological theory has been that the action of teleological causes has always been ascribed to operations into which human investigation could not penetrate, although their ultimate effects might be plainly seen. Whenever the subject becomes so well understood that the chain of natural causes can be clearly followed, miracles and final causes cease, so far as the scientific explanation of things is concerned. That a ball or spear thrown in one direction would bend its course into an entirely different direction no one ever supposed. Homer never imagined Pallas as changing the course of the javelin after it had left the hand of Diomed. But those states of the nervous system which result in a certain and accurate aim or in a tremulous or uncontrolled arm, lay beyond the pale of physiological knowledge in the time of Homer; so here it was that the goddess intervened. When nervous action became fully understood, the final cause receded, and took refuge in some deeper arcanum of our ignorance. Jove was never expected to make thunder and rain without clouds, nor was the falling of the rain ever ascribed to his interference, because every one believed that if the drops were once formed they would fall at once to the ground, without any action on his part. But the mixing currents of moist and cool air, and the processes of condensation which lead to the formation of rain and electricity, were not understood, so here Jupiter had a chance to work unseen by man. When the mode in which clouds were formed was once understood, the god of thunder left his seat upon Mount Olympus for a more distant abode. From the earliest historic times, the man who took a large dose of poison has died, as a matter of course; neither good nor evil spirit had anything to do with it; but if brain disease bereft him of reason, the malevolence of an evil spirit was called in to account for the result.

Now, I beg you to notice that in all these cases, the only distinction we can make between those effects which were supposed to be produced by natural causes and those which were produced by the will of some higher power, acting with a scrutable end in view, is this: in the first class

of cases we can clearly see the effect to have been produced by the action of natural causes, and in the second we cannot. This distinction, depending as it does upon the extent of our knowledge, cannot be regarded as a logical one. Yet, in so far as a belief in that class of final causes which we have been considering exists at the present day, I see no other definition of the limits within which these causes are supposed to act. Let us take an illustration from the plague now desolating our southern cities. No one would believe that under any circumstances any superior power would build a yellow fever hospital and supply it with the best medicines. If we should say that the prayers of the whole nation for the immediate erection of such buildings would have no effect whatever, we should not be accused of unbelief or irreverence in any quater, for everyone would fully agree with us. But there are great numbers of people who believe that, if the whole nation should pray for frost, frost might be sent in answer to prayer when it would not have come otherwise. And to many who do not share this belief, the denial of any possibility of an influence of this kind would seem to savour much more strongly of unbelief, irreligion, or irreverence, than the denial that Providence would build a hospital without human hands. And yet, if the scientific philosophy be correct, the providential production of frost would be as miraculous and as incredible as the providential erection of a hospital in a single night without human hands. The temperature of the air, and the amount of moisture, it shall have in any given place, a day, or month, or year from the present time, is as completely fixed by the present state of things, and by the laws of evaporation, condensation, and motion of gases, as is the position of the heavenly bodies. The first deposition of frost will be determined by forces now at play, and any deviation from the inevitable action would be a miracle of the same kind as pieces of timber hewing themselves into shape, and putting themselves together, untouched by man. Please notice that this similarity between the two states of things is entirely independent of any philosophical theory of natural causes. All we claim is that the laws which determine the motion of the air, the formation of clouds, the fall of rain, and the deposition of frost, are, with respect to their certainty of action, of the same class with those which determine the position, the movements, and the cohesion of a stick of timber. If you claim that both classes of causes are the acts of the Creator, we have nothing to say against it; all we say is that you must interpret his acts in the

same way in the two cases. You must not claim that he will produce heat or cold by a fiat of an arbitrary will, unless you also claim that He will build the hospital or leave it unbuilt according to a similar fiat. Nor is it of any avail to say that you know it to be His will that the hospital shall remain unbuilt unless man undertakes it. We can, in reply, maintain that we know it to be His will that the course of nature shall go on unchanged, no matter how it may affect human interests.

It thus appears that the dividing line between mechanical and final causes, as drawn by the human mind in all ages, has not been fixed by any absolute criterion, but only near the limits of the knowledge possessed by each generation. Science has extended the line entirely beyond ordinary mental vision, not by introducing any new theory of nature, but by extending the boundaries of exact knowledge, and with them, of the field in which, by common consent, final causes do not admit of being traced. The telescope has revealed to us a universe compared with which that known to ancients is but an atom, and geology has opened up to our view a vista of ages in which the lifetime of our generation is hardly more than a moment. And thus final causes have taken their flight from a vast region in which they before lay hid in obscurity. You may now ask, have they simply taken refuge in the more distant but vastly wider circumference which now marks the boundaries of our knowledge, or are we to suppose them entirely banished from nature? This is entirely a question of intuition, and not at all of scientific investigation. I have described the scientific theory of nature as not admitting scrutable final causes at all, but as claiming that the law of the falling rock is symbolic of all her operations. But I think this is a view towards which philosophers have always inclined. We must always expect that men will incline to this view in proportion to their familiarity with the material side of nature. At the same time it is evident to all that there must have been a beginning of things, and that nature could not have commenced herself. We have, therefore, a wide belt left between the origin of nature and the boundaries of our knowledge in which we may suppose the inscrutable cause to have acted. Here we reach questions of philosophy which lie outside of our field, and which, therefore, we cannot now stop to consider.

The exact bearing of the subject will be better understood by condensing what has already been said so as to present the whole in a brief space.

1. When men study the operations of the world around them, they find that certain of those operations are determined by knowable antecedent conditions, and go on with that blind disregard of consequences which they call law. The criterion for distinguishing these operations is that their results admit of being foreseen. They also find certain other operations which they are unable thus to trace to the operation of law.

2. Men attribute this latter class to invisible anthropomorphic intelligences, having the power to bring about changes in nature, and having certain objects, worthy or ignoble, in view, which they thus endeavour to compass. Men also believe themselves able to discern these objects, and thus to explain the operations which bring them about. The objects are worthy or ignoble according to the character of the intelligences, which again depend upon the state of society. In ancient times they were often the gratification of the silliest pride or the lowest lusts.

3. As knowledge advances, one after another of these operations are found to be really determined by law, the only difficulty being that the law was before unknown or not comprehended, or that the circumstances which determined its action were too obscure or too complex to be fully comprehended.

4. Final causes having thus, one by one, disappeared from every thicket which has been fully explored, the question arises whether they now have, or ever had, any existence at all. On the one hand it may be claimed that it is unphilosophical to believe in them when they have been sought in vain in every corner into which light can penetrate; on the other hand, we have the difficulty of accounting for these very laws by which we find the course of nature to be determined. Take, as a single example, the law of hereditary descent. How did such a law—or rather how did such a process, for it is a process—first commence? If this is not as legitimate a subject for enquiry as the question how came the hand and the eye into existence, it is only because it seems more difficult to investigate. If, as the most advanced scientific philosophy teaches, creation is itself but a growth, how did that growth originate? We here reach the limits of the scientific field, on ground where they are less well-defined than in some other directions, but I shall take the liberty of concluding my remarks with a single suggestion respecting a matter which lies outside of them. When the doctrine of the universality of natural law is carried so far as to include the genesis of living beings, and the adaptations

to external circumstances which we see in their structure, it is often pronounced to be atheistic. Whether this judgment is or is not correct I cannot say; but it is very easy to propound the test question by which its correctness is to be determined. Is the general doctrine of causes acting in apparently blind obedience to invariable law in itself atheistic? If it is, then the whole progress of our knowledge of nature has been in this direction, for it has consisted in reducing the operations of nature to such blind obedience. Of course, when I say blind you understand that I mean blind so far as a scrutable regard to consequence is concerned—blind like justice, in fact. If the doctrine is not atheistic, then there is nothing atheistic in any phase of the theory of evolution, for this consists solely in accounting for certain processes by natural laws. I do not pretend to answer the question here involved, because it belongs entirely to the domain of theology. All we can ask is that each individual shall hold consistent views on the subject, and not maintain the affirmative of the question on one topic and the negative on another. My object in presenting the views I have has been not so much to propound a new theory as to promote consistency, precision, and independence of thought among those who discuss the subject.

V. PERUVIAN ANTIQUITIES.*

By E. R. Heath, M.D., Wyandotte, Kas.

On the Peruvian coast in ancient times, as now, nearly every structure was made of adobes or sun-dried brick, while in the mountains stone was used instead. The adobe ruins present nothing of beauty, architecturally. The subject for wonder is their immensity and number. Go where you will relics of the past meet your eye, either in ruined walls, water-courses, terraces, or extensive lands covered with the *débris* of pottery.

Take, for example, the Jequetepeque valley. In 7° 24′ S. latitude you will find on recent maps the port of Pacasmayo. Four miles north, separated from it by a barren waste, the

* Kansas City Review of Science and Industry, November, 1878.

river Jequetepeque empties into the sea. The bottom lands of the river are from 2 to 3 miles in width, with a southern sloping bank, and the northern a perpendicular one nearly 80 feet high. Beside the southern shore, as it empties into the sea, is an elevated plat one-fourth of a mile square and 40 feet high, all of adobes. A wall 50 feet wide connects it with another, a few hundred yards east and south,—that is, 150 feet high, 200 feet across the top, and 500 at the base, nearly square. This latter was built in sections of rooms 10 feet square at the base, 6 feet at the top, and about 8 feet high. These rooms were afterward filled with adobes, then plastered on the outside with mud, and washed in colours. All of this same class of mounds—temples, to worship the sun, or fortresses, as they may be—have on the north side an incline for an entrance or means of access. Treasure-seekers have cut into this one about half-way, and it is said 150,000 dollars worth of gold and silver ornaments were found. In the sand, banked up behind the wall and mound, many were buried, as the thousands of skulls and bones now exposed prove; thrown out by the hunter of huacos, as the pottery is called, huaca being the name given to these cemeteries. Each body has buried with it a vessel or water-craft, and a pot with grains of corn or wheat, and it is supposed the drinking-vessel was filled with "chicha," a fermented drink made from corn or pea-nuts. Beside these were many ornaments of gold, silver, copper, coral and shell beads, and cloths. On the north side of the river, on the top of the bluff, are the extensive ruins of a walled city, 2 miles wide by 6 long. Within the enclosure are the relics of two large reservoirs for fresh water. The clay from which these adobes were made was found at least 6 miles distant.

Follow the river to the mountains. All along you pass ruin after ruin and huaca after huaca. At Tolon, a town at the base of the mountains, the valley is crossed by walls of boulders and cobble stones, 10, 8, and 6 feet high, 1 foot to 18 inches wide at the top, and 2 to 3 feet at the base, enclosing ruins of a city one-fourth of a mile wide and more than a mile long. The upper wall has projecting parts at the entrances, with port-holes, evidently serving as sentry-boxes. At this point the Pacasmayo Railroad enters the Jequetepeque valley. For 8 miles back it crosses a barren sand plain of more than 15 miles in length, covered with ruined walls, water-courses, dead algaroba and espino trees, with fragments of pottery and sea-shells, even to 9 feet in depth mixed with the sand. The base of the mountains

have, in good state of preservation, many thousand feet of an old water-course, while their sides to the perpendicular parts are lined with terraces. This water-course took its head from a ravine now dry, and, even beyond the memory of the oldest inhabitants, except in one or two cases, never carried water. It can be traced as far as Ascope, 45 miles south. Five miles from Tolon, up the river, there is an isolated boulder of granite, 4 and 6 feet in its diameters, covered with hieroglyphics. Fourteen miles further, a point of mountain at the junction of two ravines is covered to a height of more than 50 feet with the same class of hieroglyphics—birds, fishes, snakes, cats, monkeys, men, sun, moon, and many odd and now unintelligible forms. The rock on which these are cut is a silicated sandstone, and many of the lines are an eighth of an inch deep. In one large stone there are three holes, 20 to 30 inches deep, 6 inches in diameter at the orifice and 2 at the apex, and, although polished as porcelain, these markings extend even to the bottom. The locality is of no importance; the stones as Nature placed them; why, then, was so much labour and time expended upon them?

At Anchi, on the Rimac river, upon the face of a perpendicular wall 200 feet above the river-bed, there are two hieroglyphics, representing an imperfect B and a perfect D. In a crevice below them, near the river, were found buried 25,000 dollars worth of gold and silver. When the Incas learned of the murder of their chief, what did they do with the gold they were bringing for his ransom? Rumour says they buried it, and many places are pointed out and thousands of dollars spent in useless search for the lost treasure. May not these markings at Yonan tell something, since they are on the road and near to the Incal city? Eleven miles beyond Yonan, on a ridge of mountain 700 feet above the river, are the walls of a city of 2000 inhabitants. A perilous ascent on hands and knees is now the only way to reach it; however, on the opposite side of the river are similar ruins, but easy of access. A remnant of a stone wall, 10 and 12 feet high, built of small flat stones and without mortar, probably at one time served as river protection and against the tribe on the other side, there being a tradition that two powerful chiefs occupied these cities and were ever at war. The dead were buried in sepulchres, using large boulders as the top, while stone walls divided the space beneath into compartments. Six and twelve miles further are extensive walls and terraces. Three miles north of the latter place are the rich silver mines of Chilete, for-

merly worked by the Indians, who left excavations 200 and 300 feet deep, and must have taken out quantities of silver. A company with a paid-up capital of half a million is now working them.

Leaving the valley at 78 miles from the coast, you zigzag up the mountain side 7000 feet, then descend 2000, to arrive at Cajamarca, or Coxamalca of Pizarro's time. Here and there all the way you find relics of the past. In a yard off one of the main streets, and near the centre of the city, is still standing the house made famous as the prison of Atahualpa, and which he promised to fill with gold as high as he could reach, in exchange for his liberty. Like all their stonework, the walls are slightly inclined inward, uncemented, built of irregular stones, each exactly faced to fit the next. The floor and porch are cut out of the solid stone, 2 and 3 feet deep, as the still intact remnants of stone pillars of the same rock show. The hill from which the stone for the walls was taken is near by. On its top a large stone in the shape of a chair bears the name of "Inca's chair," and the Indians say it was the king's custom to sit here every morning and salute the sun as it rose above the horizon. The two large places excavated out of the rock on the hill-side, and now used as reservoirs for the city, were of ancient make. Three miles distant, and across the valley, are the hot springs, where the Inca was encamped when Pizarro took possession of Cajamarca. Part of the wall is of unknown make (that encloses the baths). Cemented, the cement is harder than the stone itself. At Chocofan, 9 miles from Pacasmayo, on the line of the railroad, a barren, rocky mountain, 1200 feet high, is encircled 400 feet from its top by a stone wall 8 or 10 feet high. From its northern side, running nearly north-west, is about 5 miles of the coast road of the Incas. Perfectly straight, it is 20 feet wide, and walled on both sides by round stones piled to a height of 3 and 4 feet, 3 feet wide at the base and 2 at the top, uncemented. At Chepen, a station near the terminus of the branch of the Pacasmayo Railroad, is a mountain with a wall in many places 20 feet high, the summit being almost entirely artificial. In the sand at its base is one of Peru's most extensive "huacas," and from which some of the finest pottery and ornaments have been taken.

Fifty miles south of Pacasmayo, between the seaport of Huanchaco and Truxillo, 9 miles distant, are the ruins of "Chan Chan," the capital city of the Chimoa kingdom, which extended, when conquered by the Incas, from Supe

to Tumbez, or over nearly the northern half of the coast of modern Peru. The road from the port to the city crosses these ruins, entering by a causeway about 4 feet from the ground, and leading from one great mass of ruins to another; beneath this is a tunnel. Be they forts, castles, palaces, or burial mounds called "huacas," all bear the name "huaca." Hours of wandering on horseback among these ruins give only a confused idea of them, nor can old explorers there point out what were palaces and what were not.

To the right is the "Huaca of Toledo," to the left "Bishop's Huaca." The large square enclosures, shut in by wedge-shaped walls of adobe, 20 to 25 feet high, have nothing of an entrance into them that would be defined as a palace-gate. A half-a-dozen of these, at least, are among the ruins. Within some of them are large square mounds or burying chambers, many of which have been opened and rifled of their contents. These are plastered at the ceilings. Beside the so-called "huacas" already mentioned, there is another on the left side of the road called by the Spaniards "the Mass." On many of the walls is some excellent stucco-work. Excellent as regards the material of which it is made, more than with reference to its style of art. There is not a single grain of disintegration in the parts that surround the walls of the chamber, although it is half an inch high above the ordinary plaster in which it is done, nor the slightest impairment in its integrity during the many centuries it has stood exposed to the elements. The highest enclosures—those of adobe brick, up to 30 feet, with a base of 15 feet, on the right hand of the city as you advance toward Truxillo, between that town and the "Toledo huaca"—must have cost an immense amount of labour, and needed a large number of hands for their erection. Inside some of them, besides the square mounds, are narrow passages, not more than a yard in width. In others are squares, wherein are visible, though now filled with clay, the outlines of water-tracks. On this side are the principal burial mounds, some having stairs of adobe.

In the city of Truxillo there exists, in the records of the municipality, a copy of the accounts that are found in the book of Fifths of the Treasury, in the years 1577 and 1578, referring to the "Huaca of Toledo." The following is a condensed inventory:—

First.—In Truxillo, Peru, on the 22nd of July, 1577, Don Garcia Gutierrez de Toledo presented himself at the royal treasury, to give into the royal chest a fifth. He brought a bar of gold 19 carats ley and weighing 2400 Spanish dollars,

of which the fifth, being 708 dollars, together with $1\frac{1}{2}$ per cent to the chief assayer, were deposited in the royal box.

Second.—On the 12th of December he presented himself with five bars of gold, 15 and 19 carats ley, weighing 8918 dollars.

Third.—On the 7th of January, 1578, he came with his fifth of large bars and plates of gold, one hundred and fifteen in number, 15 to 20 carats ley, weighing 153,280 dollars.

Fourth.—On the 8th of March he brought sixteen bars of gold, 14 to 21 carats ley, weighing 21,118 dollars.

Fifth.—On the 5th of April he brought different ornaments of gold, being little bells of gold and patterns of corn-heads and other things, of 14 carats ley, weighing 6272 dollars.

Sixth.—On the 20th of April he brought three small bars of gold, 20 carats ley, weighing 4170 dollars.

Seventh.—On the 12th of July he came with forty-seven bars, 14 to 21 carats ley, weighing 77,312 dollars.

Eighth.—On the same day he came back with another portion of gold and ornaments of corn-heads and pieces of effigies of animals, weighing 4704 dollars.

The sum of these eight bringings amounted to 278,174 gold dollars or Spanish ounces. Multiplied by sixteen gives 4,450,784 silver dollars. Deducting the royal fifth—985,953·75 dollars—left 3,464,830·25 dollars as Toledo's portion.

Even after this great haul, effigies of different animals of gold were found from time to time. Mantles also, adorned with square pieces of gold, as well as robes made with feathers of divers colours, were dug up. There is a tradition that in the huaca of Toledo there were two treasures, known as the great and little fish. The smaller only has been found.

Between Huacho and Supe, the latter being 120 miles north of Callao, near a point called Atahuanqui, there are two enormous mounds, resembling the Campana and San Miguel, of the Huatica Valley, soon to be described. About 5 miles from Patavilca (south, and near Supe) is a place called "Paramonga," or the fortress. The ruins of a fortress of great extent are here visible; the walls are of tempered clay, about 6 ft. thick. The principal building stood on an eminence, but the walls were continued to the foot of it, like regular circumvallations; the ascent winding round the hill like a labyrinth, having many angles, which probably served as outworks to defend the place. In this neighbourhood much treasure has been excavated, all of which must have been concealed by the prehistoric Indian, as we have

no evidence of the Incas ever having occupied this part of Peru after they had subdued it.

From Lima, north, along the coast, the Ancon and Chancay Railroad is built. Ancon, 18 miles from Lima, is a favourite summer sea-side resort. Just before reaching Ancon, the railroad runs through an immense burying-ground, or "huaca." Make a circuit of 6 to 8 miles, and on every side you see skulls, legs, arms, and the whole skeleton of the human body lying about in the sand. Legs attached to pelvis, and bent up, still with mummified skin on them; arms in the same state; relics of plaited straw, forming coffin swathes; pieces of net, of cloth, and many otner such accompaniments of funeral accessories. Some water crafts of very superior quality have been obtained from these graves. Of these there are three different forms, in places separated a short distance from each other, but each style having its defined outline of *locale*. As to the shape of the graves, there are some of an inverted cylinder form, like that of a lime-kiln, the insides of which are lined with masonry work. In these the body is placed in the upright position. There is also the ordinary longitudinal grave, in which the corpse is right in contact with the earth. Likewise the grave cut square to a depth of 6 to 8 ft., at the top of which, or within 1 or 2 ft. of the surface of the ground, is a roofing or covering of mat-work, placed on wooden rafters. In one of these Dr. Hutchinson, her Britannic Majesty's Consul at Callao, found three bodies, all wrapped up together—being a man, woman, and child—their faces being swathed with llama wool instead of cotton, as is usually seen in ordinary ones. He also turned out relics of fishing nets, with some needles for making them, varieties of cloth, tapestry, and work-bags, resembling ladies' reticules. Not a vestige of vegetation about, nor sign of relic of the terraces mentioned by Prescott. Whence came these hundreds and thousands of people, who are buried at Ancon? How did they make out a living while on the earth? Time and time again the archæologist finps himself face to face with such questions, to which he can only shrug his shoulders and say with the natives, "Quien sabe?" Who knows?

At Parmayo, 14 miles further "down north," and on the sea shore, is another great burying-ground. Thousands of skeletons lie about, thrown out by the treasure-seekers. It has more than a half-mile of cutting through it for the Ancon and Chancay R. R. It extends up the face of the hill from the sea shore to the height of about 800 ft., and being from a half to three-fourths of a mile in breadth, some idea may be formed of its extent.

Dr. Hutchinson, in two days, from these burial grounds gathered 384 skulls, which, with specimens of pottery, he presented to Professor Agassiz, and he to the Cambridge University, near Boston.

Between the teeth he found pieces of copper, as if for the Charon obolus, and one or two had plates of copper on their heads.

Crossing the brow of the hill, entering Chancay, and stretching towards the sea, are the remains of a 6 feet adobe wall. On the face of this hill, pointing to the line of the railway from Ancon, are two stone ditches, perfectly parallel and symmetrical, about 100 yards apart, and running from bottom to top to a height of about 300 yards. Between these are other lines of stones displaced, perhaps the ruins of some old terraces. All about this place, at the base of the hill, looking towards Chancay, as well as on the side in front of the sea, is full of graves; some are built up with stone walls, others lined inside with mud-bricks, of no formation more than a heap of clay and water moulded up in the hands and dried in the sun. Over the hills of Chancay are quantities of small stones of different geological formation from the rock there.

Dr. Hutchinson writes, under date of October 30, 1872, in an article to the Callao and Lima "Gazette," now the South Pacific "Times:" "I am come to the conclusion that Chancay is a great city of the dead, or has been an immense ossuary of Peru; for go where you will, on mountain top or level plain, or by the sea side, you meet at every turn skulls and bones of all descriptions."

Lima, the capital of Peru, is situated seven miles inland from Callao. Nine miles on the sea shore "up south," is the city of Chorillos, the Long Branch of Peru. A railway connects Lima with these two cities, forming with the coast nearly a right angled triangle. This triangular ground is known as the Huatica Valley, and is an extensive ruin. Between Callao and Magdalena, four miles distant, there are seventeen mounds called "huacas," although they present more the form of fortresses, residences, or castles, than burying ground. It is difficult to make out anything but fragments of walls, as the ground is mostly under cultivation. However, at various points, one can see that a triple wall surrounded the ancient city. These walls are respectively one yard, two yards, and three yards in thickness, being in some parts of their relics from fifteen to twenty feet high. To the east of these is the enormous mound called Huaca of Pando; and to the west, with the distance of about half

a mile intervening, are the great ruins of fortresses, which natives entitle Huaca of the Bell. La Campana, the huacas of Pando, consisting of a series of large and small mounds, and extending over a stretch of ground incalculable without being measured, form a colossal accumulation. The principal large ones are three in number; that holding the name of the "Bell" is calculated to be 108 to 110 feet in height. At the western side, looking towards Callao, there is a square plateau with an elevation of about 22 to 24 feet, 95 to 96 yards north and south, east and west. At the summit it is 276 to 278 yards long, and 95 to 96 across. On the top there are eight gradations of declivity, each from one to two yards lower than its neighbour; counting in direction lengthwise, the first plateau is 96 to 97 yards; second plateau, 96 to 26 yards; third plateau, 23 to 24 yards; fourth plateau, 11 to 12 yards; fifth plateau, 11 to 25 yards; sixth plateau, 23 to 24 yards; seventh plateau, 35 to 36 yards; eighth plateau, 35 to 37 yards; making a total of about 278 yards. For these measurements of the Huatica ruins I am indebted to the notes of J. B. Steere, Professor of Natural History and Curator of the Museum at Ann Arbor, Michigan.

The square plateau first mentioned, at the base, consists of two divisions, one 6 feet lower than the other, but each measuring a perfect square 47 to 48 yards; the two joining form the square of 96 yards. Beside this, and a little forward on the western side, is another square of 47 to 48 yards. On the top, returning again, we find the same symmetry of measurement in the multiples of twelve, nearly all the ruins in this valley being the same, which is a fact for the curious. Was it by accident or design? In its breadth from north to south three levels are found. The first lower down, 17 to 18 yards wide; the second or highest summit, 59 to 60 yards across; and the third descent again, 23 to 24 yards. Themound is a truncated pyramidal form, and is calculated to contain a mass of 14,641,820 cubic feet of material. For the most part, this great work is composed of adobes, each 6 inches long, 4 inches wide, and 2½ thick, many having the marks of fingers on them. But this does not consist of more than one-third of the Pando huaca.

Walking down past the southern corner, where the adobes are tumbled into a conglomerate mass by some earthquake, we see skulls with bones of arms and legs, cropping up in many places. The same adobe work is visible throughout, and the whole length of these structures range between 700 and 800 yards. The "Fortress" is a

huge structure, 80 feet high, 148 to 150 yards in measurement. Great large square rooms show their outlines on the top, but are filled with earth. Who brought this earth here, and with what object was the filling up accomplished? The work of obliterating all space in these rooms with loose earth must have been almost as great as the construction of the building itself. About two miles south of the last-named fort, and in a parallel line with it as regards the sea, we find another similar structure, probably a little more spacious and with a greater number of apartments, or divisions by walls, on the top of which we can now walk, as it is likewise filled up with clay. This is called "San Miguel." It is nearly 170 yards in length, and 168 in breadth, and 98 feet high. The whole of these ruins, big fortress, small forts, and temples were enclosed by high walls of adobones, but all of wedge-shaped form, with the sharp edge upward. Adobones are large mud bricks, some from 1 to 2 yards in thickness, length, and breadth. The huaca of the "Bell" contains about 20,220,840 cubic feet of material, while that of "San Miguel" has 25,650,800. These two buildings were constructed in the same style—having traces of terraces, parapets, and bastions, with a large number of rooms and squares—all now filled up with earth.

Near Lima, on the south, is another mound, 70 feet high and 153 yards square. Near the residence of Par Soldan, the Geographer of Peru, is a mound called "Sugar Loaf," or "San Isidro," 66 feet high, 80 yards broad at the base, and 130 yards long. Professor Raimondi, the naturalist, chemist, and scientist, who is doing for Peru what Gay did for Chili, said he found nothing in it but bodies of ordinary fishermen, relics of nets, and some inferior specimens o pottery.

Prof. Steere and Dr. Hutchinson turned out about forty skulls, some bits of red and yellow dyed thread, being relics of cloth; a piece of string made of woman's hair, plaited, about the size of what is generally used for a watch-guard; and pieces of very thick cotton cloth, bits of fish-nets, portions of slings, and two specimens of crockery-ware of excellent material.

About a mile beyond, in the direction of "Mira Flores," is Ocharan, the largest burial mound in the Huatica valley. This mound presents, as it is approached, the appearance of an imposing and enormous structure. It has 95 feet of elevation in its highest part, with an average width of 55 yards on the summit, and a total length of 428 yards, or

1284 feet, another multiple of twelve. It is enclosed by a double wall 816 yards in length by 700 across, thus enclosing 117 acres. Between Ocharan and the ocean are from 15 to 20 masses of ruins, like those already described.

Fifteen miles south of Lima, in the valley of Lurin, and near the sea, are the ruins of Pacha Camac, the Inca temple of the sun. Like the temple of Cholula on the plains of Mexico, it is a sort of made mountain or vast terraced pyramid of earth. It is between 200 and 300 feet high, and forms a semi-lunar shape that is beyond half a mile in extent. Its top measures about 10 acres square. Much of the walls are washed over with red paint, probably ochre, and are as fresh and bright as when centuries ago it was first put on. In these walls, in three or four places, are niches, apparently of the same shape and size as we see in the ruins of Pagan temples. From one side, going towards the north, are the relics of a wall, which is covered with soot, possibly the remnant of fires to make sacrifices, and nothing can better illustrate the conservative tendency of the Peruvian climate than the fresh appearance of the soot. Prescott says of Pacha Camac that it was to the Peruvians what Mecca is to the Mahometan, and Cholula was to the Mexican.

In the Canete valley, opposite the Chincha Guano Islands, are extensive ruins. In that region a terra-cotta mask was found, similar to that of which there is a drawing in Mr. Squiers's report of his explorations in the State of New York, and discovered while excavating for the St. Lawrence canal. From the hill called "Hill of Gold" copper and silver pins were taken like those used by ladies to pin their shawls; also, tweezers for pulling out the hair of the eyebrows, eyelids, and whiskers, as well as silver cups.

Buried 62 feet under the ground on the Chincha Islands, stone idols and water-pots were found, while 35 and 33 feet below the surface were wooden idols. Beneath the Guano on the Guanapi Islands, just south of Truxillo, and Macabi just north, mummies, birds, and birds's eggs, gold and silver ornaments were taken. On the Macabi the labourers found some large valuable golden vases, which they broke up and divided among themselves, even though offered weight for weight in gold coin, and thus have relics of greatest interest to the scientist been for ever lost. He who can determine the centuries necessary to deposit thirty and sixty feet of guano on these islands, remembering that since the conquest, three hundred years ago, no appreciable

increase in depth has been noted, can give you an idea of the antiquity of these relics.

The coast of Peru extends from Tumbez on the north to the river Loa on the south, a distance of 1233 miles. Scattered here and there over this whole extent, there are thousands of ruins besides those just mentioned, and similar, only not so extensive; while nearly every hill and spur of the mountains have upon them or about them some relic of the past; and in every ravine, from the coast to the central plateau, there are ruins of walls, fortresses, cities, burial vaults, and miles and miles of terraces and water courses. Across the plateau and down the eastern slope of the Andes to the home of the wild Indian, and into the unknown, impenetrable forest, still you find them. In 1861, Mendoza, in the Argentine Republic, a beautiful city on the plain, forty-five miles from the foot of the Andes, in the short space of five minutes was a complete ruin, and 15,000 out of her 20,000 inhabitants, or 75 per cent, were in the arms of death. In 1871 it was still exactly as on the evening of her destruction; the miles of skeletons lying uncovered where they perished, and the streets yet obstructed with the *débris* of the fallen walls of the houses. A new city has been built beside the old one. Seeking a photograph of the ruins, I was told there were none. Persuading one of the artists to take some views of them, and going to see the proof, he told me he had been out all day and had done nothing, as he could find nothing to take "but a pile of dirt." Thus, also, you might, as most do, style these coast ruins, and those who live among them understand and appreciate them no better than did the Mendoza artist the ruins of that ill-fated city.

In the mountains, however, where storms of rain and snow with terrific thunder and lightning are nearly constant a number of months each year, the ruins are different. Of granite, porphyritic, lime, and silicated sand-stone, these massive, colossal, cyclopean structures have resisted the disintegration of time, geological transformations, earthquakes, and the sacrilegious, destructive hand of the warrior and treasure-seeker. The masonry composing these walls, temples, houses, towers, fortresses, or sepulchres, is uncemented, held in place by the incline of the walls from the perpendicular, and adaptation of each stone to the place destined for it, the stones having from six to many sides, each dressed, and smoothed to fit another or others with such exactness that the blade of a small penknife cannot be inserted in any of the seams thus formed, whether in the

central parts entirely hidden, or on the internal or external surfaces. These stones, selected with no reference to uniformity in shape or size, vary from one-half cubic foot to 1500 cubic feet solid contents, and if, in the *many, many millions* of stones you could find *one* that would fit in the place of another, it would be purely accidental. In "Triumph street," in the city of Cuzco, in a part of the wall of the ancient house of the virgins of the sun, is a very large stone, known as "the stone of the twelve corners," since it joined with those that surround it, by twelve faces, each having a different angle. Besides these twelve faces it has its internal one, and no one knows how many it has on its back that is hidden in the masonry. In the wall in the centre of the Cuzco fortress there are stones 13 feet high, 15 feet long, and 8 feet thick, and all have been quarried miles away. Near this city there is an oblong smooth boulder 18 feet in its longer axis, and 12 in its lesser. On one side are large niches cut out, in which a man can stand, and by swaying his body cause the stone to rock. These niches apparently were made solely for this purpose. One of the most wonderful and extensive of these works in stone is that called Ollantay-Tambo, a ruin situated thirty miles north of Cuzco, in a narrow ravine on the bank of the river Urubamba. It consists of a fortress constructed on the top of a sloping, craggy eminence. Extending from it to the plain below is a stone stairway. At the top of the stairway are six large slabs, 12 feet high, 5 feet wide, and 3 feet thick, side by side, having between them and on top narrow strips of stone about 6 inches side, frames as it were to the slabs, and all being of dressed stone. At the bottom of the hill, part of which was made by hand, and at the foot of the stairs, a stone wall 10 feet wide and 12 feet high extends some distance into the plain. In it are many niches, all facing the south.

The ruins on the islands in Lake Titicaca, where Incal history begins, have often been described.

At Tiahuanaco, a few miles south of the lake, there are stones in the form of columns, partly dressed, placed in line at certain distances from each other, and having an elevation above the ground of from 18 to 20 feet. In this same line there is a monolithic doorway, now broken, 10 feet high by 13 wide. The space cut out for the door is 7 feet 4 inches high by 3 feet 2 inches wide. The whole face of the stone above the door is engraved. Another, similar, but smaller, lies on the ground beside it. These stones are of hard porphyry, and differ geologically from the surrounding

rock; hence we infer they must have been brought from elsewhere.

At "Chavin de Huanta," a town in the province of Huari, there are some ruins worthy of note. The entrance to them is by an alley-way 6 feet wide and 9 feet high, roofed over with sandstone partly dressed, of more than 12 feet in length. On each side there are rooms 12 feet wide, roofed by large pieces of sandstone 1½ feet thick and from 6 to 9 feet wide. The walls of the rooms are 6 feet thick, and have some loopholes in them, probably for ventilation. In the floor of this passage there is a very narrow entrance to a subterranean passage that passes beneath the river to the other side. From this many huacos, stone drinking-vessels, instruments of copper and silver, and a skeleton of an Indian sitting, were taken. The greater part of these ruins were situated over aqueducts. The bridge to these castles is made of three stones of dressed granite, 24 feet long, 2 feet wide by 1½ thick. Some of the granite stones are covered with hieroglyphics.

At Corralones, 24 miles from Arequipa, there are hieroglyphics engraved on masses of granite, which appear as if painted with chalk. There are figures of men, llamas, circles, parallelograms, letters, as an R and an O, and even remains of a system of astronomy.

At Huaytar, in the province of Castro Virreina, there is an edifice with the same engravings.

At Nazca, in the province of Ica, there are some wonderful ruins of aqueducts, 4 to 5 feet high and 3 feet wide, very straight, double-walled, of unfinished stone, flagged on top.

At Quelap, not far from Chochapayas, there have lately been examined some extensive works. A wall of dressed stone, 560 feet wide, 3660 long, and 150 feet high. The lower part is solid. Another wall above this has 600 feet length, 500 width, and the same elevation of 150 feet. There are niches over both walls, 3 feet long, 1½ wide and thick, containing the remains of those ancient inhabitants, some naked, others enveloped in shawls of cotton of distinct colours and well embroidered. Their legs were doubled so that the knees touched the chin, and the arms were wound about the legs. The wall has three uncovered doors, the right side of each being semicircular, and the left angular. From the base an inclined plane ascends almost insensibly the 150 feet of elevation, having about midway a species of sentry-box in stone. In the upper part there is an ingenious hiding-place of dressed stone, having upon it

a place for an outlook from which a great portion of the province can be seen. Following the entrances of the second and highest wall, there are other sepulchres like small ovens, 6 feet high and 24 in circumference: in their base are flags, upon which some cadavers reposed. On the north side there is, on the perpendicular rocky side of the mountain, a brick wall, having small windows 600 feet from the bottom. No reason for this, nor means of approach, can now be found. The skilful construction of utensils of gold and silver that were found here, the ingenuity and solidity of this gigantic work of dressed stone, make it also probably of pre-Incal date.

To support the inhabitants it became necessary to cultivate every part of the land possible; and since the greater portion is mountainous, they could make no use of that land except by such means as they adopted, *i.e.*, by terraces. Along the side, at the base of a hill or mountain, a stone wall is laid, from 1 to 8 feet high, according to the slope, and earth filled in between it and the side of the mountain, till even with the wall. Having this level for a base, another wall is laid, and again earth filled in, and so on, tier above tier, as high as the place will permit. These are terraces. The summits of the mountains are saturated with water from the melting snow or winter rains. This, forming little streams, is guided over these terraces. Each terrace is divided into patches by making a little ridge of earth a few inches high all around them, enclosing places 2 feet by 6, or 8 feet by 10, and so on according to the size of the terrace. The top terrace is first flooded, the ridge of earth serving as a dam. When it is considered wet enough, a channel is made by taking out a part of the ridge (with the hand, or a little paddle about the size of a pancake turner), permitting the water to escape to the part below, flowing over the wall to the next terrace, which is similarly treated. But there are thousands of terraces where the mountains and hills are so low and near the rainless portion that snow never, and rain very seldom, moistens their summits, and where no one could expect water for irrigation unless carried there by hand. Starvation alone would compel people to undertake so fatiguing and laborious a work, especially in a country where the evenness of the climate tends to relax the energy of both mind and body. Estimating five hundred ravines in the 1200 miles of Peru, and 10 miles of terraces of fifty tiers to each ravine, which would only be 5 miles of twenty-five tiers to each side, we have 250,000 miles of stone wall, averaging 3 to 4 feet high—enough to encircle this

globe ten times. Surprising as these estimates may seem, I am fully convinced that an actual measurement would more than double them, for these ravines vary from 30 to 100 miles in length, and 10 miles to each is a low estimate. While at San Mateo, a town in the valley of the River Rimac, 77 miles from the coast, where the mountains rise to a height of 1500 or 2000 feet above the river bed, I counted two hundred tiers, none of which were less than four and many more than six miles long. Even at four miles there would be at that point alone 800 miles of stone wall, and that only on one side of the ravine.

Who, then, were these people, cutting through 60 miles of granite, transplanting blocks of hard porphyry, of Baalbic dimensions, miles from the place where quarried, across valleys thousands of feet deep, over mountains, along plains, leaving no trace of how or where they carried them; people ignorant of the use of iron, with the feeble llama their only beast of burden; who, after having brought these stones together and dressed them, fitted them into walls with mosaic precision; terracing thousands of miles of mountain side; building hills of adobes and earth, and huge cities; leaving works in clay, stone, copper, silver, gold, and embroidery, many of which cannot be duplicated at the present age; people apparently vying with Dives in riches, Hercules in strength and energy, and the ant and bee in industry?

Callao was submerged in 1746, and entirely destroyed. Lima was ruined in 1678; in 1746 only twenty houses out of three thousand were left standing, and again injured in 1764, 1822, and 1828, while the ancient cities in the Huatica and Lurin valleys still remain in a comparatively good state of preservation. San Miguel de Piura, founded by Pizarro in 1531, was entirely destroyed in 1855, while the old ruins near by suffered little. Arequipa was thrown down in August, 1868, but the ruins near show no change.

Spanish writers refer all to Incal make, but Incal history only dates back to the eleventh century, and from that time to the Conquest is insufficient, nor do they speak of many of these works. It is granted that the Temple of the Sun, at Cuzco, was of Incal make, but that is the latest of the five styles of architecture visible in the Andes, each probably representing an age of human progress; therefore we are pretty certain that the imperial glories of the Incas were but the last gleam of civilisation that mounted up to thousands of years; that long before Manco Capac, the Andes had been the dwelling-place of races whose beginnings must have been coeval with the savages of Western

Europe. The gigantic architecture points to the Cyclopean family, the founders of the Temple of Babel and the Egyptian Pyramids. The Grecian scroll found in many places, borrowed from the Egyptians; the mode of burial and embalming their dead, points to Egypt as their similar, while the distaff, plough, manner of threshing and of making brick, are the same as when the Israelites were captives.

The hieroglyphics, to none of which as yet a key has been found, cannot be referred to the Incas, since they apparently had no knowledge of characters, but kept their records and accounts by means of a quippus, or knots and different coloured threads, as did those in Asia, China, Mexico, and Canada in ancient times, and they kept in each city an official whose business it was to keep and decipher their quippus. It was made of twisted wool, and consisted of a thread or thick string, from 1 to 18 ft. long, as a base upon which other threads or strings were attached. The different colours had different significations: the red, soldier or warrior; the yellow, gold; the white, silver or peace; the green, wheat or corn, and so on. In numerals, one knot signified ten; two simple knots, twenty: the knot doubly interlaced, one hundred; trebly interlaced, one thousand; two interlacings of this latter, two thousand. By setting apart a quippus for the military, another for the laws and decrees, another for historic events—*i.e.*, a separate quippus for distinct classes of ideas—the same knots could be used many times over, but to read them one must know to which class they belonged. Certain signs were affixed to the beginning of each "mother thread," as the base or principal string was called, by which the official could distinguish each. However, should an official visit another locality, these signs had to be explained verbally, also the signs representing local events, names of rivers, mountains, ships, cities, &c. Henee, a quippus was only intelligible, for the most part, in the place it was kept. Many quippus have been taken from the graves, in excellent state of preservation in colour and texture, but the lips that alone could pronounce the verbal key, have for ever ceased their function, and the relic seeker has failed to note the exact spot each was found, so that the records which could tell so much we want to know will remain sealed till all is revealed at the last day.

The skulls taken from the burial-grounds, according to craniologists, represent three distinct races.

The first, to which the name of "Chinchas" has been given, occupied the Western part of Peru from the Andes to

the Pacific, and from Tunebez, on the north, to the desert of Atacama an the south.

The second, called "Aymaras," dwelt in the elevated plains of Peru and Bolivia, on the southern shore of lake Titicaca, where they reside even to this day, being the only race that did not give up their language for the Inichua, or language of the Incas, when conquered by them.

The third, called "Huancas," occupied the plateau between the chains of Andes north of lake Titicaca, to the 9th degree of south latitude. The race were supposed to have caused the peculiar shape of their heads by mechanical means, as the Flat-head Indians with us, and the Conibos, a tribe that now live on the banks of the Ucayali, near Sarayacu, but the taking from a mummy of a fœtus of seven or eight months having the same configuration of skull has placed a doubt as to the certainty of this fact.

How changed! How fallen from their greatness must have been the Incas, when a little band of 160 men could penetrate, uninjured, to their mountain homes, murder their worshipped kings and thousands of their warriors, and carry away their riches, and that, too, in a country where a few men with stones could resist successfully an army! Who could recognise in the present Inichua and Aymara Indians their noble ancestry?

Their songs are typical of their condition, and are called "tristes," or sad songs. Always a duet in a minor key, and at night, as you hear it, it seems rather the expiring wail of some lost spirit than a human voice. It begins with a full inspiration of the lungs, and at the highest pitch of the voice, and ends with the expiration of the breath, in a low, long-drawn-out "andante pianissimo." The words are chanted, and often made up for the occasion. These are the words heard by a traveller from the lips of a young Indian mother, in the wild recesses of the Andes:

> "My mother begat me amid rain and mist,
> To weep like the rain and be drifted like the clouds.
> You are born in the cradle of sorrow,
> Says my mother; and she weeps as she wraps me around.
> If I wander the wide world over,
> I could not meet my equal in misery.
> Accursed be the day of my birth,
> Accursed be the night I was born,
> From this time, for ever and ever!"

Three times the Andes sank hundreds of feet beneath the ocean level, and again were slowly brought to their present height. A man's life would be too short to count even the centuries consumed in this operation. The coast of Peru has risen eighty feet since it felt the tread of Pizarro. Sup-

posing the Andes to have risen uniformly and without interruption, 70,000 years must have elapsed before they reached their present altitude.

Who knows, then, but that Jules Verne's fanciful idea regarding the lost continent Atlanta may be near the truth? Who can say that, where now is the Atlantic Ocean, formerly did not exist a continent, with its dense population, advanced in the arts and sciences, who, as they found their land sinking beneath the waters, retired, part east and part west, populating thus the two hemispheres? This would explain the similarity of their archæological structures and races, and their differences, modified by and adapted to the character of their respective climates and countries. Thus could the llama and camel differ, although of the same species; thus the algoraba and espino trees; thus the Iroquois Indians of North America and the most ancient Arabs call the constellation of the "Great Bear" by the same name; thus various nations, cut off from all intercourse or knowledge of each other, divide the Zodiac in twelve constellations, apply to them the same names, and the northern Hindoos apply the name Andes to their Himalayan mountains, as did the South Americans to their principal chain. Must we fall in the old rut, and suppose no other means of populating the Western Hemisphere except "by way of Behring's strait"? Must we still locate a geographical Eden in the East, and suppose a land equally adapted to man and as old geologically, must wait the aimless wanderings of the "lost tribe of Israel" to become populated?

Beside dead and speechless relics of the past, there exists a living antiquity. In 7° S. latitude, a couple of miles from the sea, there is a town of about 4000 inhabitants called Eten. They speak, besides the Spanish, a language that some of the recently brought over Chinese labourers understand, but differ in all other respects. They intermarry brothers and sisters, uncles and nieces, nephews and aunts, *i.e.*, promiscuously, with no apparent curse of consanguinity. They are exclusive, permitting no intermarriage into their number or with the outside world. They have laws and customs and dress of their own, and live by braiding hats, mats, and weaving cloths. They will give no account of when they came or from whence, nor does history mention them as existing before the Spaniards came, nor does it record their arrival since. Among them you will find no sick or deformed people, their custom being to send a committee to each sick or old person, and if they judge the

patient past recovery, or the aged past usefulness, the public executioner is sent and they are strangled. Eten orders it, they say, and none ever interfere with these orders.

Thirteen thousand years ago, *Vega*, or *α Lyræ*, was the north polar star. Since then how many changes has she seen in our planet! How many nations and races spring into life, rise to their zenith splendour, and then decay; and when we shall have been gone thirteen thousand years, and once more she resumes her post at the north, completing a "Platonic, or Great Year," think you that those who shall fill our places on the earth at that time will be more conversant with our history than we are of those that have passed? Verily might we exclaim in terms almost Psalmistic, Great God, Creator and Director of the Universe, what is man that Thou art mindful of him!

NOTICES OF BOOKS.

Bacon's Novum Organum. Edited, with Introduction, Notes, &c., by Thomas Fowler, M.A., Professor of Logic in the University of Oxford. Clarendon Press Series. Oxford. 1878.

There have been many editions of the "Novum Organum," annotated with greater or lesser care. The book is read at Oxford, and it appeals to the interests of a large section of the cultivated community of a nation, in its fourfold bearing upon the history of Philosophy, Logic, Literature, and Science. Prof. Fowler's edition is prefaced by a carefully-written Introduction, embodying the result of much literary research, and discussing all the principal points of interest connected with Bacon as a great thinker, and as a man who profoundly influenced the mode of thought of succeeding ages. This is followed by the text of the "Novum Organum," which is accompanied by numerous very exhaustive notes, in the preparation of the scientific portions of which the author acknowledges the assistance of Prof. H. G. S. Smith, Mr. Kitchen, and Prof. Clifton, while he gives frequent references to our most modern text-books, such as "Watts's Dictionary of Chemistry," Ganot's "Physics," Deschanel's "Physics," and Tyndall's "Heat and Mode of Motion."

Prof. Fowler commences his Introduction by giving the dates of the principal events in Bacon's life, and of the first publication of his writings. He then discusses the object of the "Novum Organum," and the nature of Bacon's philosophical opinions. An important section (pp. 22 to 43) relates to Bacon's scientific attainments and opinions. The author is compelled to admit the three charges most commonly brought against Bacon, but he finds something to plead in extenuation of each of them. "The first is that he was a *dilettante* in Science. The second, that he was imperfectly acquainted with the existing state of knowledge. The third, that he grossly exaggerated the defects of his own time, which, in spite of all that he says, was really one of great and fruitful intellectual activity." Even if we were to admit all this in its fullest extent, the services which Bacon rendered to Philosophy and Science would merit our warmest recognition. Prof. Fowler considers that the main peculiarities of Bacon's method and teaching may be embraced under four heads:—"(1.) The emphasis with which he insisted on the necessity of consulting and collecting facts, of going straight to Nature, of instituting observations and experiments before

formulating general propositions. (2.) The gradual ascent from propositions or axioms of a lower to those of a higher degree of generality. (3.) The selection and comparison of instances in place of the old *Inductio per Enumerationem Simplicem.* (4.) The disregard of Authority, and the restraint of Fancy."

No one who has the least acquaintance with Bacon's works asserts that he was a great experimental philosopher: although Voltaire and Maclaurin have called him "the father of experimental philosophy," we know that Galileo was much more worthy of the title. Again, no one who is acquainted with the history of philosophy asserts that Bacon invented the Method of Induction. To mention no earlier researches, the discoveries of Galileo in Astronomy and Dynamics far exceeded any result obtained by Bacon in direct experimental or observational Science; while the method of induction had been employed long before Bacon's time by Leonardo da Vinci, Nizolius, and many others. But for all this Bacon did contribute most materially to the advance of Experimental Science, and to the development of the Logic of Induction. Moreover, he popularised Science, he insisted on the accumulation of experimental facts, he freed men's minds from the blind deference to the authority of Aristotle, and he advocated the employment of scientific investigation to the furtherance of man's estate and the increase of man's power over Nature.

We think that Prof. Fowler's estimate of the influence of Bacon on the progress of Science is a very just one. He has weighed each side of the question fairly, and in an impartial spirit, and he has discussed with judicial accuracy the merits or demerits of the most conflicting opinions. The notes which he has written in explanation of the scientific portions of the second book are in the main very precise and accurate. It is only here and there that we find obscurity or inaccuracy of diction, as, for instance in Note 91 (p. 563), "*Omnis enim vita, immo etiam omnis flamma et ignitio,*" to which is appended the note—"Compression acts in this case by stopping the supply of oxygen which maintains the combustion"; or in Note 96 (p. 565), in which the theory of the transmutability of the elements is attributed to Bacon, while it was originated by the Greeks two thousand years before.

The Theory of Sound. By JOHN WILLIAM STRUTT, Baron RAYLEIGH, M.A., F.R.S. Vol. ii. London: Macmillan and Co. 1878.

THE second volume of this very valuable work consists of nine chapters, which treat respectively of Aërial Vibrations; Vibrations in Tubes; Special Problems connected with the Reflection

and Refraction of Plane Waves; General Equations; Application of General Equations; the Theory of Resonators; Application of Laplace's Functions; Spherical Sheets of Air, and Motion in Two Dimensions; Fluid Friction, and the Principle of Dynamical Similarity. The whole subject is treated with consummate ability, and the work affords a complete mathematical treatment of the subject.

The Moon, her Motions, Aspect, Scenery, and Physical Condition. By Richard A. Proctor. Second Edition. London: Longmans. 1878.

In the second edition of this popular work the author has omitted a good many calculations, connected with the lunar theory, which were too difficult for the general reader. He proposes to reprint these in a separate volume on the "Geometry of the Lunar Theory," for the use of those who are acquainted with the higher mathematics. Otherwise this edition does not differ much from the preceding, except that the final chapter, on the moon's physical condition, has been enlarged. We may remind our readers that the author discusses in succession the size, mass, and distance of the moon; her motions and changes of aspect, and a complete study of the surface so far as it is known. In the chapter on Lunar Celestial Phenomena the lunar atmosphere, scenery, seasons, and eclipses are described. The author considers that the "moon was *shaped*, so to speak, when the solar system itself was young, when the sun may have given out a much greater degree of heat than at present, when Saturn and Jupiter were brilliant suns, when even our earth and her fellow minor planets within the zone of asteroids were probably in a sun-like condition." He inclines also to the belief that the process of contraction which the moon's surface underwent produced the processes of disturbance which have resulted in the present condition of the moon's surface. An admirable map of the moon, after Beer and Mädler, is given at the end of the volume, and some striking illustrations of the condition of the moon's surface are taken from Nasmyth and elsewhere. Altogether the work furnishes a very popularly written, and at the same time exact, account of the moon, and we are sure that it will continue to be largely read both by the scientific and the ordinarily well-educated reader.

The Art of Scientific Discovery, or the General Conditions and Methods of Research in Physics and Chemistry. By George Gore, LL.D., F.R.S. London: Longmans. 1878.

Although we are of opinion that the faculty of original research must be inherent in a man, and cannot be often implanted in him,—in a word that *iuvestigator nascitur non fit*,—we are very glad to welcome this admirable treatise from the pen of Mr. Gore. It is a sort of *Ultimum Organum* in the matter of scientific research. It is thoroughly Baconian, and may be regarded as a nineteenth century representative of the second book of the "Novum Organum." It is philosophical in tone, and is throughout characterised by the blending of great experience with logical deduction. It will be read with profit both by the student and by the most advanced investigator.

The work is divided into five parts, which are respectively devoted to (1) a general view and basis of scientific research; (2) general conditions of scientific research; (3) personal preparation for research; (4) actual working in original scientific research; (5) special methods of discovery. The first part treats of the distinction between the attainable and unattainable objects of research; the certainty of scientific knowledge; the general principles of science; and criteria. The author passes on to the consideration (Part 2) of the general basis of success in discovery. Herein he reviews the position of man as a discoverer in Nature, the general course and character of many notable discoveries, and the classification of scientific truths. The third part treats of personal preparation for research, in which the author points out not only the necessity of manipulative skill, but also that of imaginative power. The fourth part is devoted to the discussion of the actual working in original scientific research, and this will specially recommend itself to all scientific readers. The fifth and last division of the work treats of special methods of discovery.

We most cordially recommend this work to the attention of all lovers of natural truths.

Lessons in Thermodynamics. By Robert E. Baynes, M.A., Lee's Reader in Physics. Oxford: at the Clarendon Press. 1878.

This work, which, although small, furnishes the only complete mathematical introduction in English to the Dynamical Theory of Heat, is based upon the subject-matter of a course of lectures delivered at the Clarendon Laboratory in Oxford, during the

Hilary Term of 1876. The work commences with definitions of the fundamental limits, and passes on to the methods of measuring heat. The third chapter treats of the conservation of energy, and in it the first law of Thermodynamics is fully discussed, together with Dr. Joule's experiments, and the general equation of equivalence. The fourth chapter is devoted to the transformation of heat, and includes the development of Carnot's function and the second law of Thermodynamics. Other chapters treat of change of state, the flow of fluids, and heat-engines. The kinetic theory of gases is omitted, because Mr. Watson's treatise on the subject contains all that is required.

Mr. Baynes has furnished frequent references to the most notable and most recent researches connected with his subject, and his treatise will become a standard work wherever the higher branches of Physics are taught from a mathematical point of view.

Hydrostatics and Pneumatics. By PHILIP MAGNUS. London: Longmans. 1878.

THIS forms the seventh volume of the "London Science Class-Book Series. It is divided into seven chapters, which discuss the principal facts connected with liquids and gases, at rest and in motion. The motion of liquids and gases is less completely studied than the statical problems in relation to these bodies; and the considerable results which follow, both in Nature and on the lecture table, from the diminution of pressure in the line of a moving fluid or gas, are completely ignored. The book, although intended for school purposes, is in the main somewhat too advanced for any but the older boys in a school: it will, however, be very useful for examination purposes and for special reading.

In a second edition we trust the author will introduce some mention of the experiments of his namesake, Prof. Magnus, on the motion of fluids, and will give one or two examples in each set of exercises of worked-out problems.

We cannot tell who is responsible for Fig. 35, p. 76, or whether the author has experimentally tried it; suffice it to say that if the flow of water through the pipe be at all rapid, the liquid, so far from being raised in the lateral tubes, will draw in air through them, and will afford no indication at all of the amount of pressure on the walls of the tube.

Electricity and Magnetism. By FLEEMING JENKIN, F.R.S., Professor of Engineering in the University of Edinburgh. Fourth Edition. London: Longmans. 1878.

THE fact that this work has already reached a fourth edition is sufficient evidence of its appreciation. It is based upon all the newest principles developed by Sir William Thomson, Prof. Clerk Maxwell, Mr. Latimer Clark, and the author himself, and it is quite indispensable for those who take up the study of Electric Telegraphy with a view to its application to practical purposes. The questions of quantity, intensity, resistance, and potential are fully discussed. Useful chapters are devoted to electrical and magnetic measurements, to the use of instruments, and to the whole subject of telegraphy.

We have no doubt that the work will continue to hold the high position which it has attained.

A Text-Book of Arithmetic for Use in Higher School Classes. By THOMAS MUIR, M.A., F.R.S.E., Mathematical Master in the High School of Glasgow. London: Daldy, Isbister, and Co. 1878.

A VERY comprehensive, clearly written book, with all the examples worked out at the end. We have no doubt it will be largely used in Scotch schools.

A Treatise on the Dynamics of a Particle. By P. G. TAIT, M.A., and W. J. STEELE, B.A. Fourth Edition. London: Macmillan. 1878.

THIS work leaves little to be desired, so far as the higher parts of the subject are concerned, either as to fulness of matter or excellence and variety of problems; but the elementary portion is hardly worthy of the rest. The chapter on Kinematics would be greatly improved if the analytical investigation in it were supplemented by a little Geometry. The discussion of the Laws of Motion is at times confusing to a beginner from the number of details presented to his notice, whilst the excessive brevity of treatment required by the plan of the work prevents sufficiently full explanation. The only fault in the book is that a beginner in Dynamics cannot read it.

Report of the New Jersey State Commission appointed to devise a Plan for the Encouragement of Manufacturers of Ornamental and Textile Fabrics. Trenton: Naar, Day, and Naar. 1878.

We have here, within the compass of one hundred pages, a storehouse of useful facts and abundant food for serious reflection. The State of New Jersey has issued a Commission to examine in what manner manufacturing industry may best be encouraged within its jurisdiction, and the Commissioners have evidently come to the only legimitate conclusion—that the prosperity of manufacturers can only be permanently secured by raising the standard of intelligence and education both among employers and employed. If we ask what kind of education is needed, we must at once perceive that none of the three grades of mental training, as supplied respectively to the lower, the middle, and the upper class in England,—that is, the rudimentary, the commercial, and the classical,—can answer the purpose. How fully the Commissioners agree with us appears from the fact that they consider the position of the United States "alarmingly unfavourable," whilst they recognise that the American "system of general primary education is more widely spread and more effective than in any country in the world; and while we have a larger number of schools, in proportion to population, than perhaps any other country, we are destitute of trade schools, and have extremely inadequate provision for industrial education of any kind and for any class of our people." If, then, the educational position of America, from an industrial point of view, is—in spite of its elaborate and carefully-worked "common-school" system—still to be regarded as alarmingly unfavourable, we must guard against expecting much from the recent steps taken in this country in the way of primary education. Setting on one side the palpable fact that all persons in England who really wished for elementary instruction could have acquired it even before the passing of the Education Act, we cannot see that either our "Board" or our "Denominational" schools will greatly increase the industrial or the inventive capabilities of our population. What we want is a system of training which shall fix the attention of the student upon things rather than upon words.

One point seems to us to require explanation. If, as the Commissioners expressly declare, "all Europe is a generation in advance of us" (America),—if in Germany "the visitor from the United States is oppressed by the apprehension that his own country, unprovided with such efficient and essential means of giving to the people a good industrial education, is likely to suffer severely,"—how is it, we ask, that America is so fruitful in inventions, many of them of undeniable value? Are we not

almost forced to conclude that an efficient patent-law goes very far to countervail the effects of a deficient system of secondary education? And, if this is the fact, should not our own patent-laws be amended in a totally different direction from that repeatedly proposed by the present Lord Chancellor?

The accounts given in this pamphlet of the various industrial schools—using the term in its natural sense—on the European continent are exceedingly significant. We find that the Zurich Polytechnicum consists of about a hundred professors and assistants, and numbers nearly a thousand students. It has an astronomical observatory, a large chemical laboratory, laboratories of research and special investigation, collections of models of engineering constructions, museums of natural history, architecture, &c., all extensive and rapidly growing. This important establishment is supported by a population of only three millions of people, and possessing probably less wealth than that of some single counties in England! Its revenues consist of £12,500, paid by the Swiss nation and the canton of Zurich, and not much more than £2000 from students' fees. If, therefore, the number of students is near a thousand, the average cost to each must be between £2 and £3 yearly! The Commissioners remark that "in this Institution a young man may acquire an education such as can be given him nowhere in this country (America)." Do we not now see some at least of the reasons why, in spite of great natural disadvantages, such as dear fuel and distance from the sea, Switzerland figures so honourably at the Paris Exhibition?

The manner in which the Commissioners sum up their remarks on the German system of education is profoundly significant:—"It is sufficiently evident that in Germany, even more than in France, the governing and the educated classes, instead of standing aloof from each other, and instead of forgetting—as is too generally the case in our own country—those great facts and those imperative duties which every statesman (?) does and every citizen should recognise, have worked together for the common good, and have given Germany a vantage-ground in the universal struggle for existence and wealth which is likely in the future to enable that country for many years steadily to gain upon all competitors. It is now generally admitted that Germany is the best educated nation of the civilised world: there is danger that the United States may, with reason, be reckoned the worst." We certainly should be the last to deny or doubt the value of the German educational system, which in many respects we consider a model for the world; but its industrial position is, nevertheless, not quite so assured. Did not an American paper, in summing up the results of the Philadelphia Exhibition, use the memorable words—"From all other nations we have learnt something; from Germany nothing"? Did not, on the same occasion, an eminent German professor preach to his countrymen a sermon

the text of which was "Billig und schlect"? Do we not hear the Germans themselves lamenting over the industrial condition and prospects of their own country? It seems to us that in the present state of general commercial depression every country imagines the position of its rivals better than its own, their measures more judicious, and their future more assured. Is not the brilliant figure which Germany makes in the intellectual world the result of an impulse given half a century ago, but now gradually slackening amidst the hindrances of dominant militaryism and of a newly-awakened greed for wealth? From the expulsion of the first Napoleon the principle of Germany was "plain living and high thinking;" she preferred the man of genius, learning, and research, even if in a threadbare coat, to the mere accumulator of money, Is this still her creed, or is she not, in her haste to get rich, "killing the goose that laid the golden eggs"?

To return to our more immediate subject: the Commissioners are naturally forced to admit that Britain has no such system of secondary education as exists on the Continent. They quote the advice given by C. Graham Smith to all aspirants to the profession of civil engineering, to go to Paris and take the course of study prescribed at l'Ecole Centrale, or else to attend lectures at a German or Swiss Polytechnic School. Still they point out that much is now being done "to redeem Britain from the disgrace which at one time threatened her." They consider that it is in the industrial application of art-education "that the British nation has most wonderfully exhibited the energy and the thoroughness with which it usually carries on whatever work it may undertake in real earnest." Giving a brief sketch of the rise and progress of our schools of art, they declare that—"From a position far, very far, behind the continental nations, Great Britain has, in this one technical department, brought herself up into the very foremost place."

The thought here naturally suggests itself that what we have done in one department we may do also in others. We have merely to exert ourselves for industrial science as we have done for industrial art, and we shall soon be spared the humiliation of exporting raw materials and re-importing them after having undergone a transformation. We shall have, it is true, a struggle with the speech-makers, who justly fear that if we base our education upon Science their reign will be over.

We cannot better conclude this notice than by citing the following admirable passage:—"In the selection of instructors for the highest class of technical schools another qualification is demanded, so extremely rare that few universities even can boast the possession of more than a very small proportion of such men. The man who is needed in the school of technology, where advanced students and professional men are to be taught the highest studies applicable in their professional work, must

not only be a learned scholar and a good teacher, but he must be an investigator, capable of entering upon original researches and of solving practical problems. He must be able to 'add to the sum of human knowledge,' and to supply the many serious omissions and correct the many important errors with which text-books abound wherever they touch the practical side of their subjects." If the American people will put such men in the professorial chairs of their technical schools the industrial future of their country is secured.

The Annual Report of the Queensland Philosophical Society, 1877, with the President's Address. Brisbane : Beal.

This Report contains little to cheer those who take an interest in the progress of Science in general, and who wish in particular that the treasures of fact which Nature offers in such regions as Queensland should not continue to lie unobserved and unrecorded. The Council complain that "the number of papers read has been very small [only three !], and it is a matter for regret that so little interest is taken in the Society, as is evinced by members not bringing forward either interesting papers for discussion or any objects of interest which they may possess for exhibition." The names of the three members who have done their part for the credit of the Society are—Mr. G. Bennett, who contributes a paper on the "Mammalia of Australia;" Mr. Diggles, who produced a memoir on "Some New and Rare Specimens of Australian Birds," in which he describes four species new to Science; and Mr. Thorpe, who has urged the importance of collecting Meteorological Statistics.

As regards the Presidential Address, without in the least calling in question the ability displayed, we must point out that it is devoid of local character, and might have quite as appropriately been delivered in London, Paris, or Berlin. We submit that a general outline of the immense and varied harvest of phenomena which only awaits earnest reapers to be brought into the world's garner would have been more than the somewhat metaphysical disquisitions into which Sir James Cockle has thought proper to enter.

Preliminary Report of the Field-work of the U.S. Geological and Geographical Survey of the Territories for the Season of 1877. Washington : Government Printing-Office.

This issue gives merely a general sketch of the ground worked over, without entering into a detailed account of results. We

find it mentioned in brief that Mr. Scudder has made an inspection of the insectiferous shales of the Tertiary basin of Florissant, which he estimates to be at least fifty times as extensive as those of Œningen, in Southern Bavaria. From six to seven thousand specimens of fossil insects, besides some thousand plants, have been already received from this new locality, and as many more will be secured before the end of the season. It is not too much to hope that these treasures may throw some fresh light on the development of entomological forms, and on the successive appearance in point of time of the different insect orders and families.

In the botanical department, also, much may be expected. Two of the greatest authorities in this science—Sir Joseph D. Hooker, P.R.S., Director of the Kew Botanical Gardens, and Prof. Asa Gray—took an active part in the Survey, and made a careful investigation into the flora of the Rocky Mountains and the Sierra Nevada.

Ethnologists may also look forward with interest to the forthcoming account of a "forgotten people," who once occupied the country about the head waters of the San Juan.

Geological Survey of Victoria. Prodromus of the Palæontology of Victoria. Decade V. By Frederick McCoy. Melbourne: Ferres. London: Trübner and Co.

This work consists of figures and descriptions of organic remains found in the colony of Victoria. No systematic arrangement appears to be followed, animals of various classes and plants succeeding each other promiscuously. The illustrations are well drawn, and the descriptions are full.

The Journal of the Royal Historical and Archæological Association of Ireland. Fourth Series, Vol. iv., Nos. 33 and 34, January and April, 1878. Dublin: printed for the Association.

This issue contains a history of the kingdom of Ossory, with a list of its sovereigns. There is here nothing of scientific interest, if we except the suggestion that during the later geological periods the central portion of the county of Kilkenny was the basin of a large lake. The death of a king is mentioned as having taken place "A.M. 3817." We had hoped that chronolo-

gists would by this time have ceased dating from an event, or rather a process, of unknown and unknowable antiquity.

Memoirs of the Geological Survey of India. Palæontologia Indica; being Figures and Descriptions of the Organic Remains procured during the Progress of the Geological Survey of India. Indian Tertiary and Post-Tertiary Vertebrata (Vol. i., 3). Ser. X., 3.—Crania of Ruminants. By R. Lydekker, B.A. Calcutta: Geological Survey Office, London: Trübner and Co.

The author here figures and describes all the Bovoid Ruminants in the Indian Museum, identifying as many of them as agree with those of Dr. Falconer's species of which figures and descriptions are extant, and discussing their affinities. He remarks that there are at present seven well-marked species of true Bovidæ in South-eastern Asia:—*Bubalus arni*, *Bibos gaurus*, *frontalis*, and *banting*, *Bos chinensis* and *indicus*, and *Poephagus grunniens*. Nowhere in Asia is there a true taurine ox. *Bubalus arni* is doubtless the direct descendant of the gigantic *Bubalus palæindicus* of the Nerbudda gravels and of the uppermost Siwalik beds. The fossil species now identified are *Bos namadicus*, *planifrons*, *acutifrons*, and *platyrhinus*, *Bubalus platyceros* and *palæindicus*, *Bison sivalensis*, *Peribos occipitalis*, *Hemibos triquetriceros*, and *Amphibos acuticornis*, all of which, except the first-mentioned, have been found in the Siwalik. *Bubalus palæindicus*—an animal of immense strength, far surpassing the living Arni—was doubtless contemporary with man, since stone implements have been found in the bone-beds from which its remains have been obtained. The Sivatheridæ—of which a new species, *Hydaspitherium megacephalum* (Lydekker), is here figured and described—are interesting as connecting the now isolated genus *Camelopardalis* with other families of the order,

The figures, which are beautifully executed, have been chiefly lithographed by a native artist, Rasick Lall Bose.

Ser. XI., 2.—Flora of the Jabalpur Group (Upper Gondwanas) in the Son-Narbada Region. By Ottokar Feistmantel, M.D. Calcutta: Geological Survey Office. London: Trübner and Co.

This volume is devoted to Ferns, Cycads, and Conifers. In his summary the author points out that in the Jabalpur-Kach group there is an older flora with a younger flora; whilst in the Kota-Maleri group the flora is the younger element and the land

animals and fishes the older one. The flora of the upper Godwana series is of a similar type to that of the jurassic (lower and middle) floras of Asia and South-eastern Russia.

Ser. II., 3.—Jurassic (Liassic) Flora of the Rajmahal Group from Golapili (near Ellore), South Godavari District. By OTTOKAR FEISTMANTEL, M.D., Palæontologist, Geological Survey of India. Calcutta: Geological Survey Office. London: Trübner and Co.

WE have here a description of Fossil Ferns, Cycads, and Conifers found embedded in the brown sandstone of Golapili. The beds in which they occur are concluded to correspond in age with the Rajmahal group in the Rajmahal hills. The illustrations are beautifully executed, and we are glad to perceive that native skill is employed in the lithographic process.

Ser. IV., 2.—On some Remains of Ganoid Fishes from the Deccan. By Sir PHILIP DE M. GREY EGERTON, Bart., F.R.S., F.G.S.

On the Genus *Ceratodus*, with Special Reference to the Fossil Teeth found at Maledi, Central India. By L. C. MIALL, Professor of Biology in the Yorkshire College of Science and Curator of the Leeds Museum.

On the Stratigraphy and Homotaxis of the Kota-Maledi Deposits. By W. T. BLANFORD, A.R.S.M., F.R.S., Geological Survey of India.

Calcutta: Geological Survey Office. London: Trübner and Co.

SIR P. EGERTON's paper is a description of Ganoid Fishes belonging to the genera *Lepidotus*, *Tetragonolepis*,—a form hitherto only found in the lias of Bang and Boll, in Germany, and of Dumbleton, in Gloucestershire,—and *Dapedius*.

Species of fishes referred to the genus *Ceratodus* are widely and curiously distributed. They have been found in a blackish shale in Illinois (*C. Vinsolvii*, Cope); in the upper keuper sandstone of Worcester, at Aust Cliff, and in the oolite of Stonesfield (*C. lævissimus*, *obtusus*, *disauris*, and *Phillipsii*); in the triassic rocks of South Germany, where some eight species have been discovered; in India, at Maledi, whilst in the rivers of Queensland a species, *C. Forsteri*, still exists.

The age of the beds named in the title Kota-Maledi, but in the text repeatedly Kota-Maleri, seems a difficult problem:—"We have an association in the same group of beds of triassic reptiles and fish, liassic fish and plants, which may be either middle or upper jurassic, according as plants or marine animals are considered better evidence of age. If the latter are admitted

to be the most characteristic, the plants found in the Kota-Maleri beds would be upper jurassic in other parts of India."

Botany, Outlines of Morphology, and Physiology. By W. R. McNab, M.D., F.L.S. ("The London Science Class-Books," edited by C. Foster, F.R.S., and P. Magnus, B.Sc.) London: Longmans and Co.

This little work is, as its title expresses, a condensed exposition of the morphology and physiology of the vegetable world. The results of modern research are given briefly, but with considerable clearness. To those entering upon the study of Phytology it will prove a useful guide. The absence of an index strikes us, however, as a defect.

Invertebrata. By Prof. A. Macalister. ("The London Science Class-Books.") London: Longmans and Co.

The little work before us undertakes to furnish, in the brief compass of 136 pages, an account of all the invertebrate sections of the animal kingdom. From this short space a considerable portion must further be deducted for the three well-written introductory chapters on the nature of animals, their vital functions, on nomenclature and classification, mimicry, parasitism, distribution in time and space, &c. The difficulties of the task, therefore, have not been trifling, and we may congratulate the author on his success in omitting so little that is of primary importance, and in presenting so clear a view of his subject under disadvantages so great.

At the same time we cannot pronounce the work altogether unexceptionable. There are certain errors, typographical or clerical, which a little care in revision would certainly have obviated. Thus, on p. 131, we find *elytræ* instead of *elytra* as the plural of elytron. On the same page *Agriotes*—the coleopterous genus whose larvæ are so unfavourably known as wireworms—is misprinted *Agrites*. On p. 129 we read that "the larva of the common daddy-longlegs feeds on the roots of grass, &c." There are also errors of a more serious nature. On p. 115 is a figure showing the structure of an insect's body, and said to be a dragonfly. Dr. Macalister must be aware that it differs from a dragonfly in every part and organ, and agrees much more closely with a locust. On p. 131 the reader is told that "the destructive Colorado or potato-beetle somewhat resembles the ladybird, but is ten-striped, and not spotted." In a treatise

which, though brief and elementary, is scientific, it would have been better to point out that the potato-beetle, as belonging to the Chrysomelidæ, has four main joints in each tarsus, especially as the three-jointed tarsi of the ladybird had just been mentioned. On p. 129 we find the statement that "On the lower lip in the larvæ of some moths there is a tubular spinning gland, which, when the larva has reached its full size, secretes a silken cocoon within which it is enclosed." This unhappily expressed sentence will lead learners to believe that silk is secreted by some species only, instead of, as is the fact, by all lepidopterous larvæ. If grammatically construed it also conveys the meaning that the cocoon is secreted as such, instead of merely in the shape of a silken thread from which it may be spun.

In treating of the distribution of animals the author says:—"Sometimes the presence of one animal prevents the diffusion of others: thus in Africa the tzetze fly renders whole tracts uninhabitable by oxen and deer, which are destroyed by its poisonous bites." It so happens, however, that with the exception of the Mediterranean coasts deer are absent throughout Africa, in districts free from the tzetze as well as in those which it infests.

We hope to see these defects remedied in a future edition.

Vertebrata. By Prof. A. Macalister. ("The London Science Class-Books.") London: Longmans and Co.

In this little volume Prof. Macalister completes the brief view of the animal kingdom begun in his treatise on the Invertebrata. We are unable to agree with him in his estimate of the comparative importance of these two great divisions of the animal kingdom. In his introductory remarks he expresses the opinion that "they (the Vertebrata) present to us a greater number of interesting points in structure, function, and habits than all the other sub-kingdoms put together; and as they are, for the most part, of large size and of complex organisation, they require a more careful and detailed study than do the animals which make up the other sub-kingdoms." We grant, of course, the larger size and the superior complexity of the Vertebrata, but on all other points we are compelled to join issue. We consider that among the Invertebrata there is not only a much larger number of species, but greater range of structure, and especially of habits, than among the vertebrate animals. We are glad to find here the order Primates—freed, however, from the lemurs—in place of the Cuvierian groups of Quadrumana and Bimana, and to come upon no hints as to the propriety of referring man to a separate class, or even sub-kingdom. The seals are quite justifiably separated from the Carnivora, which order is thus restricted

to comprise the Felidæ, Canidæ, Viverridæ, Mustelidæ, and Ursidæ. But, observing this, we cannot help referring to a passage at the conclusion of the volume on the Invertebrata, in which the author observes—" The subdivisions of insects are by some looked upon as deserving of a higher than ordinal rank; but as the nature of a group depends on the nature of the range of structure in the forms comprehended therein, and not on the number of included individuals, we cannot but see that in each order of insects the component species are constructed so much on one type as to forbid us from making of them more than ordinal groups." Accepting the principle thus laid down for determining the value of groups, we submit that in the so-called order Coleoptera there are differences of type more important than such as are among Mammalia allowed to separate the order Pinnepedia from Carnivora.

In the remarks on the migration of birds we notice an oversight. It is stated that the " swallows, swifts, nightingales, and warblers visit this country about the middle of April, breed here, and then return to their winter quarters about the first week of October." This is decidedly incorrect as far as the swift is concerned, which leaves us in August.

In the chapter on reptiles the common viper is described as recognisable by its dark green colour. We have had very extensive opportunities for observing this species, which, thanks to the destruction of its enemy, the hedgehog, is now on the increase in Britain, and have always found the males of an ashy or silvery grey, and the females copper-coloured, whence the popular German name " Kupferschlange." Neither can we endorse the opinion that " the poison of snake-bites is rapidly fatal, death taking place within an hour in general." This is doubtless true as regards the cobra, but not of the bites of poisonous snakes in general.

The author's description of the structural characters, both of the Vertebrata in general and of the several classes, is perhaps all that could be expected in so brief a compass. We have great pleasure in noticing that in this and in its companion volume the copious index is made to serve, at the same time, as a glossary of technical terms—a most valuable feature.

A Treatise on the Pathology of the Urine, including a Complete Guide to its Analysis. By J. L. W. Thudichum, M.D. Second Edition. London: J. and A. Churchill. 1877.

Although this is the second edition of Dr. Thudichum's well-known treatise, yet the long interval which has elapsed since the publication of the first edition in 1858 and the present one, and the consequent necessity of recording the mass of work which

has been done during these twenty years, gives an importance to this new volume which is far greater than that usually *to be* attached to the re-issue of such a volume. Of this mass of work, here so ably recorded, a very large portion is due to the active and untiring personal investigations of the author himself. Few men have studied more deeply than Dr. Thudichum the chemistry of the urine in health and in disease, and his results in this field of investigation alone, apart from his masterly remarks on the chemistry of the bile and his still later discoveries in connection with the chemistry of the brain, claim and secure for him the foremost rank among contemporary workers in science.

What is especially valuable in the book, in addition to the mere record of facts, is what may be termed its historical aspect. To each substance of importance the author devotes a special chapter, and gives at the beginning of each a summary of its history, so that from the date of its discovery we are enabled to follow its development at the hands of the various chemists who have given it their attention. It may be said, in fact, that the author, not content with mere history, has repeated all experiments of any importance relating to his subject,—a method of procedure which enables him, in his critical remarks, to confirm or condemn with an authority not to be lightly gainsaid. As a special illustration of this the exhaustive chapters on Urea and Uric Acid may be cited.

A special chapter is devoted to Xanthine. This substance, first discovered by Marcet in 1819 in a calculus, was further discovered by Liebig and Wöhler, and shown by Strahl and Lieberkühn in 1848 to be a normal ingredient of urine. Ten years later (1858) Thudichum showed it to be a normal ingredient of the human liver. Its now extensively studied properties are graphically detailed, and its relation to hypoxanthine and guanine clearly pourtrayed. Its preparation by the phosphomolybdic process is of great value. By this process Thudichum obtains Kreatinine as such, and also two new bodies—Reducine and Urochrome. To these and similar bodies he applies the term "alkaloid" in a wider sense than is usually accepted. Reducine is a characteristic type of a remarkable class of bodies occurring also in the brain, and possesses, like these, in addition to its basic properties, co-existent acid properties, pointing to the probability of its being an amido-acid with marked basic tendencies.

The fact that hypoxanthine, a pathological ingredient of urine, occurs naturally in the brain of man and ox, though vaguely indicated by Müller, was first experimentally demonstrated by Thudichum. Guanine, although not occurring in urine, also receives special attention, on account of its chemical relations to the uric acid group.

Under Kreatinine, an undoubted ingredient of urine, we have

much interesting matter. It is important to note that Thudichum insists on the existence of kreatine in flesh and urine as by no means proved, since the processes of its extraction involve the transformation of kreatinine into kreatine. The question arises, in our mind, whether the phospho-molybdic process already mentioned—which is said to produce kreatinine from urine free from kreatine—would, when applied to the juice of flesh, yield a similar result.

The chapter on Hippuric Acid affords a strong interest, historical, critical, and chemical. The same remark applies to the next following chapter, on Indigogen and Urrhodinogen. The long and not unamusing controversy on these chromogen products—or, as their partisans insist on calling them, colouring-matters—is here reproduced. The theory that the yellow colour of the urine in certain diseases is due to an indigo-bearing substance is clearly stated, and still more clearly disproved by Thudichum. Thudichum demonstrates that the most colourless urine—viz., the first urine in the reaction from cholera—yields indigo-blue in greatest abundance, and that urrhodine (the red matter stated by many to be isomeric with indigo-blue) contains, unfortunately for this wild theory, no nitrogen whatever!

Passing over the chapters on Pyrocatechin, and the presence of phenol and cresol producing substances in the urine,—matters still surrounded with mystery,—we come to Schorling's Omicharyb-oxide, which Thudichum shows to be identical with the fallow-resin of Proust, discovered as far back as 1800. Next follows a brief description of Urerythrine, also first described by Proust as rosacic acid—a pathological ingredient of urine which imparts to it a fiery red colour, and is found among the "latentious deposits" along with the urates. Its spectrum has been specially examined and recorded by Thudichum.

We now come to Urochrome, "the matter to which urine owes the whole or the greater part of its yellow colour,"—an important discovery of Thudichum's in the year 1864, and for which he received the Hastings Prize Medal of the British Medical Association. It is impossible to enter into details relating to this body here; suffice it to say that the competent recognition it has received is fully justified by the mass of scientific data concerning it. The products of its decomposition—uromelamine, uropittine, omicholine, and omicholic acid—are also new to chemistry.

An equally important discovery of Dr. Thudichum's is kryptophanic acid, first observed by him in 1869. It is a normal constituent of urine, and the principal one of the bodies "which constitute the complexity of matters formerly called 'extractives.'" The detailed description of this remarkable substance, and the abundance of analytical proof concerning it and its sixteen studied salts, cannot fail to convince the reader of its chemical individuality. The occurrence of acetic and formic

acids in the urine has also received special attention at the author's hands.

With mention of these we conclude our notice of what for convenience may be termed the "organic" normal ingredients of the urine. Space precludes a similar notice of the "inorganic" matters. We must be content with mere enumeration of their divisions; to wit, carbonic acid, chlorine, chlorides of sodium and potassium, sulphuric acid and sulphates, hyposulphurous and sulphurous acids (a chapter full of new and original matter), hydrothion and hydrosulphocyanic acid, phosphoric acid and phosphates (also a most important chapter), ammonia, trimethylamin, potassium, sodium, calcium, magnesium, and iron.

Chapter XXXII., on the occurrence of the anatomical elements of the blood, and that following on Cruenturesis, will repay careful study, especially on account of their spectroscopical data. The chapters on albumen and grape sugar are also of first importance.

The rest of the volume is occupied with the consideration of specially morbid aspects, and of substances of slight, less frequent, or abnormal occurrence in the urine; and the book is fitly brought to a close by two chapters on the systematic qualitative analysis of urine, urinary sediments, and calculi. The average composition of normal urine given in the table on p. 563 is particularly useful for purposes of comparison.

The slight exposition here given of this work will be sufficient to show its importance both to the medical and chemical professions.

Etna; a History of the Mountain and its Eruptions. By G. F. Rodwell. London: C. Kegan Paul and Co.

We have here a monograph of what must be recognised as the greatest plague of Sicily, always excepting the Maffia and the police of "Italia redenta." The work is an expansion of an article prepared for the "Encyclopædia Britannica," but although accessible authorities have been consulted it must not be regarded as a mere compilation, since the author ascended the mountain during the summer of 1877, and obtained much information on the spot.

The first chapter is devoted to a notice of the principal writers, ancient and modern, who have given an account of the mountain and its eruptions, among whom Lucretius, Brydone, the Canon Recupero, Dr. Jos. Gemellaro, Elie de Beaumont, Baron Waltershausen, and Sir C. Lyell are the most prominent.

The physical features of this celebrated volcano are next described. The circle of vision from the summit has the enor-

mous radius of 150·7 miles, so that, to quote the author's illustration, were it placed at Derby a spectator on its highest point would be able to see the sea eastward of Yarmouth, the Isle of Wight southwards, the Irish Channel on the West, and the country beyond Carlisle to the north. A very remarkable feature is the dense population of the habitable zone of Etna, amounting to 1424 persons per square mile, a proportion almost equalling that of Lancashire, and due doubtless to the great fertility of the region. A characteristic of Etna is the great number of minor cones and craters, from all of which fire and lava have at times issued. Of these Waltershausen has mapped no fewer than two hundred within a 10-mile radius from the summit crater. Among the chief organic features of the region are the gigantic chestnut trees of Carpinetto, which are no myths, but sober realities. The trunk of one, according to the author, is 25 feet in diameter, whilst a public road passes through the much-decayed stem of the largest, the *Castagno di Cento Cavalli*. The author gives a brief account of the flora of the mountain, and dismisses the fauna with the brief and somewhat hazardous remark that "of course it is the same as that of the eastern sea-board of Sicily."

The author's ascent of the mountain is next described. He recommends travellers who undertake a similar expedition to select, as did he, the middle of summer, as the cold on the summit has been declared by some more severe than anything experienced in the Alps. If this opinion is based on sensations experienced, it may be due to the abrupt change from the semi-tropical temperature of the sea-coast.

Like Brydone, the author appears to have been much struck with the clearness of the sky and the brilliance of the stars. The spectroscopic lines he found defined with wonderful distinctness. Hence it is very satisfactory to learn that an observatory is to be erected at the Casa Inglesi, 1200 feet below the summit. Mr. Rodwell gives a very interesting account of the sunrise, and of the curious optical effects produced. The shadow of the mountain was projected across the island a hundred miles away, and seemed suspended in space at or beyond Palermo. The villages at the base of the mountain "seemed painted on a vertical wall in front of us." The chief hardship experienced is the change of temperature.

We have next a description of the principal localities in the Etnean region, including the ancient city of Adranum, whose temple was once "guarded by a thousand dogs." Aci Reale is noteworthy for its seven beds of suposed lavas, one of the earliest discovered pieces of evidence against the chronology of Usher.

After noticing the seventy-eight recorded eruptions of Etna, our author turns to the geology and mineralogy of the region. "The oldest eruptions must have taken place during the Glacial

epoch, but before the period of greatest cold in Northern Europe." It is noteworthy that, notwithstanding its vastly superior magnitude, Etna is far poorer in mineral species than is Vesuvius.

We must here conclude our brief survey of a book which cannot fail to prove attractive at once to the general reader, the historian, and the man of science, especially as there exists no other separate work on the same subject.

First Annual Report of the United States Entomological Commission, for the Year 1877, relating to the Rocky Mountain Locust. Washington: Government Printing-Office.

The ravages of locusts have forced themselves upon the notice of mankind for some thousands of years; but in no country has this subject been dealt with in so thorough a manner as in the United States. Rightly judging that an accurate knowledge of the enemy must be the first step towards his destruction, an Act of Congress appointed a Commission to examine and report upon the "depredations of the Rocky Mountain locusts, and the best practicable methods of preventing their recurrence and guarding against their invasions." This Commission consisted, not as would have been the case in England, of lawyers, civil engineers, or sportsmen, but of three skilled and well-known entomologists—Messrs. Riley, Packard, and Thomas. The result has been the report now before us, which may safely be pronounced a mass of accurate evidence upon the ravages of the locust such as the world has not hitherto possessed. The national—and more than merely national—importance of the subject will best appear if we state that the direct and indirect losses sustained during the years 1874 to 1877 are estimated at not less than 200 million dollars, and that in certain districts immigration has been checked and the value of land depreciated. That the commissioners have been able to show how so gigantic an evil may be successfully combatted is strong testimony to the economical value of Entomology. It must not, however, be supposed that the report in question is merely utilitarian in its character. The authors have carefully investigated the distribution, the migrations, habits and vital conditions, the anatomy, embryology, and metamorphoses of the locust, and in studying its natural enemies—evertebrate as well as vertebrate—they have thrown useful side-lights upon a great number of animal species.

Much of the information here given will be no less useful in Australia, South Africa, &c., than in the United States, and the

Entomological Commission has therefore a just claim to public gratitude on both sides of the Atlantic.

Clavis Synoptica Hymenomycetum Europæorum. Conjunctis studiis scripserunt M. C. Cooke, A.L.S., et L. Quelet, M.D. London: Hardwicke and Bogue.

This work is a systematic account of the European species of plants belonging to the sub-order of the Hymenomycetes, one of the groups of Fungi to which the common mushroom belongs. The characteristics of each species are brief, rarely, if ever, exceeding two lines. The localities are not given, save that British species are marked with an asterisk in the margin. The great fault of the work is that it is written in Latin, a feature which might have been justifiable a century ago, when every man of scientific pursuits might safely be assumed to be a classical scholar, but which can now only be regarded as a survival of the unfittest.

The Freedom of the Truth. By Mungo Ponton, F.R.S.E. London: Longmans and Co.

From the title of this little work no very definite idea of its subject is likely to be gained. On reference to the Preface we find, however, that its aims are distinctly theological. The author attempts to show that "Christianity claims to be ranked as an experimental science, and that its principles ought to be studied by methods perfectly analogous to those followed in other experimental sciences. In this undertaking he makes a very frequent use of illustrations drawn from physics, chemistry, and biology, and in this latter department at least he scarcely shows himself as an unbiassed searcher for truth. There is the usual feeble attack upon the doctrine of Evolution, in which theological writers will still for some years think themselves bound to indulge. There is the somewhat stale anecdote of Lord Monboddo, and his belief that man was born with a rudimental tail, which "professors of the obstetric art" have to remove from newly-born infants. But Mr. Ponton does not seem to be aware that the real error of his "eccentric" countryman was his ignorance of the facts that the orang-outang and the gorilla are as free from outwardly visible tails as is man himself, whilst in all three species the internal rudiments of this dishonourable appendage are easily demonstrated. Our author, if he does not share this error, does not guard against being suspected of so doing, since

he simply writes that "man, being according to his views only a more perfectly developed variety of the ourang-outang, was probably born with a rudimental tail." Whilst pointing out the importance of keeping hypothesis in her proper place, he expresses his belief in "dominant ideas" whose "subtle and curious influence, united to the expectation of preconceived results, and the combined influence of these two on the brain and nerves," are to account for "all the remarkable phenomena of mesmerism, clairvoyance, and spiritualism." He does not, however, deem it necessary to enlarge on this topic, "seeing it has been so recently treated with great ability by Dr. Carpenter!" Mr. Ponton might perhaps have difficulty in finding a wilder hypothesis than that which he here adopts, nor does he rightly consider the terrible weapon which he is here putting into the hands of Materialists. For as the important "experiment" by which the Divine authority of the Scriptures is to be ascertained, he recommends prayer,—prayer in which we are to *expect* the fulfilment of our desires, our "affections and our emotions" being influenced. What reply, then, can he make if the Atheist turns his own arguments against him, and contends that the supposed answer to his supplications is merely the result of "dominant ideas" united to the "expectation of preconceived results," &c.? We do not, indeed, attach much importance to this argument, but then we are not accustomed to ignore facts, or what appear to be facts, without a careful and thorough scrutiny. To refuse such a scrutiny, even in cases where much error and delusion is probably mixed up with truth, we hold to be unworthy of Science.

Leisure Time Studies, chiefly Biological. A Series of Essays and Lectures. By Andrew Wilson, Ph.D., &c. London: Chatto and Windus.

We have here a collection of biological essays, pleasing, readable, and popular in the better sense of the word,—that is, intelligible outside of strictly professional and scientific circles, but not shallow nor inaccurate.

Perhaps the most interesting chapter is that devoted to "the Sea-serpents of Science." Taking for his motto the well-known lines "There are more things in heaven and earth, Horatio," &c., the author pleads for the reconsideration of a question which by some misfortune has been handed over to the jokers of the day to be dealt with at their indiscretion. In consequence of their exertion "*the* great sea-serpent—for no one ever condescends to speak of *a* sea-serpent—has been made the very type of mendacious exaggeration. It is even related that a

ship's captain, when told that some of his crew had sighted one of these mysterious monsters, absolutely refused to come on deck, since, to use his own words, 'Had I said I had seen the sea-serpent I should have been considered to be a warranted liar all my life after.'" It is surely much to be regretted that the elucidation of an interesting scientific question should be impeded or frustrated by childish ridicule. Following in the track of our late lamented Edward Newman, but with the advantage of having at his command much additional evidence, Dr. Wilson examines some of the principal alleged appearances of unknown sea-monsters, and points out the insufficiency of the explanations ordinarily given.

It is, in the first place, well known to zoologists—though perhaps not to the outside public—that veritable marine snakes inhabit the Indian and Chinese seas. These creatures are known to be very venomous, and have compressed fin-like tails adapted for swimming. More than forty distinct species have been captured and examined. Here, therefore, we have the proof that serpents inhabit the sea, and are met with at great distances from the coast. The only point, then, at issue is the size which a sea-serpent may attain. What length can we accept as possible, and what, on mere preconceived grounds, warrants us in pronouncing the narrator a liar? It must be remembered that marine animals are usually found larger than their nearest terrestrial allies. We know that boas and pythons have been found in South America and India not less than 30 feet in length. If, then, there are marine Ophidians bearing the same proportion to these that the whale does to the elephant, their size would not be smaller than that of the monster described by the crew of the *Pauline*. The only really valid piece of evidence against the existence of huge marine serpents is the fact that none of their bones have as yet been dredged up. Whether such a negative fact is sufficient to overpower the positive testimony of the various eye-witnesses may at least be doubted. Except there can be found in the narratives themselves something self-contradictory, we for one should at any rate lay aside our *a priori* notions, and vote for a suspension of judgment and the calm dispassionate collection of further evidence.

Dr. Wilson quotes from a weekly contemporary an explanation lately suggested. Dr. Jos. Drew saw a column of birds flying rapidly along the surface of the English Channel, and describes them as presenting the appearance of "an immense serpent, about a furlong in length, rushing furiously along at the rate of 15 or 20 miles an hour." Floating barrels, pieces of wreck to which sea-weed has become attached, and porpoises swimming in line are all said to have been, at first sight, mistaken for some huge oceanic serpent. Mr. Williams, in his interesting work "Through Norway with Ladies,"* suggests that low ridges of

* See Quarterly Journal of Science, vol. vii., p. 539.

rack stretching out into the Norwegian Seas may, under certain favourable conditions of the atmosphere, have seemed like the coils of a vast snake. We do not for a moment doubt but that all these, and perhaps other phenomena, may have occasionally passed muster for avatars of "the sea-serpent." But the question is, can all the described appearances be thus explained away? In three of the best-established cases quoted by Dr. Wilson—those of the *Dædalus*, the *Pauline*, and the *Georgina*—the problematical animal was seen either too close at hand or under such circumstances that all the above-mentioned hypotheses must be regarded as utterly inadmissible; and we have merely the alternative of branding the narrators as wilful and deliberate liars, or accepting their statements as substantially true. Now, great as may be the human tendency to falsehood, men of Teutonic or of Celtic blood may generally be depended upon when they have nothing to gain by a departure from the truth. What possible advantage could the crews of these three vessels hope to gain by the circulation of such narratives, and by what inducements were they brought to persist in the story in face of ridicule? We have heard, indeed, that the after life of Captain M'Quhæ, of H.M.S. *Dædalus*, was embittered, if not shortened, by the reflections cast upon his personal honour.

Our author, after describing certain other cases where the unknown animals seen were apparently other than Ophidian in structure, concludes "that many of the tales of sea-serpents are amply verified when judged by the ordinary rules of evidence; this conclusion being especially supported by the want of any *prima facie* reason for prevarication; that laying aside appearances which can be proved to be deceptive, and to be caused by inanimate objects or by unusual attitudes on the part of familiar animals, there remains a body of evidence only to be explained on the hypothesis that certain gigantic marine animals, at present unfamiliar or unknown to Science, do certainly exist, and that the existence of such animals is a fact perfectly consistent with scientific opinion and knowledge, and is most readily explained by recognising the fact of the occasional development of gigantic members of groups of marine animals already familiar to the naturalist."

A far stronger demand is made upon our powers of assent by the narratives concerning an underground monster of Southern Brazil—the "minhocao." Yet the eminent naturalist Fritz Müller, who is collecting evidence on this subject, sees reason for admitting that there is a substratum of truth in the exaggerated stories in circulation.

Will the time ever come when such books as the one before us will be to a great extent substituted for the literary rubbish, grave or light, which now fills the shelves of libraries and engrosses the leisure of the reading public?

Six Months in Ascension; or, an Unscientific Account of a Scientific Expedition. By Mrs. Gill. London: John Murray.

The Introduction to Mrs. Gill's "Unscientific Account" contains a short but most interesting history of the various attempts to measure our distance from the Sun, from that of Aristarchus of Samos, which gave about a twentieth part of the real value, to the plans of modern times, which are expected to come within one-tenth of a million of miles of the truth.

Mrs. Gill has said just enough to show some of the difficulties of this problem, and to increase the interest of her Expedition, without being technical and tedious; so that the most unscientific reader must read her Introduction with pleasure.

Mrs. Gill begins with a short description of the reasons and aims of her journey, and then goes on to their start at once, quite pathetically describing an accident that happened to Lord Lindsay's heliometer in Burlington House.

Ascension is an island in the Atlantic, about 800 miles north-west of St. Helena, and midway between the coasts of Africa and South America. It was discovered by Juan de Nova, on Ascension Day, 1501, whence it received its name.

No Government cared to take possession of 38 square miles of barren volcanic ash till 1815, when England, being afraid to leave a standpoint so near to St. Helena unoccupied, seized Ascension, and placed a captain and crew on board the *Flora tender*, as it is called in the "Naval Gazette."

The population consists of a company of marines, some St. Helena boys, and 70 or 80 Kroomen. The constitution is eminently monarchical, as the captain has the same power over his subjects as a captain of a man-of-war.

Mrs. Gill seems to have carried away the same impressions of Funchal as most other travellers; but as they were compelled to stay some time at St. Helena, we have an interesting description of that island, but for some uncouth Latin botanical names. *Polypodium*, for instance, is more like those dreadful things that frightened Hans Andersen's mermaid than a pretty fern.

Ascension gained, the first impressions of Garrison are not favourable. In spite of the Admiralty's gracious permission to buy meat, very little seems to be forthcoming, and that of a most peculiar quality. Hill, the servant, informs Mrs. Gill that as for sheep and bullocks "hardly any were killed that have not *fainted* first."

The first thing is to get the observatory up, of course; but no sooner is this done than envious clouds come in the way, and obscure the astronomer's view, till at last he decides to move his observatory, heliometer and all, across the "clinker," to another corner of the island,—a dangerous transit, for the clinker is rough, and the Kroomen insist on carrying the heliometer-case on their

heads. But all is accomplished safely, and a settlement is made at Mars Bay, as they call it, where the opposition of Mars is successfully observed.

But with the end of the work the end of the book by no means coincides. Excursions are made to the Green Mountain, Wideawake Fair, and other places of interest, which Mrs. Gill describes as pleasantly and chattily as St. Helena; and only after Christmas spent in Ascension do they return home.

It is impossible to do justice to the lively, easy style in which "Six Months in Ascension" is written. From the opening chapter, with the Professor's story, to the return home at the end, interest never once flags, and there is no inclination to skip a word.

But why say any more? The subject is interesting, and if it were not it would be made so in the telling. The book must be read to be appreciated.

Section Cutting. A Practical Guide to the Preparation and Mounting of Sections for the Microscope. By Dr. SYLVESTER MARSH. With Illustrations. 87 pp. London: J. and A. Churchill.

THIS unpretending little book, professing to be scarcely more than a compilation made with a view in saving the student's time in searching numerous books and periodicals, is one of a class of treatises of which we have only too few.

The paste and scissors work of collecting and putting together scattered articles only requires the exercise of a little judgment and patience; but to try every process described, and in many cases to give modifications and improvements, as the author of this treatise has undoubtedly done, shows that the writing of only eighty-seven pages may involve a vast amount of real and original work.

Part I. of the book treats of Section Cutting in general, and describes all the well-known processes, with a great many useful hints, which will render the work much more hopeful to those who are commencing this difficult branch of microscopical investigation. The method of treating those tissues which require preparation before cutting is given very fully, considering the small space at the author's disposal. The Section Cutter, or Microtome,* is described at some length, the author laying great stress upon the necessity of obtaining a good instrument if fine sections are required. The points to be attended to in selecting

* This term in some microscopical works is applied to a cutting instrument made like a small pair of sheepshears.

a microtone are thus stated:—"(1.) That the cutting-plate of the instrument should be made of glass, or, in default of this, of very hard metal of the most perfect smoothness. (2.) That the diameter of the tube be neither too large nor too small; it ought not to be less than five-eighths of an inch or greater than 1 inch. (3.) That the screw, which should be fine and well cut, be provided with a graduated head. (4.) That there be some kind of index by which fractional portions of a revolution of the screw may be measured. And (5.) That the plug fit the tube of the microtome so accurately that when melted paraffin, gum, or any other imbedding agent, be poured into it, it may not find its way between the plug and the side of the tube." Attention is called to a very common fault—neglect of any means of preventing the rotation of the cylinder of imbedding substance, and keeping it from separating from the plug, a defect which entirely destroys the power of cutting sections of uniform thickness. The description of the process of cutting, and also the use of the freezing microtome, is given in so plain a manner that a mistake in carrying out the process is hardly possible. The author gives his experience of knives—a very important matter, an unsuitable tool being the cause of very many failures. The description of staining processes and the best mode of mounting the finished sections have not been neglected. A valuable hint is given respecting the exclusion of air-bubbles in "balsam mounting" during the application of the cover. It is recommended to dip the cover in turpentine before it is applied, when it will be found that "you can't get air-bubbles, even if you try."

Part II. is devoted to special methods, and in the paragraphs headed Bone, brain, cartilage, coffee-berry, fat, hair, horn, intestine, liver, lung, muscles, orange-peel, ovary, porcupine-quill, potato, rush, skin, spinal cord, sponge, stomach, tongue, vegetable-ivory, and wood, the student will find a collection of useful directions hardly to be obtained elsewhere.

A few blank pages for notes complete this valuable little book, which will prove as indispensable to the working microscopist as the very practical treatises of Carpenter, Beale, and Davies.

Victoria; Reports of the Mining Surveyors and Registrars. Quarter ending December 31st, 1877. Melbourne: J. Ferres, Government Printer.

The total amount of gold obtained in the quarter, both from alluvial washings and quartz-reefs, is calculated at 218,159 ozs., and the exportation at 163,846 ozs. The grand total of mines employed in the gold-fields of the colony during the quarter is 38,055, so that the average quarterly yield of gold per man falls

short of 6 ozs.—a fact decidedly at issue with prevalent notions as to the lucrative character of gold-mining.

Flowers and their Unbidden Guests. By Dr. A. KERNER, Professor of Botany in the University of Innsbruck. With a Prefatory Letter by CHARLES DARWIN, F.R.S. The translation revised and edited by W. OGLE, M.D. London: C. Kegan Paul and Co.

THE part played by insects in the fertilisation of flowers since it was first distinctly pointed out by Mr. Darwin has given scope for a great amount of investigation, and has proved a delightful field of study both to botanists and entomologists. Indeed, it is difficult for any naturalist to enter upon this study without being compelled to notice simultaneously the structure of plants and the habits of insects, and thus to enlarge at once our knowledge of both. But whilst former observers have carefully ascertained the various contrivances by which self-fertilisation is prevented and by which insects are enticed to the flower precisely when their services are needed, Dr. Kerner has opened up quite a new phase of the mutual relations of plants and insects. All winged and creeping things are not alike welcome visitants to a flower. Those only of a certain weight, size, and shape, and, we may add, of certain habits, are likely to promote the great object of fecundation. Others would be useless or positively detrimental, and accordingly to prevent, as far as possible, their intrusion, we find a most interesting set of arrangements which the author here describes. He has thus succeeded in showing use and meaning in a vast number of floral structures which former observers had passed over as purposeless. By so doing he has invested the study of botany with new charms, and has encouraged men of science to persevere in their attempts to detect the function of parts not yet understood. It is interesting, however, to find that in this task Dr. Kerner has not been without a forerunner. Our lamented friend Mr. Belt, in his "Naturalist in Nicaragua," as Dr. Ogle points out in his preface, "distinctly recognises the fundamental point" of Kerner's memoir. "May flowers," he wrote, "have contrivances for preventing useless insects from obtaining access to the nectaries;" and again, "The structure of many (flowers) cannot, I believe, be understood, unless we take into consideration not only the beautiful adaptations for securing the services of the proper insect or bird, but also the contrivances for preventing insects that would not be useful from obtaining access to the nectar." This anticipation, however, is far from diminishing the interest and merit of the work before us. The idea which Belt first propounded Dr. Kerner has worked out in detail,

showing in a multitude of cases the various agencies by which "unbidden guests" are kept aloof. The author puts forth his work as a "small contribution" to the solution of one of the questions underlying the theory of natural selection. He complains that the phrase "the preservation of advantageous varieties" is often misused, and he recommends that instead of indulging in vague generalities on this preservation, we should solve the question at issue by the experimental method." No one can accuse him of neglecting to follow out his own advice. In his chapter on the advantages which accrue to the plant from bearing flowers, and especially from certain conformations of parts of the flower, we note that the word "biology" is used in a novel and, we fear, in a misleading sense. It generally signifies the entire science of living beings, but here we find it limited to "the determination of the functional significance of morphological characters. Dr. Kerner remarks very truly, "it is rare that any part of a plant is so shaped as to be suitable for the attainment of but one end," and he complains that naturalists have overshot the mark by trying to explain everything by the relation between the shape of the flower and that of the animals who visit it. In a special chapter he shows what insects and other animals are calculated to frustrate rather than subserve fertilisation, whether by destroying the flower, by consuming the nectar without rendering the desired services in return, or by preventing the visits of bees, moths, &c. Among such intruders a prominent part is played by ants, earwigs, and small beetles. Wingless insects are of little service, since even when they leave a flower laden with pollen, they cannot reach the flowers of a second stem of the same species until after a long journey and a proportionately long lapse of time.

The means of protection met with in different flowers against unbidden guests are explained in succession. Among these we find the secretion of distasteful substances, the hindrance of access by water, by viscid secretions, by prickles, hairy formations, by the diversion of visitors to other parts. The whole work forces upon our notice that constant strife which, whether we like it or not, must be admitted as pervading all nature. The flower seeks, as it were, to obtain the services of animals at the lowest market price. The animal, on the other hand, seeks to take the pay without doing the work, as when bees bite a side entrance into a flower so as to steal the nectar without effecting the desired exchange of pollen. Amidst such contrivances and counter-contrivances, every improvement on the one side being met by some fresh stratagem—if we may so term it—on the other, the notion of the "perfection" of every species, as inculcated by the old school of natural history, does not show to advantage. It reminds us of an old work in which the writer gave instructions for the "successful attack triumphant defence of any given fortress."

This charming work will, we are convinced, win new students for botany, and open out even to older observers fresh and fruitful paths. The editor has done his part well, and his preface and notes are a valuable addition to the volume. The work is illustrated with three double plates, well executed, and furnished with explanatory references. The general execution and appearance of the book are pleasing, and highly creditable to the publishers.

American Journal of Mathematics, Pure and Applied. Editor-in-Chief: J. J. Sylvester, LL.D., F.R.S.; Associate Editor-in-Charge: W. E. Story, Ph.D., with the co-operation of D. Pierce, F.R.S., in Mechanics, S. Newcomb, F.R.S., in Astronomy, and H. A. Rowland, in Physics. Vol. I., No. 1. Baltimore: the Editors; London: Trübner and Co.

The appearance of this Journal is a proof of the growing attention paid in the United States even to those sciences which do not appeal directly to the sentiment of utility. For the character of the work the names of the editors will be a sufficient guarantee. The present number contains a "Note on a Class of Transformations which Surfaces may undergo in space of more than Three Dimensions," by S. Newcomb; in which the proposition is announced that if a fourth dimensiom were added to space, a closed material surface (or shell) could be turned inside out by simple flexure, without either stretching or tearing. Mr. G. W. Hill contributes, "Researches in the Lunar Theory;" Dr. H. T. Eddy a paper on the "Theorem of Three Moments;" G. Weicheld, the "Solution of the Irreducible Case;" H. A. Rowland, a "Note on the Theory of Electric Absorption," in which he concludes that electric absorption will almost certainly take place unless the ratio of conductivity to the specific inductive capacity is constant throughout the body.

Next follows a notice by G. S. Pierce of Annibale Ferrero's exposition of the method of least squares. From the pen of the Editor we have an "Application of the New Atomic Theory to the Graphical Representation of the Invariants and Covariants of Binary Quantics." This paper commences with the explanation that "by the *new* atomic theory I mean that sublime invention of Keklulé, which stands to the *old* in a somewhat similar relation as the astronomy of Kepler to Ptolemy's, or the System of Nature of Darwin to that of Linnæus. Like the latter, it lies outside of the immediate sphere of energetics, basing its laws on pure relations of form; and like the former, as

perfected by Newton, the laws admit of exact arithmetical definition." We think this paper will be read by chemists not without interest.

Geological Survey of Canada. Report of Progress for 1876-1877. published by Authority of Parliament.

THE work done in these two years comprises an exploration of British Columbia, by Mr. G. M. Dawson; a survey of the coal-fields of Nanaimo, Comox, Cowitchen, &c., by Mr. J. Richardson; a report on the Goderich Salt Region, by Dr. T. S. Hunt; report on geological researches north of Lake Huron, and east of Lake Superior, by Mr. R. Bell; surveys of the iron ores, apatite and plumbago deposits of Ottawa County, by Mr. H. G. Vernor; an account of the "Albert Shales" of Albert and Westmoreland counties, New Brunswick; notes on miscellaneous rocks and minerals, by Dr. B. J. Harrington; and an examination of Canadian graphites, by Mr. C. Hoffmann, from which they appear for the purposes of the crucible maker to be equal to the Ceylonese quality. Indeed the mineral wealth of the dominion is greater and more varied than is often supposed. The annual production of gold in British Columbia is considerable, but it is unfortunately not stated in ounces, but merely in dollars' worth. The argentiferous leads of Eureka, and of the Van Bremer Mine, show, acording to the assays of Dr. Harrington and Dr. Hunt, 271·48 and 347·08 ounces of silver per ton of 2000 lbs. Various well-authenticated specimens of cinnabar, native amalgam, and native mercury, have been met with; and Dr. Blake has found nickel among the heavy materials separated from the fine gold of the Fraser River. The exploration of British Columbia is as yet, however, exceedingly imperfect.

Dr. Hunt finds the salt from the Goderich deposits remarkably pure, and calculates that every twenty acres of this layer, if mined out, would be sufficient to supply the whole yearly consumption of the United States.

The principal palæontological facts recorded relate to the fossil insects discovered by Mr. G. M. Dawson, at Quesnel, in British Columbia, and examined by Mr. S. H. Scudder.

Bulletin of the United States Geological and Geographical Survey of the Territories. Vol. iv., No. 2. Washington: Government Printing-Office.

This issue contains "Field Notes on Birds observed in Dakota and Montana along the 49th parallel, during the Seasons of 1873 and 1674," by Dr. Elliott Coues. The most interesting result is the capture of a newly-fledged Bohemian waxwing, showing that this species must breed at no great distance.

The next paper consists of "Notes on a Collection of Fishes from the Rio Grande, in Texas," by Dr. D. S. Jordan. Mr. A. R. Grote follows with "Preliminary Studies on the North-American Pyralidæ," Dr. C. A. White with "Descriptions of New Species of Invertebrate Fossils from the Laramie Group"—strata extending through Colorado, Wyoming, and Utah. The species collected, with the exception of a few insects and crustaceans, are all molluscous.

Dr. Hoffmann gives an interesting account of the mineral species found in Nevada.

The Scottish Naturalist: a Quarterly Magazine of Natural History. Edited by F. Buchanan White, M.D., F.L.S. No. 30. April, 1878. Edinburgh: Blackwood and Sons.

Is the increasing number of journals devoted to Natural History the "outward visible sign" of a corresponding increase in the amount of facts recorded and of work done? Or is it merely a distribution over a wider field of the subject matter formerly found concentrated in a few organs? If the former, as we trust, is the true supposition we wish these new comers every success.

The issue before us contains the continuation of an essay on Migration, by Col. Drummond Hay. It contains a number of interesting facts, but the author seems to take exception to Mr. Wallace's remarks on the dangers incurred by migratory birds in crossing the ocean upon mere *à priori* and teleological views. He considers that in "Nature's proper course there shall be no undue loss of life, and that everything should fulfil its own special purpose and maintain its own proper balance; and though man may disturb it by the ruthless destruction of many creatures, that in no way sets aside the design of a beneficent Creator."

It is difficult to comprehend the relevancy of such a passage, unless the author is seeking to contend that species never become scarcer or disappear without human intervention.

The papers on the "Lepidoptera of Moncreiffe Hill" and on "Glen-Tilt, its Fauna and Flora" are contributions towards the task of mapping out the range of every animal and vegetable species within this island—an operation which, as we learn from too many sources, is fast being simplified by the extirpation of many interesting animals and plants.

A significant paragraph by Mr. J. H. Gurney, of Norwich, points to the increase of wood-pigeons in Scotland, and the injury which they inflict upon crops, as facts not unconnected with the slaughter of 2642 individuals of the hawk tribe on three estates only. This is a fact which the author of "Game-Preservers and Bird-Preservers would do well to take into serious consideration.

The paper by Dr. Lauder Lindsay on the "Gold-fields and Gold-diggings of Crawford-Lindsay" has, perhaps, more of an antiquarian than of a mineralogical interest.

OBITUARY.

THOMAS BELT, F.G.S,

IN the obituary of the year, and amongst the list of scientific men who have loved science for itself, and sought truth for truth's sake, few will leave a brighter or happier memory with their friends than Thomas Belt.

Born in Newcastle in 1832, he was an early member of the Tyneside Naturalists' Club, and there began that love of nature and nature's ways that ever remained fresh with him throughout his life.

In 1852 his adventurous nature took him to the Australian Gold Diggings, and there (the leading spirit of a family of four brothers located in the colony), from 1853 to 1860 he successively visited, as a miner, the districts of Friars and Forest Creeks, Maryborough, Mount Molingul, Kingower, Korong, Mount Egerton.

In this rough "School of Mines" he acquired that practical knowledge which not only served him so well in after life in his profession, but gave him that insight into the building-up of the earth's crust which enabled him, not seldom, to put forth novel theories in geology and natural phenomena. Unorthodox as they were when first promulgated, yet, silently and solidly, they commended themselves to those who studed the facts and the inferences he drew from them. Amid real hard work in Australia, he found time to speculate on the flight of birds, and to show that the mechanical action of the bird's wings is not always the prime mover, but that the force of the wind, particularly in the Albatross, is the real agent that carries them sweeping over the ocean with the rapidity of the wind itself. Further, that this force is utilised by the faculty the bird has of balancing itself against the power of the wind. It is the equivalent of the string of the boy's kite, and almost overwhelming proof of this theory is afforded by the fact that the albatross is helpless in a calm, and cannot—from a level surface, as the deck of a ship—raise itself or fly so well as a domestic goose.

His theory of whirlwinds, viz., that the upper strata of air, pressed upon the lower rarefied and lighter strata, till a casual opening or thinning out in the upper layer leaves the lower strata free to fly upwards, and to form the circular whirlwinds common even in this country, was an outcome of his actual experience acquired in the dreadful dust storms of Australia. It is a curious fact that the paper on this subject, sent to a Melbourne scientific society, and put aside as unworthy of notice, was sent by Mr. Belt to the present Astronomer Royal, and then, as communicated by the Astronomer Royal to the Philosophical Institute of Victoria, was accepted and read in Melbourne, in December, 1857. The paper itself will be found in the "London, Edinburgh, and Dublin Philosophical Magazine" for January, 1859.

The boldest of his speculations, and one of the soundest, as after events proved, was his plan for crossing the Australian continent. He proposed, at the time the Government Expedition was mooted to replace the costly plans of the Government by the following scheme:—That he and his brother Anthony (who was unfortunately lost in the *Royal Charter*), should be conveyed to the Gulf of Carpentaria, with about twenty pack-horses loaded with provisions and water; that an escort should protect them for some twenty miles from the coast, and that then the two voyagers only, with their pack-horses should make their way to Cooper's Creek, the farthest known accessibl point from the Victorian settled districts. Belt argued justly: "If we fail, only two lives will be lost, but all the chances are in our favour; we are provided with water and food more than ample to cover the distance we have to travel. Every step of our road carries us homewards and to safety. If we

never find a drop of water on the road, our animals have enough to carry those who have to bear the whole journey to their goal, and as the animals succumb they will be shot or turned adrift." The event showed Belt's sagacity. The unfortunate Government Expedition left Melbourne loaded with camp followers and *impedimenta*, and by the time they reached a few stages beyond Cooper's Creek were well-nigh exhausted. Burke, the leader of the expedition, in desperation started with his two men, Wills and King, and bravely struck out for the Gulf of Carpentaria. Through desert and fertile plains, not altogether destitute of water, they reached in safety the northern shore of Australia; but the energy, the courage, and the strength that took them this long, weary journey did not suffice to carry them back over double the distance to their camp. Brave hearts! they struggled on, but King only, and as a worn out man, ever saw Cooper's Creek again. Belt's plan would have solved the problem without loss of life, and at a tenth of the cost. His ideas were in advance of his time, and he had that belief in his own powers which should have won his plan the attention its merits deserved. The writer knows the fact, that had Belt then possessed the means, he would have spent them all in his endeavour to carry out this scheme of crossing the Australian continent.

In 1862 Belt returned to England, and his professional engagements led him to North Wales, Nova Scotia, Central America, and Chontales Nicaragua. At the latter place his entomological collection has made him famous. Many hundred species of coleoptera and lepidoptera attest his energy and labour; and his charming book, "The Naturalist in Nicaragua," whilst illustrating his great powers of observation, has endeared him to every lover of nature, and proved the painstaking truth with which he collected his facts.

The succeeding years of his life were spent in almost continued travel: to North and South Russia, Siberia, the Kirghese Steppes, and many times to the United States. In these journeys he, from time to time, made those observations upon glacial action, upon which he built up his theories of the Ice Age. These became the ruling passion of his later years. Much of this work will be found in the "Quarterly Journal of Science" and in the "Quarterly Journal of the Geological Society." How much of it will stand the test of time the future only can tell; but all this special work of his is, at least, a careful and elaborate argument, advocating the theory that the extraordinary changes of climate in past ages, over large areas of the earth's surface which are now temperate regions, during the period called by geologists the glacial epoch, may have been brought about by other causes of less intensity than the submergences and emergences of the land, even than by the displacement of whole continents, which theories have been advanced by some to account for the phenomena in question.

Mr. Belt advocates the agency of ice, and ice dams, and great lakes—to use his own words—in place of "great upheavals and depressions of the earth's surface within a comparatively short period," and he questions the hypothesis by which "we are taught that an immense area in Europe and America has been a sea bottom, and every part of it a sea beach as the land rose again, without any evidence of marine life having been left behind;" and he claims that his theory of glacial action "explains all the phenomena by one great advance southwards of the ice of a single glacial period."—*Quarterly Journal of Science*, "Loess of the Rhine and the Danube," January, 1877.

The immediate cause of his death was brain fever, following a long attack of mountain fever. He died at Kansas City, United States, on the 22nd day of September, 1878, in his 46th year.

THE MONTHLY

JOURNAL OF SCIENCE.

FEBRUARY, 1879.

I. "PROGRESS,"

THE ALLEGED DISTINCTION BETWEEN MAN AND BRUTE.

IS man essentially progressive? Are all other animals as essentially stationary? The public opinion of these latter days and a number of eminent writers answer both these questions in the affirmative, trusting to find here the long-sought "great gulf."

To man they ascribe a threefold movement—of the individual, of the community, and of the species. To brutes they ascribe immobility. Disregarding the charge of "cynicism" which may perhaps be brought against us, we shall examine a portion at least of these assertions. We shall make bold to ask whether it is really true, as so often asserted, that man is from the cradle to the grave a "progressive being," still able and willing to learn, and to improve even to the very last? Do his views as he grows older become continually freer and wider, his insights deeper, and his prejudices feebler? The notion can scarcely be fairly and fully stated without revealing its utter absurdity. It is one of those errors which men assert in general terms, and yet deny daily in words as well as actions. Every observer of human nature, every man of experience, admits that the mind, like the body, up to a certain period increases in its powers, and afterwards gradually declines. Like the body, it becomes first less flexible, and then feebler. Figuratively speaking it is ossified. The epoch of this change varies not a little in different individuals. Yet in almost every man there comes a time when he ceases to grow mentally just as he has ceased to grow corporeally—when he no

longer looks forward; losing at once the power and the desire to observe new facts and evolve new theories, or even to recognise them if discovered by others. He becomes stagnant, and instead of the bold "plus ultra" of his prime he feebly murmurs "Rest and be thankful."

It is stated that not a single physician who had reached the age of forty, prior to the announcement of Harvey's discovery of the circulation of the blood, could ever be brought to recognise it as correct. World-betterers of all shades, moral and religious reformers who are labouring at the difficult task of improving man, continually bear testimony that "it is of little use attempting to elevate or amend the adults; we must turn our attention mainly to the children." In so saying they substantially admit our position. There are, indeed, men of vast and highly-cultivated intelligence, who, up to an advanced age, and even to the very close of their days, seem to go on thinking and discovering, to long for new truths, and to weigh every novel theory with candour and impartiality. But these are the very exceptions which prove the rule. Their biographers do not fail to point out this persistent energy of the mind as a proof of their exceptional nature and of their superiority to ordinary men.

Nor must we forget that many of the discoveries and other great works, emanating apparently from men far advanced in life, have in reality been wrought out in their earlier days, though the publication of the results, especially in a popular form, has been delayed. The power of expression, too, as a rule, survives by many years the power of origination.

But what of the normal average man? Is he not from the middle of life, and often indeed from an earlier date, a mere bundle of habits and prejudices? His interest in anything not immediately relating to self—where it ever existed—has dwindled. His mind has lost its suggestiveness. He shrinks from change of place, scene, and occupation. He dislikes new associates. He suspects new views, and imputes motives to their promulgators. With him, just as with the lower animals, no further mental growth is possible, however long he may happen to survive.

The truth here put forward has recently received full demonstration—if such were needed—at the hands of an American man of science, Dr. G. M. Baird. In his treatise* he shows that 70 per cent of the work of the world

* **Legal Responsibility in Old Age.**

is done before 45, and 80 per cent before 50. "The golden decade of a man's life (the interval between his thirtieth and fortieth year) alone represents nearly one-third of the work of the world. The advantage of the period from 20 to 30 over that from 50 to 60 is very striking, and will cause surprise."

In short, from a careful biographical examination, it fully appears that in the individual man—so far at least as his present life is concerned—there is no indefinite and continued progress. His powers of body and mind are developed together, and simultaneously—or nearly so—they decline. The same law applies to him as to the lower animals, and, if we take into account the different term of life of each species, all have their "golden epoch" about the same part of their career.

To man, then, brutes exhibit no well-marked contrast, but a decided similarity. In the earlier part of life they are, like ourselves, capable of progress; they observe, learn, and retain facts, and to a certain extent they draw conclusions from such observations. By-and-bye their faculties are blunted, and, like man, they become stationary. That in them stagnation may set in somewhat earlier than it does in civilised man is not impossible. But this is a mere difference of degree, not of kind. It is very easy for the rhetorician to exclaim—"The brute soon attains a degree of perfection which he can never surpass, and were he to live a thousand years he would still be the same." This is no less exact a portrait of the human species than of the gorilla, the dog, or the ant. As far, then, as individual progress is concerned, man and beast differ merely in degree.

There is also among mankind a national or tribal progress distinct from that of the individual and that of the species, but, like them both, not unlimited in its extent and duration. Historians tell us how a small community has, if unchecked by hostile circumstances, grown up into a great empire, and how, again, it has declined and come to ruin. For such national decay they assign a variety of causes, according to their political and social prepossessions. In one case the calamity is ascribed to luxury; in another, to the toleration of slavery or of usury; in others, to the importation of foreign-grown corn, to the non-recognition of "woman's rights," or to the existence of vivisection, and other the like springs, which to examine would be a departure from our purpose. But whatever share of blame may attach to such causes, we should seek for the main source of the evil in the

decay of that which in a nation corresponds to vitality in the individual,—that is to say, of the so-called "tribal instinct." When this instinct gives place either to avowed selfishness or to cosmopolitanism—and the two differ really more in seeming than in being—the doom of the nation is sealed, despite alike of its resources and its virtues.

Among those animal species which live in organised communities or nations the very same phenomenon occurs; increase and rise are followed by decline and fall. Every ant-hill might have its Gibbon. This fact has been fully established by the illustrious French chemist M. Berthelot, who for the last quarter of a century has devoted much of his leisure to a study of the manners of ants.* He noticed in particular a very prosperous ant-hill, which had established roads in all directions of more than 100 metres in length. After about ten years it sent out a colony which established itself at the root of a young oak at some little distance, and though feeble in the outset increased year by year, and passed safely through a very critical epoch, the periodical felling of the wood. The observations of M. Berthelot were temporarily interrupted by the war. On resuming his studies he found, to his surprise, the colony in full prosperity, but the mother-city declining. Its population had lessened, and continued so to do, and the survivors neglected their habitations. The former colony now takes the lead, and its new buildings and well-kept roads contrast strangely with the crumbling structures of the old formicary, which, however, is still inhabited.

We have ourselves observed a very similar case among rooks. An old and populous rookery from no apparent cause dwindled away, whilst a colony which it had founded in its early days gradually surpassed the mother-city, and became in turn the rook-capital of the district. Surely, then, we must admit that the cities and nations of brutes, like those of mankind, have their origin, their progress, and their decline. Here therefore, again, instead of contrast we find resemblance.

We turn now to what is in popular literature more generally known as "progress"—the real or fancied superiority of every generation of mankind over the foregoing. But this very superiority, though assumed in after-dinner speeches and in songs of the "good time coming" type, is a matter of most grave question. Even as regards knowledge and power, the advance which some claim as a

* Correspondance Scientifique, Oct. 1, 1878.

characteristic feature of humanity is effected by exceptional individuals who arise in certain races under favourable circumstances only, and is quite compatible with long intervals of immobility, and even of decline. Can a property so rare and fluctuating be made an essential point in any definition of man, or can it be used to establish a "great gulf" between him and the brute creation?

Further, it is nowise proved that the lower animals are literally incapable of progress. Our acquaintance with the habits and powers of most brute species, incomplete even at the present day, is far too recent to justify such an assumption. On the other hand, cases are not wanting which can scarcely be regarded in any other light than as inventions made, *e.g.*, by ants. Thus, from a work recently noticed in the "Quarterly Journal of Science,"* we quote the following incident recorded by Prof. Gredler, of Boston:—"One of his colleagues had for months been in the habit of sprinkling pounded sugar on the sill of his window, for a train of ants which passed in constant procession from the garden to the window. One day he took it into his head to put the pounded sugar into a vessel which he fastened with a string to the transom of the window, and, in order that his long-petted insects might have information of the supply suspended above, a number of the same set of ants were placed with the sugar in the vessel. These busy creatures forthwith seized on the particles of sugar, and soon discovering the only way open to them—viz., up the string, over the transom, and down the window-frame—rejoined their fellows on the sill, whence they could resume the old route down the wall into the garden. Before long the route over the new track, from the sill to the sugar by the window-frame, transom, and string, was completely established, and so passed a day or two without anything new. Then one morning it was noticed that the ants were stopping at their old place, the window-sill, and again getting sugar there. Not a single individual any longer traversed the path that led thence to the sugar above. This was not because the store above had been exhausted, but because some dozen little fellows were working away vigorously and incessantly up aloft in the vessel, dragging the sugar-crumbs to its edge, and throwing them down to their comrades below on the sill—a sill which, with their limited range of vision, they could not possibly see."

This is indisputably an instance of invention, and as all

* Flowers and their Unbidden Guests, p. 21.

man's progress in the industrial arts, in power, whether destructive or constructive, is ultimately due to invention, the alleged stationariness of the lower animals as compared with man can no longer be maintained.

II. MATTER DEAD.

"Elementa sunt facta de Yle."—ROGER BACON.

LET us suppose Roger Bacon to be a good representative of the philosophy that matter is one, and John Dalton as representing belief in the primordial diversity of atoms, and let each have a modern mouthpiece. We can imagine the following:—

Roger Bacon—I have read in a German book something like this; Give me an atom, and I make the world, life and thought included. This was not writing of the highest class there, but it was from observing the tone of the writing of some of the most talented men, and I certainly think it not quite unfair to draw such conclusions as the writer drew after imbibing for truth the thoughts of the most prominent men. Have I not seen, also, in the writings of one of the men considered first in rank, and certainly by himself as first in importance, that the finest feelings of the soul are all dependent on a certain fluid which we are left to suppose produces them.

To the first observation there is a short reply, and if any man founds his philosophy on such a supposed knowledge of the action of atoms he can gain confidence only from the most ignorant or the most illogical. We do not know how any atom acts, and the assertion is simply akin to the sayings of Baron Munchausen.

If to the second observation any one gives trust, he may be correct in a sense, as we may say no conscious thought occurs in man without water; but if any man will say that he knows why any certain substance, liquid or solid, acts in producing thought, he is a visionary, and does not belong to the class of sound scientific men: him also I put on my shelves with the boasting Baron. And yet how many men there are who suppose that these things are actually known,

whilst there are some who speak amongst us as if they knew them. Men assert the power of matter who have not to themselves clearly defined what matter is. Most men, even scientific men, speak as if it were only the substances known to us in chemistry as "the elements." Some will allow both elements and forces, or ponderable and imponderable matter, the imponderable acting as forces; but it is probably the opinion most prevalent that matter and force are one. To use the words of one of the most recent volumes, by a scientific man of careful thought,—"Wherever matter exists, energy (either potential or active) also exists (or matter is the seat of energy)—is another truth of the widest kind, based upon universal experience, and is inferred from the fact that energy has never been observed except in connection with matter; *i.e.*, in connection with that which possesses weight."

John Dalton—We cast the seeds into the earth, and they grow, collecting matter willingly, whilst matter readily flows to them. A tree grows, and we throw it into the fire; it acts with violence, and matter again collects about it and carries off all that had been so laboriously brought together. Surely matter is alive.

Is not the world in a constant state of motion itself, whilst its surface is restless, and much of it extremely agitated? Surely, too, every element is vigorous, scarcely any to be found in freedom, all attached servilely to some other, and by constant changings keeping up the proof of vitality. This world is far apart from other worlds, and yet it keeps in constant communication with them and shows its sympathy, which can only exist because of its life.

The elements in many cases cling violently to each other, and combination is accompanied by burning heat, which heat again goes to set other bodies in motion. Matter is therefore not only powerful, but it is a reservoir of power.

So active is matter that if it is freed from the violence of cohesive enchainment, as in a state of gas, every particle raves continually, and is in eternal wildest motion as constantly as the moving planets, if not so carefully regulated in every step. Here is eternal motion; here is an atom with an evident endowment of eternal life. If it has that now, must its eternity not have existed in the past also?

Indeed so full of activity is matter, that even when atoms are in combination and are apparently at rest, in the centre even of the everlasting hills, they are for ever revolving or in agitation in their very limited sphere.

Roger Bacon—Is all this true? This system has brought

about no true definition. So far as I can understand, most if not all the life is held to be in the definite atom; so far as many are concerned it certainly is. It has been supposed that there is one common atom as well as one substance; but it has been said that the atom possesses always more or less motion, due, it is assumed, "to a primordial impulse. This motion gives rise to volume; the more rapid the motion the greater the space occupied by the atom."

Let us look at the elements, and see if all is true. The metals are certainly not much inclined to combine amongst themselves. Mercury will attack some; but freeze it, and it is as quiet as the others, whilst soft metals are deadened by a similar treatment.

John Dalton—True the metals are cold-blooded, and require a hot climate to live in; but give them warmth, and you see another result. Even without warming them, put them to a cool liquid; platinum itself will give way before acids.

Roger Bacon—Yes, but freeze the acids, and I fear we come to the well-known saying, "Corpora non agunt nisi soluta."

John Dalton—But why trouble yourself with these frozen bodies? Take hydrogen and chlorine on the coldest day, and let them but get a glimpse of the sun, and they burst out with a joy unknown even to the violence of our youth. There we see the activity of elements.

Roger Bacon—True, but freeze your chlorine, not difficult to do; freeze your hydrogen into such a solid that it falls like a pebble on the floor. I fear they will have no joyful shout, but will not deign to recognise each other, even if the pieces are in apparent contact.

Nay more, take all the elements that you have in the world and freeze them to solids, throw them into a heap together as large as you please, and is it not almost certain from our not perfect knowledge that the action will be simply nothing. The elements are dead, the matter we see is dead, the matter concerning which men speak is dead. Here is an end; let that be left in peace.

John Dalton—That is easily granted if there is no warmth, but add only a small amount of heat to this mass of elements and will you not light up the whole? give even a little corner the light of a match and this will cause a combination, and this will produce heat, and this heat will warm up the next portion, and like a fire of the richest coal the whole will burn and spread heat to all around.

Here is the inherent power of the atom; it was not dead—it slept.

Roger Bacon—If so, what has become of the eternal motion of your atoms? what has become of the motion of your gases? what has become of the independent life of your matter? What power is in these masses to grow trees and thinking men? I thought you admired the theories of Lucretius, who tells us of the "primordia rerum," the very beginning of things moving gradually into complexity and evolving worlds and life. He was the earliest of the evolutionists whose system is well known to us, and he has many followers who somewhat modify his views. I thought you admired the self-initiating vortices by which are made all worlds, and the beautiful nebular theory of Laplace and of moderns now making worlds busily by this atomic eternal activity.

John Dalton—Not exactly. Moderns require heat, and heat itself is produced by atomic action, so that the action is enough after all.

Roger Bacon—Heat is motion. I thought it was agreed that the atoms would not move by themselves.

But as you require something more I will give it you. Indeed I will give you as much heat as you please near this corner, where you proposed to set the bulk on fire. Let there be a piece of any of our elements glowing white placed near. If heat is motion there must be something to move. By what means will this heat communicate motion to the elements?

John Dalton—Heat passes in a few minutes from us to the sun, and your small bundle of elements would soon absorb all you could send.

Roger Bacon—True it may be that heat passes to us rapidly, through space, but if it is only motion it must pass on something movable. Can you imagine it passing through empty space? Men used to think so when heat was to them a solid, but now there must be a something that is put into agitation. If your elements were quite alone they could not be fired; they must have that which we call ether, simply because it is so thin compared with our usual matter.

I will go farther. Even if your elements in combining produced heat, that heat would not be given out; to what place would it go? It could not go into empty space. What motion can there be in emptiness; can there be motion in nothingness? Your great fire would burn out, and the force would not be preserved. The waves would be lost for want

of the sea; or it would be a dash against an immovable shore, from which too there was no rebound.

I may even go farther still. Let us suppose some of the elements heated: I doubt if this heat could be communicated to the others. I do not know that an atom can touch an atom; if not the heat could not be communicated through the empty space. This ether, unknown as it is, may be required to bridge over the same distance between atom and atom. Where, then, is the force of your atom and its power of life? To live it requires the aid of a something only beginning to be recognised as matter, and the ingenious Lucretius and all similar thinkers to this day have speculated on an empty supposition. We have seen that our matter is dead unless it has heat, that heat cannot be conveyed to it unless by means of a something that we must suppose to exist, but whose existence is unintelligible to us, and which has none of the properties usually assigned to matter. Moreover, even if this something is allowed to exist in abundance it cannot give heat, but can only convey it.

John Dalton—I am willing to concede the ether as matter and the heat as motion; then I think we have all the preparation for the heat which you require.

Roger Bacon—If you concede the ether as matter you go before many physicists and chemists. You observe the quotation I made from a very recent book that matter is that which possesses weight. I never heard of the ether gravitating. You concede then the point that the elements known to chemists do not show any activity or vitality of themselves, and they must be warmed before they act. You allow that the elements have not initiated chemical activity, and that is a great concession, and I think it only an honest one demanded by the known facts. You allow that we must bring in motion from the exterior. You allow that when we have it in any amount it will never reach the elements, however near, unless the ether intervene. You allow also, I think, that even the motion of heat will in all probability not pass from molecule to molecule, let us say, or our present chemical atoms. Surely then it is not far wrong to say of our elements, regarding them as the total matter of the universe; matter is dead.

III. A CONTRIBUTION TO THE HISTORY OF ELECTRIC LIGHTING.

By W. Mattieu Williams, F.C.S., F.R.A.S.

As the subject of lighting by electricity is occupying so much public attention, and the merits of various inventors and inventions are so keenly discussed, the following facts may have some historical interest in connection with it.

In October, 1845, I was consulted by some American gentlemen concerning the construction of a large voltaic battery for experimenting upon an invention, afterwards described and published in the specification of "King's Patent Electric Light" (Letters Patent granted for Scotland, November 26, 1845; enrolled March 25, 1846).

Mr. King was not the inventor, but he and Mr. Dorr supplied capital, and Mr. Snyder also held a share, which was afterwards transferred to myself. The inventor was Mr. Starr, a young man about 25 years of age, and one of the ablest experimental investigators with whom I have ever had the privilege of near acquaintance.

He had been working for some years on the subject, commencing with the ordinary arc between charcoal points. His first efforts were directed to maintaining constancy, and he showed me, in January of 1846, an arrangement by which he succeeded in effecting an automatic renewal of contact by means of an electro-magnet, the armature of which received the electric flow, when the arc was broken, and which thus magnetised brought the carbons together and then allowed them to be withdrawn to their required separation, when the flow returned. This device was almost identical with that subsequently re-invented and patented by Mr. Staite (quite independently I believe), and which, with modifications, has since been rather extensively used.

Although successful so far, he was not satisfied. He reasoned out the subject, and concluded that the electric spark between metals, the electric arc between the carbons, and other luminous electric phenomena are secondary efforts due to the heating and illumination of electric carriers; that the electric spark of the conductors of ordinary electrical machines is simply a transfer of incandescent particles of

metal, which effect a kind of electric convection, known as the disruptive discharge; and that the more brilliant arc between the carbon points is simply due to the use of a substance which breaks up more readily, and gives a longer, broader, and more continuous stream of incandescent convection particles.

This is now readily accepted, but at that time was only dawning upon the understanding of electricians. I am satisfied that Mr. Starr worked out the principle quite originally. He therefore concluded that, the light being due to solid particles heated by electric disturbance, it would be more advantageous—as regards steadiness, economy, and simplicity—to place in the current a continuous solid barrier, which should present sufficient resistance to its passage to become bodily incandescent without disruption.

This was the essence of the invention specified in King's Patent as "a communication from abroad," which claims the use of continuous metallic and carbon conductors, intensely heated by the passage of a current of electricity, for the purposes of illumination.

The metal selected was platinum, which, as the specification states, "though not so infusible as iridium, has but little affinity for oxygen, and offers a great resistance to the passage of the current." The form of thin sheets known by the name of leaf-platinum is described as preferable. These to be rolled between sheets of copper in order to secure uniformity, and to be carefully cut in strips of equal width, and with a clean edge, in order that one part may not be fused before the other parts have obtained a sufficiently high temperature to produce a brilliant light. This strip to be suspended between forceps.

I need not describe the arrangement for regulating the distance between the forceps, for directing the current, &c., as we soon learned that this part of the invention was of no practical value, on account of the narrow margin between efficient incandescence and the fusion of the platinum. The experiments with the large battery that I made—consisting of 100 Daniell cells, with 2 square feet of working surface of each element in each cell, and the copper-plates about $\frac{3}{4}$ of an inch distant from the zinc—satisfied all concerned that neither platinum nor any available alloy of platinum and iridium could be relied upon; especially when the grand idea of subdividing the light by interposing several platinum strips in the same circuit, and working with a proportionally high power, was carried out.

This drove Mr. Starr to rely upon the second part of the specification, viz., that of using a small stick of carbon made incandescent in a Torricellian vacuum. He commenced with plumbago, and, after trying many other forms of carbon, found that which lines gas-retorts that have been long in use was the best.

The carbon stick of square section, about one-tenth of an inch thick and half an inch working length, was held vertically, by metallic forceps at each end, in a barometer tube, the upper part of which, containing the carbon, was enlarged to a sort of oblong bulb. A thick platinum wire from the upper forceps was sealed into the top of the tube and projected beyond; a similar wire passed downwards from the lower forceps, and dipped into the mercury of the tube, which was so long that when arranged as a barometer the enlarged end containing the carbon was vacuous.

Considerable difficulty was at first encountered in supporting this fragile stick. Metallic supports were not available, on account of their expansion; and, finally, little cylinders of porcelain were used, one on each side of the carbon stick, and about three-eighths of an inch distant.

By connecting the mercury cup with one terminal of the battery, and the upper platinum-wire with the other, a brilliant and perfectly steady light was produced, not so intense as the ordinary disruption arc between carbons, but equally if not more effective, on account of the magnitude of brilliant radiating surface.

Some curious phenomena accompanied this illumination of the carbon. The mercury column fell to about half its barometric height, and presently the glass opposite the carbon stick became slightly dimmed by the deposition of a thin film of sooty deposit.

At first the depression of the mercury was attributed to the formation of mercurial vapour, and is described accordingly in the specification; but further observation refuted this theory, for no return of the mercury took place when the tube was cooled. The depression was permanent. The formation of vaporous carbon was suggested by one of the capitalists; but neither Mr. Starr nor myself was satisfied with this, nor with any other surmise we were able to make during Mr. Starr's lifetime, nor up to the period of final abandonment of the enterprise.

When this occurred the remaining apparatus was assigned to me, and I retained possession of the finally arranged tube and carbon for many years, and have shown it in action worked by a small Grove's battery in the Town Hall of

Birmingham, and many times to my pupils at the Birmingham and Midland Institute.

These exhibitions suggested an explanation of the mysterious gaseous matter, which I believe to be the correct one, and also of the carbon deposit. It is this:—That the carbon contains occluded oxygen; that when the carbon is heated some of this oxygen combines with the carbon, forming carbonic oxide and carbonic acid, and a little smoke. I proved the presence of carbonic acid by the usual tests, but did not quantitatively determine its proportion of the total atmosphere.

If I were fitting up another tube on this principle I should wash it with a strong solution of caustic potash before filling with mercury, and allow some of the potash solution to float on the mercury surface, by filling the tube while the glass remained moistened with the solution. My object would be to get rid of the carbonic acid as soon as formed, as the observations I have made lead me to believe that—when the carbon stick is incandescent in an atmosphere of carbonic acid or carbonic oxide—a certain degree of dissociation and re-combination is continually occurring, which weakens and would ultimately break up the carbon stick, and increases the sooty deposit.

The large battery above described was arranged for intensity, but even then it was found that the quantity (I use the old-fashioned terms) of electricity was excessive, and that it worked more advantageously when the cells were but partially filled with acid and sulphate. A larger stick of carbon might have been used with the whole surface in full action.

After working the battery in various ways, and duly considering the merits of the other forms of battery then in use, Mr. Starr was driven to the conclusion that for the purposes of practical illumination the voltaic battery was a hopeless source of power, and that magneto-electric machinery driven by steam-power must be used. I fully concurred with him in this conclusion, so did Mr. King, Mr. Dorr, and all concerned.

Mr. Starr then set to work to devise a suitable dynamo-electric machine, and, following his usual course of starting from first principles, concluded that all the armatures hitherto constructed were defective in one fundamental element of their arrangement. The thick copper-wire surrounding the soft iron core necessarily follows a spiral course, like that of a coarse screw-thread; but the electric current or lines of force which it is designed to pick up and carry circulate

at right angles to the axis of the core, and extend to some distance beyond its surface. The problem thus presented is to wind around the soft iron a conductor that shall be broad enough to grasp a large proportion of this outspread force, and yet shall follow its course as nearly as possible by standing out at right angles to the axis of the armature. This he proposed to effect by using a core of square section, and winding round it a broad ribbon of sheet copper, insulated on both sides by cementing on its surfaces a layer of silk ribbon. This armature to be laid with one edge against one side of the core, and carried on thus to the angle; then turned over so that its opposite edge should be presented to the next side of the core; this side to be followed in like manner, the ribbon similarly turned again at the next corner, and so on till the core becomes fully enclosed or armed with the continuous ribbon, which would thus encircle the core with its edges outwards, and nearly at right angles to the axis, in spite of its width, which might be increased to any extent found by experiment to be desirable.

At this stage my direct co-operation and confidential communication with Mr. Starr ceased, as I remained in London while he went to Birmingham in order to get his machinery constructed, and to apply it at the works of Messrs. Elkington, who had then recently introduced the principle of dynamo-electric motive-power, electroplating, &c., and were, I believe, using Woolrich's apparatus, the patent for which was dated August 1, 1842, and enrolled February 1, 1843.

I am unable to state the results of his efforts in Birmingham. I only heard the murmurs of the capitalists, who loudly complained of expenditure without results. They had dreamed the same dream that Mr. Edison has recently re-dreamed, and has told the world so loudly. They supposed that the mechanically excited current might be carried along great lengths of wire, and the carbons interposed where required, and that the same electricity would flow on and do the duty of illumination over and over again, as a river may fall over a succession of weirs and turn waterwheels at each. Mr. Starr knew better; his scepticism was misinterpreted; he was taunted with failure and non-fulfilment of the anticipations he had raised, and with the fruitless expenditure of large sums of other people's money. He was a high-minded, honourable, and very sensitive man, suffering already from overworked brain before he went to Birmingham. There he worked again still harder, with further vexation and disappointment, until one morning he

was found dead in his bed. Having, during my short acquaintance with him, enjoyed his full confidence in reference to all his investigations, both completed and incomplete, I have no hesitation in affirming that his early death cut short the career of one who otherwise would have largely contributed to the progress of experimental science, and have done honour to his country. His martyrdom, for such it was, taught me an useful lesson I then much needed, viz., to abstain from entering upon a costly series of physical investigations without being well assured of the means of completing them, and, above all, of being able to afford to fail.

There are many others who sorely need to be impressed with the same lesson, especially at this moment and in connection with this subject.

The warning is the most applicable to those who are now misled by a plausible but false analogy. They look at the progress made in other things, the mighty achievements of modern Science, and therefore infer that the electric light—even though unsuccessful hitherto—may be improved up to practical success, as other things have been. A great fallacy is hidden here. As a matter of fact the progress made in electric lighting since Mr. Starr's death, thirty-one years ago, has been very small indeed. As regards the lamp itself no progress whatever has been made. I am satisfied that Starr's continuous carbon stick, properly managed in a true vacuum, or an atmosphere free from oxygen, carbonic oxide, carbonic acid, or other oxygen compound, is the best that has yet been placed before the public for all purposes where exceptionally intense illumination (as in lighthouses) is not demanded. It is the steadiest, the cheapest, and least glaring in proportion to the amount of light it radiates. It has not been "pushed" like other devices, simply because it is nobody's exclusive property.

Comparing electric with gas lighting, the hopeful believers in progressive improvement appear to forget that gas making and gas lighting are as susceptible of further improvement as electric lighting, and that, as a matter of fact, its practical progress during the last forty years is incomparably greater than that of the electric light. I refer more particularly to the practical and crucial question of economy. The byproducts, the ammoniacal salts, the liquid hydrocarbons, and their derivatives, have been developed into so many useful forms by the achievements of modern chemistry, that these, with the coke, are of sufficient value to cover the whole cost of manufacture, and leave the gas itself as a volatile residuum that costs nothing. It would actually and

practically cost nothing, and might be profitably delivered to the burners of gas consumers (of far better quality than now supplied in London) at one shilling per thousand cubic feet, if gas making were conducted on sound commercial principles,—that is, if it were not a corporate monopoly, and were subject to the wholesome stimulating influence of free competition and private enterprise. As it is, our gas and the price we pay for it are absurdities; and all calculations respecting the comparative cost of new methods of illumination should be based not on what we *do* pay per candle-power of gas-light, but what we *ought* to pay and *should* pay if the gas companies were subjected to desirable competition, or visited with the national confiscation I consider they deserve.

Having had considerable practical experience in the commercial distillation of coal for the sake of its liquid and solid hydrocarbons, I speak thus plainly and with full confidence.

There is yet another consideration, and one of vital importance, to be taken into account, viz., that—whether we use the electric light derived from a dynamo-electric source, or coal-gas—our primary source of illuminating power is coal, or rather the chemical energy derivable from the combination of its hydrogen and carbon with oxygen. Now this chemical energy is a limited quantity, and the progress of Science can no more increase this quantity than it can make a ton of coal weigh 21 cwts. by increasing the quantity of its gravitating energy.

The demonstrable limit of scientific possibilities is the economical application of this limited store of energy, by converting it into the demanded form of force without waste. The more indirect and roundabout the method of application, the greater must be the loss of power in the course of its transfer and conversion. In heating the boiler that sets the dynamo-electric machine to work, about one-half the energy of the coal is wasted, even with the best constructed furnaces. This merely as regards the quantity of water evaporated. In converting the heat force into mechanical power—raising the piston, &c., of the steam-engine—this working half is again seriously reduced. In further converting this residuum of mechanical power into electrical energy, a further and considerable loss is suffered in originating and sustaining the motion of the dynamo-electric machine, in the dissipation of the electric energy that the armature cannot pick up, and in overcoming the electrical resistances to its transfer.

I am unable to state the amount of this loss in trustworthy figures, but should be very much surprised to learn that, with the best arrangements now known, more than one-tenth of the original energy of the coal is made practically available. This small illuminating residuum may, and doubtless will, be increased by the progress of practical improvement; but, from the necessary nature of the problem, the power available for illumination at the end of the series must always be but a small portion of that employed at the beginning.

In burning the gas derived from coal we obtain its illuminating power *directly*, and if we burn it properly we obtain nearly all. The coke residuum is also directly used as a source of heat. The chief waste of the original energy in the gas-works is represented by that portion of the coke that is burned under the retorts, and in obtaining the relatively small amount of steam-power demanded in the works. These are far more than paid for by the value of the liquid hydrocarbons and the ammonia salts, when they are properly utilised.

In concluding my narrative I may add that after Mr. Starr's death the patentees offered to engage me on certain terms to carry on his work. I declined this, simply because I had seen enough to convince me of the impossibility of any success at all corresponding to their anticipations. During the intervening thirty years I have abstained from further meddling with the electric light, because all that I had seen then, and have heard of since, has convinced me that—although as a scientific achievement the electric light is a splendid success—its practical application to all purposes where cost is a matter of serious consideration is a complete and hopeless failure, and must of necessity continue to be so.

Whoever can afford to pay some shillings per hour for a single splendid light of solar completeness can have it without difficulty, but not so where the cost in pence per hour per burner have to be counted.

I should add that before the publication of King's specification, Mr. (now Sir William) Grove proposed the use of a helix or coil of platinum, made incandescent by electricity, as a light to be used for certain purposes. This was shown at the Royal Society on or about December 1st, 1845.

IV. THE PROBLEM OF FLIGHT: BALLOONING IN ARCTIC EXPLORATION.

THE proposal to employ balloons in Arctic Exploration has recently attracted more general attention to the hitherto unsolved problem of artificial flight. The methodical study of the laws of natural flight, which has been pursued for several years by Dr. Pettigrew, Prof. Marey, and M. Dupuy de Lome, has cleared up many disputed points. The researches of these gentlemen have already been fully described and illustrated in the "Journal of Science." Our object in again directing the attention of our readers to the subject is not therefore to refer to their labours, but to give a brief account of the more recent experiments of Mr. Brearey, the indefatigable Secretary of the London Aëronautical Society. Mr. Brearey has been studying the subject for some years, and has communicated to the Society the results he has obtained, illustrating his paper by models the bird-like action of which becomes, as Mr. Glaisher remarked, very interesting when we consider that it is produced by mechanism. By following up these experiments our knowledge of the subject must of necessity be largely increased, if the problem of flight be not actually solved.

Mr. Brearey first illustrated flight by means of models projected by the hand. The first experiment demonstrated flight by gravity alone, showing how the bat, hanging by its claws, by simply releasing itself attains its first flight.

Then he showed how the application of force neutralises the force of gravity. In this model the screw propelled a plane surface, which was represented by wings. It was made after the model of M. Penaud, of the French Aëronautical Society, improved as to the screw by Mr. Brearey.

To show that different forms of surface may be employed with instructive results for future work, the author in the next model adapted the albatross form of wing, the model being about half the length, viz., 7 feet, but the breadth being only one-fourth, or 2 inches, that of the albatross being about 8 inches.

This class of experiment may be greatly varied with a view to ascertain the weight which can be carried under a given surface. According to Mr. Brearey the angle of in-

clination with which the wing advances will have to be increased with the weight, and also the force in the same relative proportion.

It is asserted by some naturalists, in explanation of effective wing action, that the feathers of a bird's wing are made to underlap each other, so that in the downward stroke the pressure of the air closes them upwards against each other, and converts the whole series into one connected membrane, through which there is no escape; whilst in the upward stroke the same pressure has precisely the reverse effect. "It opens the feathers," says the Duke of Argyll, "separates them from each other, and converts each pair of feathers into a self-acting valve through which the air rushes at every point." The Duke, in his "Reign of Law," remarks Mr. Brearey, so thoroughly recognises, in another place, the immense importance of the concave and convex surfaces in gripping the air in the one case and evading it in the other, that he can scarcely lay much stress upon the valvular system of feathers. Dr. Pettigrew, whose researches give weight to his statement, estimates this difference as two to one.

To show that in the wing propeller the convex and concave arrangement is most effective, Mr. Brearey stood upon a pivoted stool, and holding the artificial wing perfectly level, waved it up and down, by which action he was caused to revolve.

He then proceeded to the practical application of the concave-convex theory by exhibiting a model after the construction of M. Penaud, of the French Society.

Experimenting with various forms of wings, he has been enabled to imitate the leisurely flight of the crow and the swift flight of the swallow. Mr. Brearey also made some observations upon the vertical screw, again resorting to M. Penaud's Helicoptère in illustration.

Passing to the question of the use of balloons in Polar Exploration, the author remarked that the balloon has a sphere of its own quite independent of its shape, unapproachable by any other invention, and the question for discussion is—Was the late Polar Expedition such an opportunity as afforded any reasonable chance for the useful employment of the balloon?

This question can never be fairly answered unless a balloon, with the necessary means for its inflation, form part of the vessel's equipment.

All the materials and apparatus being conveyed to the place of destination, there should be no difficulty in the

inflation. Giffard's balloon, exhibited in 1869 at Cremorne, was inflated with pure hydrogen, and could carry upwards of 16 tons.

The balloon successfully inflated, then what would be the work expected from it?

Instead of a seventy days' journey, to accomplish about 70 miles, at a fearful cost of life and suffering consequent upon having to drag over ice hummocks sledges containing provisions, as described in the Arctic Report, Mr. Brearey argues that the whole of the stores could have been conveyed over the heads of the explorers, and the men holding the ropes of this floating observatory would have been assisted by the upward tendency of the balloon. The question is—Would the daily consumption of stores compensate the leakage of gas? Major Beaumont, in his history of the Balloon as employed in the American War, says "that the balloon when inflated can, unless in very windy weather, be very readily carried. Twenty-five or thirty men lay hold of cords attached to the ring and march along, allowing the machine to rise only sufficiently to clear any obstacle. He had frequently," he says, "seen it carried thus without the least difficulty." He further says "that there was always a small amount of leakage, but, from the superiority of the varnish, at the end of a fortnight, sufficient gas remained in the balloon to enable an ascent to be made without its being replenished." The ascensive power of a balloon thus conveyed for purposes of war must be available at any moment for the two observers, and the additional weight of the two guy ropes which it also has to sustain, so that the necessity for the twenty-five or thirty men is explained; but for the purposes of exploration and the carrying of stores a very few pounds of ascensional force need be requisite. These stores, however, upon being removed from the balloon; or the sledges, which might be partly buoyed by the balloon, being detached; could not the balloon then be utilised to survey the country from some thousand feet or more by means of a let-out cord?

An interesting discussion followed the reading of Mr. Brearey's paper, in the course of which Mr. Reece remarked that there would be no fear of the efficacy of hydrogen. After it was generated it would pass through ice, or would be so cold that it would maintain the same temperature throughout the journey. Hydrogen gas would be generated at a heat of 180°. It would then pass through a tube and be chilled, and would remain at a temperature of about 32°, so that there would be no fear of its depositing a mass of

snow or ice. Mansfield, in his work on ballooning, suggested that the weight of a man might be taken off by ballooning. A balloon of 18 feet diameter would take off the weight of a man; and in this way a man named Ward was able to leap in the forest, from tree to tree, with a velocity of 15 miles an hour. In this case a man might guide a sledge of dogs at a great pace, and could convey stores by this means to any point.

With regard to the nature of hydrogen gas in a severe frost, Mr. Reece said he had submitted the gas to intense cold, and it appeared to have no effect upon it. Faraday exposed it to cold 100° below zero and a pressure of 800 atmospheres, and never found that either had the slightest effect upon it; neither had the most intense cold or pressure that he could produce at the Royal Institution.

Mr. Simmons remarked that the hot-air balloon seemed to be the best adapted to the especial object—

1. Because in the presence of intense cold, wind does not exist, wind being the chief drawback to the inflation of hot-air balloons in England. (Mr. Simmons's experience in this matter was confined to Canada; he had not visited the Arctic Regions.)
2. Because the more intense the cold of the air surrounding the balloon, the greater the ascending power.
3. The hot-air balloon during inflation will give off sufficient heat from its surface to keep the men warm whilst they are holding the net, and when the balloon is afloat no inconvenience can be experienced from cold.
4. The time required for the inflation of the hot-air balloon is about half-an-hour, and the preparation of the apparatus for the inflation will never be found so troublesome or occupy so much time as that for the hydrogen balloon.
5. The danger and annoyance from carrying oil of vitriol will be obviated.
6. Hot-air balloons have no preparation spread upon their surfaces that can sustain any injury, decomposition, or spontaneous combustion from being closely packed for a lengthened period.

The entire weight of the balloon apparatus used at the Royal Arsenal, Woolwich, was 1200 lbs., its diameter was 70 feet, and the heat when inflated, taken 10 feet above the open neck of the balloon, was 120° F.

The greatest difficulty against the inflation of a balloon with pure hydrogen gas in intensely cold regions would, in

Mr. Simmons's opinion, be the keeping of the water in the retort from freezing whilst charging or after being charged with water, until the vitriol is poured in. The process of making pure hydrogen gas by means of furnaces would necessitate the employment of exceedingly cumbersome apparatus.

Mr. Reece, alluding to the formation of hydrogen, remarked that, according to Sir George Nares, the average temperature during the Expedition in the Arctic Regions was 30° F. That would not have the slightest effect on a composition of 1 part sulphuric acid and 4 parts water. When poured on zinc the temperature would rise to 180°. If anyone tried that in a glass vessel he could not keep his hand on it, so that any fear of not generating the gas must be entirely visionary. Air expands only one 480th part for each degree, so that great heat would be required for an air balloon. A fire balloon had enormous power, but nothing like one filled with hydrogen gas.

Mr. Simmons remarked that the heat generated in a hot-air balloon would be 120°. The weight of a balloon and all its paraphernalia might be 1200 lbs., the diameter 70 feet, and it would carry a man into the air if the average heat were 120°.

Mr. Glaisher, in thanking Mr. Brearey for his paper, observed that in the late Arctic Expedition everything was excluded that was possible to be excluded on account of the want of room. If a balloon of 70 feet diameter had to be taken out, a very large space would be required. Again, it could only be used in summer time, when there is wind in the Arctic Regions. In Russia and Sweden in winter time, when the temperature approaches zero, it is nearly always calm. To realise the intensity of the cold one must move the hand against the cold air or run against the air. No person standing in an atmosphere 70° below zero would feel that the cold was so intense. It might be far more painful when the temperature was above zero if the air were in motion; but the winter was not the time when these experiments would be made: they would take place in the summer, when the temperature would be 40°, and in the sun very much hotter. He (Mr. Glaisher) saw no reason, however, why the balloon should not be made available in various ways in Arctic Exploration, and he hoped that if there was another Expedition the balloon would be tried and the question settled. It would certainly, if used in connection with a sledge, enable the distance that could be traversed in the day to be increased. With regard to the view that

could be obtained from the balloon: when he was half a mile over London he could see Margate and Brighton, and on to the Norfolk coast. This showed how much may be seen from a comparatively small elevation. From the height of a mile he could see nearly 90 miles, and even when a few hundred feet high one was in a position to see over the country for several miles ahead. In any case he hoped that in the next Expedition, from whatever country it may proceed, not only one balloon but several balloons would be taken out.

V. ELECTRIC LIGHTING BY INCANDESCENCE.

To determine the Electromotive Force required to maintain a rod of a given material, of given dimensions, at a given temperature, when loss of heat by radiation, convection, and conduction are taken into account.

By Prof. W. E. AYRTON.

HAVING found it necessary to solve the above question in connection with the possibility of economically illuminating, by a material rendered incandescent with the electric arc (the system employed by Edison, Wedermann, Sawyer-Man, &c., and *first* patented in England by De Moleyns in 1841, and not, as commonly supposed, by King, since his patent bears the date of 1845), I have thought that the results obtained may be generally interesting.

The problem of the flow of heat through a bar from which there is loss of heat by radiation and convection has, since the time of Fourier, been often treated; the mathematical investigation was employed by Forbes in his experiments made to determine whether the heat conductivity of iron, like the electric conductivity, varied with the temperature, and exactly similar equations have been made use of in studying the passage of an electric current through a wire from all points of which there is leakage, as in an ordinary submarine cable, and especially in a submarine cable made of inferior gutta-percha. The problem, however, we have on the present occasion to deal with differs from the preceding in that there is, at every point, not only loss of heat by conduction, radiation, and convection, but, in addition,

a generation of heat due to the resistance opposed to the passage of the electric current.

The method of solution that must be employed consists in first calculating the amount of heat that will be lost per second by conduction, convection, and radiation in each very small section of the bar, as well as the amount generated per second by the electric current in the same section. Then, when the temperature of the bar has settled down into a constant state, that is when the temperature of any one point does not vary from second to second (although the temperature of different points of the bar may be very different at the same time), the total loss and the total generation of heat per second in any one section must be equal. This equality leads to a differential equation, the solution of which is an exponential formula, connecting the strength of the current required with the temperature of the middle point of the wire, its dimensions, specific resistance, heat conductivity, and surface emissivity.

In order to apply this formula to the two very important cases of illuminating by an incandescent platinum wire, or by an incandescent carbon rod, it is necessary to know the surface emissivity, or heat radiating power, of the substance of our incandescent wire.

Now probably no experiments have yet been made to determine the *absolute* loss of heat per unit of surface from a body of a given temperature when the difference of temperature between the heated surface and the walls of the room exceeds 100 or 200 degrees centigrade. And, in fact, when this difference is even less than 100 degrees the *accurate* information at our disposal is scanty. Fortunately, however, the law of radiation of heat is such that unless there is a total discontinuity in the phenomenon as the temperature rises we are enabled to make a very good rough estimate of what this absolute amount of radiation must be, even at very high temperatures, such as that of white-hot charcoal.

Formerly, it was thought that Newton's law of cooling, viz.:—that the rate of loss was simply proportional to the excess of temperature, was correct; next the experiments of MM. Dulong and Petit seemed to show that in a vacuum the rate of loss was proportional to $m \; 1{\cdot}0077_{\theta} \; (1{\cdot}0077^{t}-1)$ where m is a constant depending on the radiating surface, θ the temperature of the enclosure, and t the excess of temperature of the hot body; while those of Mr. Hopkins led to the result that the loss by the convective action *alone* of the gas in which the heated body was placed was proportional

to $m\,p_a\,t^{1\cdot233}$ where m and a depend on the nature of the gas at a pressure p, and t, as before, is the excess of temperature.

The preceding formulæ do not, however, give results agreeing well with more recent experiments. In the "Proceedings of the Royal Society" for 1872, Mr. McFarlane published the results of his experiments on the absolute amount of radiation and convection from a polished and from a blackened copper ball in air. In addition to the fact that these experiments were made probably under the eye of Sir William Thomson, to whom Mr. McFarlane is an assistant, and that, therefore, they are probably correct, I have verified them experimentally myself, and extended them to other gases than air, and at various pressures.*

Mr. McFarlane's experiments show that the measured surface emissivity increases *less* and *less* rapidly as the difference of temperature rises. Probably, therefore, we may be safe in saying that for a difference of temperature of 1000° C. (about the temperature of melting cast-iron), the value for platinum and carbon will not be very much higher than the highest he obtained, respectively, for polished and for blackened copper.

From experiments made in a vacuum, I find that when the difference of temperature is under 100° C. the value of the surface emissivity is about half, and for gases like coal-gas about double that obtained by Mr. McFarlane in damp air at ordinary pressure.

In some of the systems of electric lighting it has been proposed to enclose the incandescent rod in a vacuum, in others in some non-decomposable gas, like nitrogen. In this latter gas I have not hitherto made absolute radiation and convection experiments, but we may fairly take as a mean value of this surface emissivity the air value previously referred to.

The specific electric resistance of platinum is a comparatively constant quantity, while that for carbon varies within wide limits. It, therefore, becomes an important question to determine what kind of carbon, that which conducts electricity pretty well, or that which is a much worse conductor, is the more suitable to be used for illumination by incandescence. Now the temperature equation previously referred to, shows that for a given electromotive force maintained at the two ends of a carbon rod, the smaller be the specific resistance of the carbon employed, the greater will be the heat, and, consequently, temperature produced.

* The results of these experiments of Professor Perry and myself will shortly be published.

Of course, this is on the assumption that the heat conductivity does not vary with the electric conductivity, and this seems to a certain extent to be the case, since, while we find the electric conductivity of carbon varying between very wide limits, the heat conductivity, as far as results of experiments are available, seems to be much more constant.

At first sight it might appear inconsistent with ordinary experience to say that for a carbon rod of given dimensions the smaller the specific resistance the greater the quantity of heat developed, but it must be remembered that here we are considering a *constant electromotive force* applied at the two ends of the rod, whereas in ordinary practice it is a *constant current* we have to deal with, since when even a Grove's battery is employed to produce the current through a *short continuous* rod of carbon, the chief resistance is in the battery itself, so that the current is practically independent of changes of resistance in the carbon.

Consequently, for this investigation, it is better to employ a form of carbon opposing a low resistance, than one offering a high resistance to the passage of the current.

Substituting in our equation the known values of the heat conductivity, &c., of platinum and carbon, we arrive at the result that if our incandescent wire be about No. 20, Birmingham wire gauge, or have a diameter of about one millimetre, and if its length be five centimetres in the case of the platinum and two in the case of the carbon (this longer length in the first instance being necessary on account of the better conducting power of the platinum), then the electromotive force necessary to be maintained at the two ends of the wire is—

0·2848 volts, or roughly one-third of that of a Daniell's cell, in the case of the platinum, and

0·46013 volts, or roughly one-half that of a Daniell's cell, in the case of the carbon wire.

It is, therefore, quite possible to produce a light with an electromotive force far less than that of a single Daniell's cell, but not, of course, with a Daniell's cell itself, since the resistance of this would be incomparably greater than that of our incandescent rod or wire.

Although it requires 50 per cent more electromotive force to maintain the centre of our two-centimetre rod of carbon at 1000° C. than it does to maintain the centre of our five-centimetre length of equally thick platinum wire at the same temperature, it is probable that the shorter carbon rod would give out more light—first, because, in consequence of the inferior heat conductivity of carbon, the temperature curve

would be flatter in the middle and steeper at the ends, so that the "surface integral" of the temperature would be higher in the case of the carbon than for the platinum; secondly, because a substance like carbon, which is black when illuminated by an extraneous light, gives out more light than a bright substance like platinum, at the same temperature, when sufficiently heated to become self-luminous; a phenomenon easily tried by placing in the flame of a spirit lamp a piece of platinum foil on which a black spot has been made with Indian ink.

In the preceding investigation no reference has been made to the value of the specific heat of the substance rendered incandescent; and reflection will show that, if a continuous light be required, the specific heat can (contrary to what has been often said) have nothing to do with the question; for, if the electric current only produces per second as much heat as will be lost per second by radiation, convection, and conduction after the body has reached a temperature of 1000° C., then it must necessarily reach this temperature, since at all lower temperatures it will gain more heat than it loses.

If, however, instead of a constant light, it were, for any purpose, desired that a rapid succession of lightings and extinguishing of the lamp should be possible, then, undoubtedly, it would be well to employ, for the incandescent rod, a substance having a small specific heat.

I found it not difficult, in 1873, when designing, at the request of the Government, an electric lamp for the use of the divers who recovered the property sunk in the French Mail steamer *Nil* off the coast of Japan, to make such thin carbon wires as I have spoken of in this paper. The process I employed was first to scrape an ordinary carbon rod very thin, then fix it to the two electrodes inside a glass globe from which the air could be exhausted. A strong electric current was now passed through the rod, which was burnt to the required degree of thinness by the air pumped through the globe. The current was then stopped, the air pump out, and replaced by nitrogen, when the globe was sealed up.

Another plan I employed, on account of the fragile nature of very thin carbon, was to use platinum wire coated with carbon, and rendered incandescent in an atmosphere of nitrogen, but it was difficult to prevent the carbon coming off the smooth platinum wire.

VI. INSTINCT OR REASON?*

HERE we wish to place on record a narrative of a most singular occurrence witnessed by the writer and a friend while hunting ducks at Bean's Lake, in Piatte County, Missouri. It will be found a most vivid illustration of the wonderful intelligence of a bird whose habits of solitary life have kept it beyond the ordinary observation of men.

While sitting behind a "blind," over our decoys, waiting for game, on the 3rd day of April, 1875, we saw, at the distance of a mile toward the west end of the lake, a large number of huge white birds, and, watching them curiously, finally discovered that they were coming toward us on the water—a host. In an hour the mass was abreast of us, and proved to be made up of pelicans, a band we estimated to be 60 feet in width and 300 feet long closely packed together, each one touching his neighbours on all sides. It appeared as though a foot-ball could not have fallen to the water at any point on the band. They moved as a raft might move with a slow current—no motive power visible, each one maintaining his place without flutter or struggle. Moreover, they were unlike swan or geese in that their heads were not raised, being drawn backward and laid upon the back between the wings, while the long mandibles lay forward, flat upon the neck. Not a sound or a motion from an individual; only the still, solemn glide of the entire raft. And yet there were many thousands of them. Allowing a foot in width and a foot and half in length for each bird, there were twelve thousand pelicans before us. They passed on to the lower end of the lake, then almost immediately rose and flew back over us, and settled again in the shoal water. While flying the pelican shows the broad black band on the extreme feathers of the wings, and would be a very beautiful bird, but it does not stretch forth the neck, even while flying, thus appearing like a swan with his head cut off—*very awkward*. During the day this vast multitude was joined by scores of large flocks, which came down from the sky, circling down in narrow spirals from a height from which they could not be seen. Whence did they come? What was the object of the meeting? Who was the messenger that had called this horde together? They must

* From an article by Ermine Case, Jun., in the Kansas City Review.

have gathered from all the corners of the earth; for by extensive inquiry made since that day among those most likely to know, no witness has been found who ever saw more than fifty together, in one place in this region. All day long they moved slowly about the upper end of the lake, and only occasionally did small groups fly about or splash in the water. A great and solemn convention was evidently being held. Toward evening, however, we saw more commotion among them, and, desiring to witness more closely this wonderful gathering, we went up the lake toward them. After quietly approaching within a half mile of the nearest, we saw that a general motion was being made in our direction, whereupon we concealed ourselves close to the edge of the water, and from thence during the next hour witnessed the most amazing pageant of our lives.

The number had clearly doubled since morning, and the entire troop were arranged in line and engaged in a movement the object of which we could not at first make out. The line was some 300 yards in extent, and at least thirty birds in depth—all "dressed" to the left (continuing the military figure), and at that wing resting upon the bank from which the line slightly diverged, at the extreme right being some 30 yards out; the general movement being toward the shore. As the birds on the left reached the land, they immediately rose in the air and passed back of the line, and formed again on the extreme right. In this way probably five hundred birds were flying at a time, and by this method of deploying from the left to the right wing the grand army was being rapidly moved toward us. When quite near and before us we easily saw that the object of this advance in order of battle was not mere dress parade—each bird was rapidly dashing his head under water, bringing up fish. This long line, then, was a seine fastened to the bank at the left, and being gradually drawn toward the shore, driving the finny prey into the shallow water, where it was incontinently swallowed by the living meshes of this enormous net. Nothing could be of more thrilling interest than to witness the intelligence with which this enormous flock carried out every detail to success. Several times we noticed a detachment, as if detailed for special duty, swing around over some deeper pool, flying so as to drag their feet and flap the extremities of their wings upon the water, closing in finally at the open end of the net on the right. Had a skiff load of boys been sent out with poles to splash the water and drive in the fish, the manœuvre could not have been plainer in its object than was the one before us.

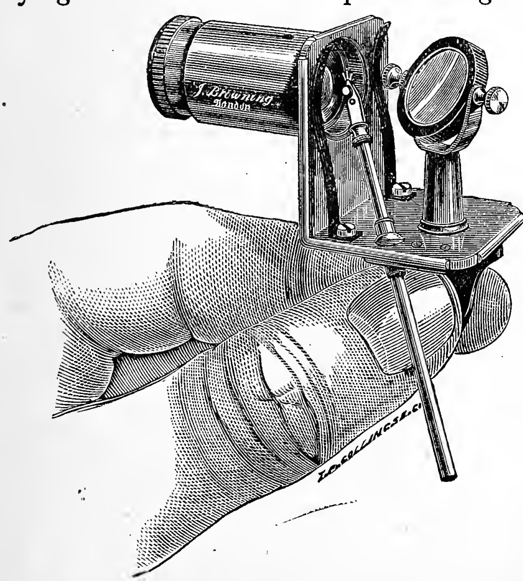
Why should one flying group "drive" in this way, while other scores passed regularly on to their places? Where was the commander-in-chief? There was no struggle for precedence, no fighting, no pressing out of line; here was a voiceless, disciplined multitude, intelligently carrying out an elaborate plan.

When the line had advanced, and lay within 30 feet of us, we rose quickly and fired our guns over them; and when this myriad rose, utterly filling the air, the broad wings crashed against one another with the roar of a hundred trains. Thus closed this most wonderful spectacle.

A specimen we obtained measured $9\frac{1}{2}$ feet from tip to tip of the wings. The mandibles were 15 inches long, and the pouch held 2 quarts of wheat.

VII. COMPOUND ACHROMATIC MICROSCOPE.

MR. JOHN BROWNING has constructed a compound achromatic microscope of extremely small dimensions, the actual size being represented in the accompanying woodcut. The two powers magnify respect-



ively about 15 and 35 diameters: the highest power is provided with a lieberkuhn for the illumination of opaque

objects, which may be held in the forceps, or, if suitably mounted with black spot, examined on the ordinary 3 × 1 slide, for holding which a spring clip is provided. A little practice with the mirror will enable great variety in illumination to be obtained. The definition is remarkably good, showing the markings of some of the coarser diatoms, such as Arachnoidiscus and Isthmia, and will prove useful as a demonstration microscope, as it can easily be passed from hand to hand with little chance of the adjustments becoming deranged. It will be also very useful to those who, from unsteadiness of hand, are unable to use a lens of tolerably high power, as no amount of tremor can interfere with its definition, and when packed in its case it can easily be carried in the waistcoat-pocket.

Mr. Zentmayer has contrived a stage for his new microscope which has virtually *no thickness whatever.* By placing the slide below the stage instead of above, as usual, every impediment to employment of illuminating pencils of extreme obliquity is done away with. The object is secured beneath the stage by a pair of spring clips. The whole of this very simple contrivance can be removed at pleasure, and the usual stage substituted when required.

VIII. SAFE ANÆSTHESIA.

THE dangers attending the administration of chloroform as an anæsthetic agent have always been a source of great anxiety to surgical operators; but while many other so-called safe anæsthetic agents have from time to time been proposed, it has hitherto been impossible to omit chloroform from its position as the only practical anæsthetic in surgical operations of a protracted nature. It will, however, be a source of great gratification to the whole of the members of the medical profession to know that the subject is under investigation, and that a competent committee are endeavouring not only to discover wherein the special dangers of chloroform consist, but also to discover if some anæsthetic agent can be found which will avoid these dangers. The Committee of the British Medical Association, consisting of Dr. Coats, Pathologist to the Western Infirmary, Glasgow; Dr. Ramsay, Assistant to the Professor of Chemistry; and

Dr. McKendrick, Professor of Physiology in the University of Glasgow, have already issued two reports, abstracts of which we propose to lay before our readers:—

In studying the question of the effects of chloroform on the respiration and the heart, it soon became apparent to the committee that chloroform administered to dogs and rabbits has a disastrous effect on the respiratory centres; it is easy to kill one of these animals by pushing the chloroform till respiration is paralysed. In observing the state of the heart during these experiments, it could often be determined by auscultation that its contractions were maintained after respiration had ceased. It was apparent, however, that, even when failure of respiration was more directly the cause of death, the heart was to some extent simultaneously affected; and there were even cases in which the heart appeared to fail at least as soon as, if not before, the breathing. Considering these facts, and bearing in mind that failure of the heart is often asserted in the reports of death from chloroform, a method of experimentation was devised by which respiration would be eliminated, and the effects of chloroform on the heart observed apart from that complication.

In the frog the movements of respiration are not necessary to life, so far as the heart is concerned, as that organ continues beating long after these movements have ceased. After exposing this animal to the vapour of chloroform under an inverted jar until it was anæsthetised, it was found that the heart became rapidly weaker, till it ceased beating, but with ether the heart continued vigorously beating as long as the experiment was continued.

A method was now devised for warm-blooded animals, such as rabbits and dogs. It was found that chloroform immediately produces a serious effect on the heart; the right ventricle almost immediately begins to distend, and the heart presently stops with the right ventricle engorged with blood. In the case of rabbits the heart often virtually came to a standstill within a minute of the introduction of chloroform. Ether may, however, be given under the same conditions for an indefinite period without interfering with the heart. Artificial respiration with ether was maintained in the circuit for an hour, and at the end of that time the heart was beating as vigorously as at first.

In this respect, therefore, ether possesses an enormous advantage over chloroform, but ether has the great disadvantage of tardiness of action. In comparative experiments with rabbits, in which the anæsthetics were given on a towel, it appeared that with chloroform complete anæsthesia was

produced in about three minutes, while in the case of ether it took fifteen to twenty minutes, although the cloth was kept saturated. The committee therefore endeavoured to find an agent which should be as potent as an anæsthetic as chloroform, yet affect the heart and respiration as little as ether.

Whatever substitute for chloroform is found it must be one which can be administered in large doses, and in trying the following agents the committee administered full doses.

Benzine (C_6H_6) was used with the frog. Its effects were nearly as slow as those of ether, and it produced struggling; weakening of the heart was apparent, but not so great as with chloroform.

Acetone (C_3H_6O) produced only slight anæsthesia in the frog, even after prolonged administration.

Pyrrol (C_4H_5N) produced anæsthesia in the frog with considerably less rapidity than chloroform, but great excitement and muscular spasms took place before complete anæsthesia. Administered to three young rabbits subcutaneously, it produced convulsive movements, chiefly of the jaws and fore paws. Anæsthesia in these rabbits was doubtful.

So-called bichloride of methylene, the reputed formula of which is CH_2Cl_2, on being administered to frogs, it was found that the heart became quickly affected, and soon stopped. With rabbits, respiration rapidly deteriorated and stopped while the heart was still beating. In an experiment with artificial respiration the heart was weakened and soon stopped, but not as rapidly as with chloroform. As in the case of chloroform, the right ventricle became enormously distended—the first sign of paralysis being the commencement of this distension.

Amylene (C_5H_{10}) was administered to rabbits both by cloth and subcutaneously. No anæsthetic effect was produced.

Butyl chloride (C_4H_9Cl) administered to rabbits affected respiration, but not very rapidly. In experiments with exposure of the heart, the cardiac pulsations became weaker, and ceased altogether after some time. In one experiment, it was noted that, almost immediately after complete anæsthesia, the respiration became shallow and soon stopped.

Ethene dichloride, formerly named ethylene dichloride, or Dutch liquid ($C_2H_4Cl_2$) produced convulsive movements of both extremities, continuing up to death. There was no anæsthesia up to the commencement of the convulsions.

Methyl chloride (CH_3Cl), which boils at the ordinary temperature, was obtained in alcoholic solution in a sealed tube, and allowed to boil off into a funnel, into which the muzzle of a rabbit was inserted. After somewhat prolonged use, there was not any abolition of reflex action, and the animal almost immediately recovered. The only effect was slight drowsiness.

Ethyl chloride (C_2H_5Cl) boiling at 12 deg. C. = 53·6 deg. Fahr., administered to rabbits in the same way as the above, produced rapid anæsthesia; but in one case the respirations soon stopped, and in another, when air was admitted more freely, general convulsions occurred.

Nitrous ethyl ether ($C_2H_5NO_2$) produced great excitement and convulsions, almost immediately followed by cessation of respiration.

It is obvious that none of these substances would take the place of chloroform, but the committee experimented with isobutyl chloride and ethidene dichloride, with which they obtained comparatively successful results. They thus describe their experiments with these agents:—

Isobutyl chloride (C_5H_9Cl).—1. Experiments on the frog. When it was administered under a glass jar, complete anæsthesia occurred in about five minutes. The heart was observed for thirty-five minutes, during which period its contractions were perfectly vigorous.

2. Experiments on rabbits. When it was administered with a cloth, anæsthesia was produced in three to five minutes. It was continued after anæsthesia for nearly half-an-hour without any interference with respiration.—3. Experiments on dogs. It was administered on cloth; anæsthesia was produced in four minutes. It was continued for half-an-hour, and respiration was unaffected, except slight occasional stertor.

Ethidene dichloride ($C_2H_4Cl_2$, an isomeride of ethene-dichloride, produced from aldehyde). The committee have administered dichloride of ethidene to six patients in the Western Infirmary of Glasgow, with most satisfactory results. The first case was that of a young man aged eighteen, who had an abscess opened near the hip-joint. The anæsthetic was given at 9.19 a.m.; at 9.27, there was complete anæsthesia; the operation began at 9.29, and was completed at 9.32, the patient waking up at 9.35. The recovery was rapid, and there was no vomiting, sickness, or headache. About half an ounce of the substance was used.

In the second case a girl aged thirteen was operated upon, who had abscesses in the thigh, from spinal disease. One

abscess pointed towards the outer aspect of the thigh, while another was more in front. The opening of the first was the special operation at this time. The notes record that, before administration of the anæsthetic, the pulse was 134. At 9.47 the anæsthetic was given, and at 9.50 the pulse had fallen to 120; at 9.54 it was 100, and at 9.55 anæsthesia and muscular relaxation were complete. At 9.56½, there was vomiting, which was undoubtedly caused by the patient having had food in the morning, contrary to orders. The operation was over at 10.1, and at 10.3 the patient was recovering. At 10.5½, she was able to answer intelligently. The amount used was three drachms and a half.

Case III. was a girl aged eight. The operation was removal of the forefinger for strumous disease of bone. At the beginning the pulse was 120; in one minute and a half, it had fallen to 110; in two and a half, to 105; in five, to 102; in six and a half, to 100; and in seven minutes anæsthesia was complete. At eight minutes and a half from the time the anæsthetic was given, the breathing became slightly stertorous. At the end of fourteen minutes, the operation was over, and the pulse was 105. The patient recovered from the anæsthetic, so as to answer questions, in about two minutes, and there was no headache, sickness, or vomiting afterwards.

Case IV. was the same patient as in No. 2. The abscess in front of the thigh previously alluded to was opened. At 9.9 a.m., before the anæsthetic was given, the pulse was 140; at 9.10½, it had fallen to 110; at 9.12, it was at 104; at 9.13, there was slight flushing of the face; at 9.14, the pulse was 100, and there was slight stertor; and at 9.15 there was complete relaxation and absence of reflex movements. The operation was quickly over; at 9.22, the pulse was 110; at 9.23, consciousness had returned; and at 9.24, the pulse had risen to 130. The amount used was four drachms.

Case V. was that of a very powerful young man, aged twenty-one, who was operated upon for the radical cure of inguinal hernia. Immediately before giving the anæsthetic, the pulse was very rapid from excitement—about 140. At 9.17, the anæsthetic was given; at 9.18½, the pulse had fallen to 110; at 9.22, pulse 110 and slight struggling; at 9.23, it was observed that the face was slightly flushed; at 9.24 and 9.26, considerable struggling; at 9.28 the pulse had fallen to 74, and the operation was commenced; at 9.35, pulse 70, and respirations deep and regular; at 9.40, the operation was over, and at 9.42 consciousness returned. About 9.41, there was deep anæsthesia and complete

muscular relaxation; a condition specially necessary for such an operation. The amount used was an ounce and a quarter.

In ase VI. a little girl aged eight had several pieces of diseased bone removed from the ankle. The anæsthetic was given at 10.3½; at 10.5½, the patient was "over," and the operation was begun. After 10.10, no anæsthetic was given. She quickly recovered consciousness, when there was some retching. It appears that this girl had been ailing with sickness and occasional retching for several days previously. The amount used was about two drachms.

Such is a short record of the six cases observed by the committee. The features of special interest in these cases are the facts that there was no injurious effect observable on the respiratory mechanism, although in all cases the anæsthetic was given in such doses as to produce complete anæsthesia and muscular relaxation, and in one the patient was deeply under its influence for twenty-five minutes. 2. The pulse diminished in frequency and increased in volume, and in the deepest anæsthesia it was steady, regular, full, and compressible. There was no indication of failure of cardiac action in any case: a result anticipated from what had been previously observed in animals. 3. There was never any pallor of the countenance or blueness of the lips, but, on the contrary, and even during the deepest anæsthesia, there was a healthy flush on the face and the lips were rosy-red. Taking into account the change in the character of the pulse and in the colour of the face, it would appear that, in anæsthesia from dichloride of ethidene, the blood still remains in a normal amount in the arterial and capillary systems, and does not tend to engorge the venous system and right side of the heart, as is apparently the physiological action of chloroform.

The experiments on dogs show that a dog will live for a lengthened period in a state of complete anæsthesia under the influence of ethidene dichloride, whilst it will die in a short time when chloroform is used.

Attention is drawn to the fact, which is certainly remarkabe, that butyl chloride and isobutyl chloride, having the same chemical formula, exhibit such different actions. Ethene dichloride and ethidene dichloride are also isomeric, but the first of these produced severe convulsions, while the second promises to be an excellent anæsthetic without any convulsive effects.

Several curious effects, interesting especially from a psychological point of view, have been elicited with regard

to the effects of small doses of chloroform and ether on the rapidity of nervous and mental processes. By a refined method of experimenting with Regnault's chronograph, it was ascertained that a few respirations of air containing chloroform or ether produced remarkable retardation in the time of signalling back that a visual impression had been perceived, although the person operated on was quite unconscious of any such delay.

The committee are continuing their researches, and we shall look forward to the issue of their third report. There would seem to be little doubt of their finding an agent or agents which will partially, if not entirely, supersede chloroform as an anæsthetic.

Writing on this subject in the *British Medical Journal*, Mr. J. T. Clover, F.R.C.S., says that a few years ago he received a supply of the ethidene from Messrs. Schoelensack; and after using about half he put the remainder on the shelf, not finding it sufficiently better than chloroform to counterbalance the objections to introduce a new anæsthetic. He found that it produced sickness; and he was not so much impressed by its not seriously depressing the action of the heart, because he has found that chloroform in diluted doses rarely does so.

After reading the report, he found half a bottle (about eight ozs.) of his original stock left. The cork was as sound as if it had only just been put into the bottle, and the liquid was unaltered, thus affording proof of the stability of the substance. He has tried it again, and although probably it excites as much vomiting as chloroform, he is disposed to give it a further trial.

With regard to the remark of the committee that practically a dog will live for a lengthened period in a state of complete anæsthesia under the influence of ethidene dichloride, whilst it will die in a short time when chloroform is used, Mr. Clover suggests that the strength of the vapour of chloroform was greater than that of the ethidene, which would be greatly reduced in consequence of the coldness of the liquid produced by evaporation. He thinks a like failure of the heart might have happened if a fresh bottle of ethidene had been used instead of chloroform.

IX. ON THE TRANSMISSION OF POWER BY MEANS OF ELECTRICITY.

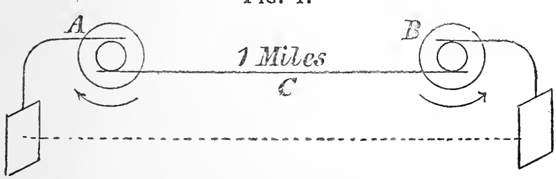
By Profs. Elihu Thomson and Edwin J. Houston.

THE statements recently made as to the size and cost of the cable that would be needed to convey the power of Niagara Falls to a distance of several hundred miles by electricity have induced the authors to write the present paper, in the hope that it may throw light upon this interesting subject.

As an example of some of the statements alluded to we may cite the following, viz., that made by a certain electrician, who asserts that the thickness of the cable required to convey the current that could be produced by the power of Niagara would require more copper than exists in the enormous deposits in the region of Lake Superior. Another statement estimates the cost of the cable at about 60 dollars per lineal foot.

As a matter of fact, however, the thickness of the cable required to convey such power is of no particular moment.

Fig. 1.



Indeed it is possible, should it be deemed desirable, to convey the total power of Niagara, *a distance of* 500 *miles or more, by a copper cable not exceeding one-half of an inch in thickness.* This, however, is an extreme case, and the exigencies of practical working would not require such restrictions as to size.

The following considerations will elucidate this matter:—Suppose two machines connected by a cable, of say 1 mile in length. One of these machines, as, for example, A, Fig. 1, is producing current by the expenditure of power; the other machine, B, used as an electrical motor, is producing power, by the current transmitted to it from A, by

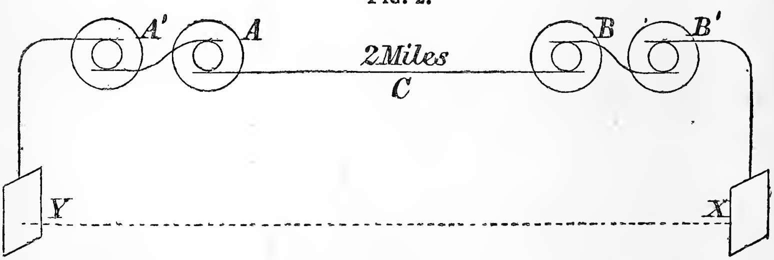
the cable C. The other terminals, x and y, are either put to earth or connected by a separate conductor.

Let us suppose that the electromotive force of the current which flows is unity. Since by the revolution of B, a counter-electromotive force is produced to that of A, the electromotive force of the current that flows is manifestly the difference of the two. Let the resistance of A and B together be equal to unity, and that of the mile of cable and connections between them the 0·01 of this unit. Then the current which flows will be—

$$C = \frac{E}{R} = \frac{1}{1 \cdot 01}.$$

If now an additional machine, A′, Fig. 2, an additional motor, B′, and an additional mile of cable, be introduced into the above circuit, the electromotive force will be

FIG. 2.



doubled, and the resistances will be doubled, the current strength remaining the same as—

$$C = \frac{E}{R} = \frac{1+1}{1 \cdot 01 + 1 \cdot 01} = \frac{2}{2 \cdot 02}.$$

Here it will be seen that the introduction of the two additional machines, A′, B′, has permitted the length of the cable, *c*, to be doubled, without increasing the strength of the current which flows, and yet allowing the expenditure of double the power at A A′ and a double recovery at B B′ of power, *or, in other words, a double transmission of power without increase of current.* Increase, now, the number of machines at A to say one thousand, and of those at B in like proportion, and the distance between them, or the length of the cable, one thousand, or in the case we have supposed, make it 1000 miles, its diameter remaining the same. Then although the same current will flow, yet *we have a thousand times the expenditure of power at one end of the cable, and a*

thousandfold recovery at the other end, without increase of current. And the same would be true for any other proportion.

Since the electromotive force is increased in proportion to the increase of power transmission, the insulation of the cable and machines would require to be proportionally increased.

As an example, it may be mentioned that a dynamo-electric machine used for the purpose of A in the figure, may have a resistance of say 40 ohms, and produce an electromotive force of say 40 volts. Such a machine might require from three to five horse-power when used in connection with a suitable motor, B, for recovery of the power transmitted.

If the resistance of the motor B be say 60 ohms, and the cable transmitting the currents a distance of 1 mile be 1 ohm, then the current—

$$C = \frac{400}{60+40+1} = \frac{400}{101}.$$

If, now, one thousand machines and one thousand motors, and a thousand miles of cable, each of the same relative resistances, be used, the current—

$$C = \frac{1000 \times 400}{1000 \times 101},$$

which has manifestly the same value as before. If our supposition of the power used to drive one machine be correct, then from 3000 to 5000 horse-power would be expended in driving the machines, and possibly about 50 per cent of this amount recovered. Then we have from 1500 to 2000 horse-power conveyed a distance of 1000 miles. What diameter of copper cable will be required for such transmission? Since this cable is supposed to have the resistance of 1 ohm to the mile, calculation would place the requisite thickness at about $\frac{1}{4}$ inch. If, however, the distance be only 500 miles, then the resistance per mile may be doubled, or the section of the cable be decreased one-half, or its diameter will be less than one-fifth of an inch.

For the consumption of 1,000,000 horse-power a cable of about 3 inches in diameter would suffice under the same conditions. However, by producing a much higher electromotive force, the section of the cable could be proportionally reduced, until the theoretical estimates which we have given in the first part of this paper might be fulfilled. The enormous electromotive force required in the above calculation would, however, necessitate such perfect insulation of the

cable that the practical limits might soon be reached. The amount of power required to be conveyed in any one direction would of course be dependent upon the uses that could be found for it, and it is hardly conceivable that any one locality could advantageously use the enormous supposed power we have referred to.

Stripped of its theoretical considerations, the important fact still remains that with a cable of very limited size an enormous quantity of power may be transferred to considerable distances. The burning of coal in the mines, and the conveyance of the power generated by the flow of rivers, may therefore be regarded as practicable, always, however, remembering that a loss of about 50 per cent will be almost unavoidable.

It may be mentioned that Dr. C. W. Siemens and Sir William Thomson have recently made statements that are in general accordance with the views here expressed.

NOTICES OF BOOKS.

Journal of a Tour in Morocco and the Great Atlas. By JOSEPH DALTON HOOKER, Pres. R.S., and J. BALL, F.R.S. With an Appendix including a Sketch of the Geology of Morocco, by G. MAW, F.L.S., F.G.S. London: Macmillan and Co.

THERE is perhaps no country in existence so near to the centres of European civilisation, and yet so little known, as the empire of Morocco. Its geology, its flora and fauna, and even the main features of its physical geography, are to a very great degree uncertain. The higher interest, therefore, attaches to the explorations undertaken by the authors. The main object of the journey was of course botanical. It had long been the wish of Sir J. D. Hooker to explore the range of the Great Atlas, "up to that time little better known to geographers than it was in the time of Strabo and Pliny, and especially to find whether its vegetation formed a connecting-link between that of the Mediterranean region and the flora of the Canaries and Madeiras." For this task the authors were specially qualified by an acquaintance with the vegetation of Spain, Italy, Asia Minor, Syria, and Algeria. As an indispensable preliminary to travelling in such a region they had been furnished from the Foreign Office with a special recommendation to the Emperor, and they received every possible assistance from Sir J. Drummond Hay, at that time British Ambassador to the Moorish Court. Still their task was by no means easy. The provincial authorities received them, indeed, in deference to the Imperial mandate, with much courtesy, but at the same time showed no little suspicion, and were strongly desirous to prevent them from penetrating to the points they most desired to reach. The Moors, like many persons in our own country, cannot understand a man pursuing knowledge for its own sake. Hence the authors were obliged to represent themselves as searching for plants of medicinal value. Geological and mineralogical research could only be indulged in by stealth, as any one found breaking off fragments of stone is suspected of being a treasure-seeker. How a collector of animals would have to justify his actions to native curiosity is hard to imagine. In this department, however, the travellers do not appear to have taken any special interest. Indeed the fauna of North-western Africa is far from rich. Insect-life was found remarkably scarce, only one species of butterfly (*Papilio podalirius*) being mentioned. Locusts, however, were plentiful and destructive, though their ravages were by no means so indiscri-

minating as is often represented, certain plants escaping untouched. They are most greedy whilst still young, becoming sluggish when mature. If a swarm invades the fields before the middle of April, when the rainy season is not over, the plants that have been devoured almost down to the root revive with wonderful rapidity. If the swarms appear late, and attack the wheat or maize after it has flowered, the consequences are very serious. In combatting this evil the Moors would be greatly benefitted by the ingenious appliances lately devised for this purpose in the United States.

Certain of the serpents are said to be venomous, the bite of one species—not named—being reputed fatal in two minutes; but no case came under the observation of the travellers. In the Sous Valley, according to tradition, pythons of from 20 to 30 feet in length still lurk—descended doubtless from that monster which encountered the army of Regulus. Birds are not very numerous, but their fearlessness in presence of man is mentioned as an interesting fact.

The flora of Morocco, like its fauna, is far from showing that admixture of semi-tropical and tropical forms which might have been expected. The view of Mr. Wallace—that the Great Desert forms the northern boundary of the Ethiopian region, whilst the Mediterranean coast organically speaking pertains to Europe—appears to hold good in a botanical as well as in a zoological point of view. The Straits of Gibraltar must, however, have divided the two continents for a very considerable time, since, in spite of a general resemblance between the floras of Spain and of Morocco, each has a great number of distinct species. The connecting-links between the vegetation of the latter country and that of the Canaries are unimportant. Still these islands contain some peculiar forms more closely allied to endemic Moroccan species than to those of any other country. They contain also a large class of plants not hitherto found in Morocco, but more closely allied to Mediterranean forms than to any others. Hence Sir J. D. Hooker is disposed to regard these Atlantic islands "as a very distinct sub-region of the Mediterranean province.

The flora of Morocco, and secondarily its fauna, like those of many other countries, has suffered from fire-raising. The herdsmen, to obtain a fresh growth of grass for their cattle, set fire to the thickets, and in this manner the ancient forests of Mount Atlas have been gradually destroyed. A still worse enemy are the goats. Our authors most justly say—"The young plants while yet seedlings are cut off by the teeth of the goat, the great enemy of tree vegetation—an animal whose disastrous influence acting indirectly on the climate of wide regions entitles it to rank as one of the worst enemies of the human race."

The fertility of Morocco, were the land intelligently cultivated, would be prodigious. Even now the Aguidal garden at the city

of Morocco, a plot of about 40 acres, yields an average yearly return of £20,000.

The climate is remarkably genial and pleasant. The temperature fluctuates less than that of Madeira, the difference between the average temperature of the hottest and the coldest months not exceeding 10° F., whilst the number of rainy days at Mogador is only forty-five. Pulmonary affections are here all but unknown, and the region wants merely a settled Government to become an admirable sanitarium.

We regret that we must here cut short our notice of a work which literally teems with interesting facts. We can confidently recommend it to all who love accounts of travel, even if they are utterly unacquainted with botany.

The volume is tastefully got up, and is enriched with characteristic views of scenery, and with a map of Southern Morocco, corrected according to the observations of the travellers, and showing their route.

An Illustrated Dictionary of Scientific Terms. By WILLIAM ROSSITER. London and Glasgow: W. Collins, Sons, and Co. 1879.

MR. ROSSITER has tried to compress too large an amount of information into too small a compass, and in numerous instances he has sought for his definitions in untrustworthy directions, the consequence being that a number of words that are found every day in books and newspapers are of necessity omitted, while some of the explanations are calculated to mislead the student. Columbium is not a rare *mineral* found in columbite; coal brasses are not spathic ore; caviare is not a sauce; the papyrus is not a *tree* found on the banks of rivers; and soot is only in a very restricted sense "the unburnt remains of fuel." Had the generally useless cuts been left out Mr. Rossiter might have found room for such words as eosin, quantivalence, phonograph, microphone, &c. A large number of the words, too, are quite out of place in a dictionary of this kind. Surely it is not the office of a scientific dictionary to tell people the meaning of such words as bayonet, beet-root sugar, explosion, gable, language, and so on, all of which they can find in any sixpenny Johnson. This book is not at all worthy of a place by the side of many excellent scientific text-books issued by the same firm.

Coal; its History and Uses. By Professors Green, Miall, Thorpe, Rucker, and Marshall, of the Yorkshire College. London: Macmillan and Co. 1878.

A series of Ten Lectures delivered by the Professors of the Yorkshire College at Leeds and Keighley, in accordance with a suggestion made by Dr. Carpenter, the Secretary of the Gilchrist Educational Trust. These Lectures were afterwards revised, put together in a consecutive form, and illustrated with nearly sixty woodcuts. The subjects treated of are—the Geology of Coal, by Prof. Green; Coal Plants and Animals, by Prof. Miall; the Chemistry of Coal, by Prof. Thorpe; on Coal as a Source of Warmth and Power, by Prof. Rücker; and, lastly, on the Coal Question, by Prof. Marshall.

Science made Easy. A Series of Ten Familiar Lectures on the Elements of Scientific Knowledge most required in Daily Life, &c. Parts I. to VI. By Thomas Twining, Author of "Technical Training." London: Hardwicke and Bogue.

The first four parts of this work, which originally appeared in 1871, having speedily run out of print, Mr. Twining has taken the opportunity afforded him by the publication of Parts V. and VI. to thoroughly revise them, and bring down the knowledge contained in them to the present year by adding a postscript to the reprint of each of the four parts. The discoveries of MM. Pictet and Cailletet in relation to the liquefaction of oxygen, hydrogen, and nitrogen, De la Bastie's method of making toughened glass, and several others are noticed in their proper places. In other respects but little change has been made in the work. The last two parts contain four Lectures, entitled—A Glance at the Mineral Kingdom, with Notions concerning the Vegetable Kingdom; Outlines of the Animal Kingdom; and, lastly, a double Lecture on the Outlines of Human Physiology. The diagrams to this part of the work are drawn upon a black ground, which gives them an appearance of relief that is a very great help to the understanding. This is especially apparent in the diagrams relating to cell structure. The experiment was made by Mr. Twining in instituting Working Class Examinations in different parts of the country; and a programme is now issued yearly from the Economic Museum, Twickenham, giving full particulars of the regulations, certificates, and prizes, as well as the facilities offered to Institutions in the metropolis for the delivery of the Course. We are glad to see that Mr. Twining's praiseworthy and intelligent exertions in the cause of the spread

of scientific truth amongst the working classes are meeting with such success.

Bulletin of the United States Geological and Geographical Survey of the Territories. Vol. iv., No. 4. Washington: Government Printing-Office. 1878.

This issue contains Prof. S. H. Scudder's account of the fossil insects of the Green River shales, Wyoming. The bulk of the specimens found are damaged beyond recognition. The eighty species identified belong to six different orders of true insects, and have a decidedly tropical character.

Dr. S. Jordan reports on the collection of fishes made in Dakota and Montana by Dr. Elliott Coues, and Prof. Chickering gives a catalogue of 692 species of phanerogams and vascular cryptogams collected in the same territories. This document is of great value, from the light it throws upon the range of many plants.

Dr. Endlich's memoir, "On some Striking Products of Erosion in Colorado," is exceedingly instructive, and should be read by those—for such persons still exist—who fancy that the beds of rivers have been made not by them, but for them.

Mr. J. A. Allen contributes a paper on the *Sciuri*, or tree-squirrels, of America.

Records of the Geological Survey of India. Vol. xi., Part 3. 1878.

This issue contains an account of the progress of the Gold Industry in Wynad, in the Nilgiri district. Three companies have been at work for some time, not in the most scientific manner, but until recently the returns have not been very favourable. It is only within the last few months that some stone from "Wright's level," at the Alpha Works, has yielded from 11 to 17 dwts. per ton. From the latest reports of the working of the Prince of Wales' Company a rich vein has at length been struck.

Mr. F. R. Mallet describes a curious antimony ore from Sarawak, consisting partly of senarmontite.

Botany; Classification of Plants. By W. Ramsay McNab, F.L.S. "The London Science Class-Books," edited by G. Carey Foster, F.R.S., and P. Magnus, B.Sc. London: Longmans and Co.

In this little manual compression seems to have reached, if not exceeded, its fair limit. Reversing the plan followed by Prof. McAlister in the companion volumes on zoology, the author devotes the larger portion of the space at his disposal to the lower forms of plant-life. Among the Thallophytes the families are characterised, whilst among phanerogamous plants they are simply enumerated without any reference to their distinctive features. In consequence the two portions of the book are of unequal value. There is a table of contents, but no index—an unfortunate deficiency, though inevitable from want of space.

Annual Report of the Board of Regents of the Smithsonian Institution, showing the Operations, Expenditure, and Condition of the Institution for the Year 1877. Washington: Government Printing-Office.

This annual volume contains, in the first place, an account of the work done by the Institution and of its present position. The secretary, Mr. Jos. Henry, continues to urge, on what appear to us very valid grounds, the entire separation of the Institution from the National Museum of the United States. This latter establishment was formerly under the charge of the Patent Department, but was transferred to the Smithsonian Institution in 1858, whilst its importance fully warrants a distinct establishment. The Proceedings of the Smithsonian Institution and of the Museum are of course blended in the volume before us.

Amongst the most important work of the year we must mention Dr. Habel's "Archæological and Ethnological Researches in Central and South America." This gentleman, having taken Guatemala as a centre, devoted seven years to an exploration of the region, and afterwards visited Colombia, Ecuador, and Peru.

Nos. 7, 8, and 9 of the Bulletins of the Museum have also been published. The first of these is a valuable contribution to the natural history of the Hawaian and Fanning Islands, and of Lower California. No. 8 is an index to the hitherto very complex synonymy of the Brachiopoda, which rank among the most characteristic forms by which strata are co-ordinated.

Perhaps the most valuable part of the "Report" is the Appendix, which contains a profusion of valuable matter not

commonly to be met with. We find, in the first place, a full translation of Prof. Holmgren's memoir on colour-blindness—a subject which is now attracting general attention, from its bearing upon the correct interpretation of signals by railway officials and sailors. For the detection of this defect he gives a decided preference to Seebeck's method, an assortment of Berlin wools serving as test-objects. There is another interesting essay on the same subject, by Mr. Jos. Henry.

Next follow translations from the "Transactions of the Geneva Society of Physics and Natural History." There are, further, very valuable papers on the antiquities and ethnology of various parts of the Western Hemisphere; notes on the "History and Climate of New Mexico," in which that region, on the faith of a considerable body of statistical evidence, is recommended as a sanitarium for the consumptive. Dr. R. H. Coolidge is quoted as saying that "the worst possible climate for a consumptive is one with a long-continued high temperature and a high dew-point." Yet we have heard of not a few cases of incipient consumption where great benefit has been experienced from a residence in Sierra Leone.

Dr. Weismann's paper on the change of the Axoloth to an Amblystoma is given in full, together with an account of the experiments performed by Marie v. Chauvin.

The volume is completed by a number of short meteorological memoirs.

We notice in this "Report," as in former issues, a peculiarity which greatly detracts from its utility. The index gives merely the headings of the memoirs or articles, omitting all details,—a serious defect in a work containing a mass of information at once so important and so varied. We should beg to suggest the publication—say once in ten years—of a general index of subjects.

A Treatise on Chemistry. By H. E. Roscoe, F.R.S., and C. Schorlemmer, F.R.S., Professors of Chemistry in Owens College, Manchester. Vol. ii., Metals (Part 1). London: Macmillan and Co.

This volume opens with a consideration of the generic properties of the metals. The authors, we find, take a well-founded objection to the term "metalloid," still applied by some writers to non-metallic bodies, and insist on the absence of any distinct line of demarcation between the two great classes of elementary substances, not a few of which are alternately pronounced either metallic or non-metallic, according to the point of view whence they are considered. The remarks on alloys, though necessarily

brief, bring into prominence several facts often overlooked. After discussing the specific gravity of metals, their melting-points, atomic heats, and quantivalence, the authors pass to a consideration of the general properties of acids, the constitution of salts, &c. The metals and their compounds are then described in succession, according to the classification given on p. 22.

The present work is one of the very few chemical treatises in which the peculiar and unpleasant odour of potash-lye, so well known to practical men, is mentioned, though no light is thrown on its origin. In treating of the commercial sources of potash-salts, the manufacture of beet-root and cane-sugar is mentioned as a fortunate discovery. We cannot help regarding it as a serious mistake, which, if persevered in, must lead to the exhaustion of the soil.

Sodium and its compounds are described in considerable detail. The great English beds of rock-salt at Northwich, generally said to occur in the new red sandstone, are here described as found in the mountain limestone. It is noted that the process of evaporating brine, as followed in Cheshire, has not been improved since the times of the Romans. The alkali manufacture (Leblanc's process), as now carried on, is very fully described, and admirably illustrated with drawings done to scale. The recent improvements, proposed or actually carried out, are not omitted. Thus we have notices of Deacon's closed roasters for preventing the escape of hydrochloric fumes, of the mechanical salt-cake process of Messrs. Jones and Walsh, of Middlesborough, intended to obviate all hand-labour, and of the salt-cake furnace of Messrs. Cammack and Walker, which is declared to be based upon a more scientific view of the decomposition than any former plans, though at the same time no opinion is expressed as to its practical feasibility. The Hargreaves process for decomposing salt directly with sulphurous acid, in presence of air and steam, is also described. Mr. Mond's process for the recovery of sulphur from the vat-waste is noticed, as also the ammonia-soda process of Dyer and Hemming, which, as latterly improved by Solvay, is now worked on the large scale.

Under Calcium we find a description of the manufacture of bleaching-lime, with an account of Weldon's process, and of the improved chamber devised by Mr. Deacon. We do not see any mention of the revolving process recently invented, and said to give remarkably uniform results.

The authors enter at some length into the metallurgy of zinc, lead, copper, silver, and mercury, giving carefully executed drawings of the requisite furnaces. The electrotyping process is also described, without any reference to the share of the late Mr. Smee in the early development of the art. Electroplating and photography are noticed, though necessarily in brief compass.

Aluminium in the metallic state is a curious instance of a discovery from which great hopes were entertained proving subsequently a practical failure. Two interesting facts not generally known are mentioned in this chapter—the resuscitation of the ancient and renowned alum-works of La Tolfa by a French company, and the cessation of the manufacture of ammonia-alum. Some ten or twelve years ago it was often difficult to meet with a sample of potash-alum.

The volume before us merits and will receive the same favourable reception which was awarded to its predecessor. To the chemical student it offers the great advantage of bridging over the gulf between theory and practice better than probably any other text-book in the language. To manufacturers, foremen, &c., it would be of equal service for the same purpose were it not for the exclusive use made of the so-called "new" nomenclature. Had it been desired to keep practical men in the dark concerning chemical principles, and to produce confusion by the multiplication of synonyms, we doubt if a more successful method could have been adopted than this change of terminology.

CORRESPONDENCE.

THE BAND-PATTERN IN ANIMALS.

To the Editor of the Monthly Journal of Science.

Sir,—May I ask if any readers of the "Journal of Science" have made observations bearing on the curious fact that in a multitude of species belonging to different parts of the animal kingdom the colours are arranged, in bands or stripes, approximately at right-angles to a line drawn from the head to the tail? Further, in a vast majority of cases, these bands are alternately yellow and black, or at least a very deep green, deep purple, &c. This pattern prevails, among mammals, in the tiger and the zebra; among Hymenopterous insects, in a number of bees and wasps. But among the Coleoptera it recurs in a multitude of groups differing widely in their habits, localities, and affinities. Thus among the so-called "long-horns" we find it in the genera *Clytus*, *Rhagium*, *Stenura*, and *Diastocerus*; among Buprestids it occurs in *Cyria*, *Diadoxus*, *Nascio*, *Conognatha*, *Stigmodera*, and *Buprestis*. Among the Cetoniadæ it is met with in *Plæsiorhina*, *Anisorrhina*, *Allorrhina*, *Clinteria*, *Gametis*, *Pachnoda*, and *Euphoria*. It may finally be traced among the highly carnivorous tiger-beetles (*Cicindela*), and in the carrion-feeding sexton-beetles (*Necrophorus*). In all these cases, save the moths, the bands consist alternately of yellow, orange, or buff, and of black or some very dark shade. A few of these instances, such as those of the tiger and zebra, may subserve the purpose of concealment; and others, *e.g.*, *Clytus arietis*, may be mimetic, causing the insect to be mistaken for some more formidable species. But in the majority it seems as if we encounter here the markings of some more deeply-hidden law.—I am, &c.,

J. W. S.

THE SEA-SERPENT.

To the Editor of the Monthly Journal of Science.

Sir,—I have read with much interest your notice of Dr. Wilson's memoir on sea-serpents, and quite agree with you and with him that the question of their existence should be fairly and dis-

passionately discussed by biologists. With your permission, however, I will make a few remarks on the case of the *Pauline*. If the narrative deposed to at Liverpool is literally true, all doubt is at an end. No giant seal, basking-shark, piece of floating wreck, sea-weed, or flight of birds could act as the unknown monster is said to have done. The testimony—that of a number of disinterested observers who watched the struggle for a quarter of an hour—is such as would be accepted in any court of justice in the most serious case. Still there are certain points on which further light would be very welcome. When a boa or python attacks a deer its dry scaly surface is pressed against the rough hairy coat of the quadruped, whose limbs, further, prevent the deadly coils from slipping from their place. The sea-serpent, on the contrary, has to apply its skin, probably slimy, to the exceedingly slippery surface of the whale, where there is no neck, waist, or projecting limb to aid in retaining the folds. Would not the difficulty of killing by constriction be greatly enhanced under such circumstances? Again, the serpent whilst grappling with the whale is described as having its own head and tail disengaged, to the extent, as I understand, of 30 feet at each end. Now non-poisonous serpents, in attacking, invariably either keep hold of some fixed object with their tail, or lay hold of the intended victim with their jaws. As no fulcrum for the tail was possible in this case, I should think that a preliminary seizure with the mouth would be all the more essential. That the jaws must be capable of opening wide enough for this purpose is evident if the prey was afterwards to be swallowed. A "large" sperm whale measures in girth about 17 metres. It is much to be regretted that no naturalist appears to have had a conversation with the crew of the *Pauline*.—I am, &c.,

SERPENT-HUNTER.

PROCEEDINGS OF SOCIETIES.

Royal Society, *January* 9.—The first paper was a "Note on the Inequalities of the Diurnal Range of the Declination Magnet as recorded at the Kew Observatory," by Balfour Stewart, F.R.S., and William Dodgson. The authors are at present engaged in searching for the natural inequalities of the above range, more especially for any of which the period is between 24 and 25 days. They have found strong evidence of an inequality of considerable magnitude of which the period is 24·00 days, very nearly. They have also found preliminary evidence of the existence of two considerable inequalities having periods not very far from 24·65 and 24·80 days. These two appear to come together in about eleven years, but the exact time of this cannot be given. No trace of any inequality with a period of 24·25 days has been fouud. We shall give a report of a more recent paper on this subject in our March number.

"Some Experiments on Metallic Reflexion," by Sir John Conroy, Bart., M.A. The author finds that when light is reflected from a polished surface of gold or copper in contact with various media, the angle of principal incidence diminishes, and the principal azimuth increases with the increase of the refractive index of the medium in contact with the metallic surface; and further, the diminution in the value of the principal incidence appears to be nearly in proportion to the increase of the refractive index of the surrounding medium. The values of these angles for gold with red light are—

	Principal Incidence.	Principal Azimuth.
In air	76° 0′	35° 27′
In water	72 46	36 23
In carbon bisulphide ...	70 03	36 48

Assuming that the angle of principal incidence for a metal is the same as the angle of polarisation of a transparent substance, —that is, the angle whose tangent is equal to the refractive index,—the value of that angle in air, as deduced from the measurements made in water and carbon bisulphide by multiplying the tangent of the principal incidence in those media by their refractive indices, is 76·53 and 77·22 instead of 76.

"Researches on the Absorption of the Ultra-Violet Rays of the Spectrum by Organic Substances," by W. N. Hartley, F.Inst. Chem., F.R.S.E., F.C.S., and A. K. Huntingdon, F.Inst. Chem., A.R.Sc. Mines, F.C.S. The authors find that—

1. The normal alcohols of the series $C_nH_{2n+1}OH$ are remarkable for transparency to the ultra-violet rays of the spectrum, pure methylic alcohol being as nearly so as water.
2. The normal fatty acids exhibit a greater absorption of the more refrangible rays of the ultra-violet spectrum than the normal alcohols containing the same number of carbon atoms.
3. There is an increased absorption of the more refrangible rays corresponding to each increment of CH_2 in the molecule of the alcohols and acids.
4. Like the alcohols and acids, the ethereal salts derived from them are highly transparent to the ultra-violet rays, and do not exhibit absorption-bands.

In order to ascertain whether isomeric bodies exhibited similar or identical absorption-spectra, a series of benzene derivatives was examined. The results may be summarised as follows:—

1. Benzene, and the hydrocarbons, the phenols, acids, and amines derived therefrom, are remarkable—first, for their powerful absorption of the ultra-violet rays; secondly, for the absorption-bands made visible by dissolving them in water or alcohol, and diluting; and thirdly, for the extraordinary intensity of these absorption-bands,—that is to say, their power of resisting dilution.
2. Isomeric bodies, containing the benzene nucleus, exhibit widely different spectra, inasmuch as their absorption-bands vary in position and in intensity.
3. The photographic absorption-spectra can be employed as a means of identifying organic substances, and as a most delicate test of their purity. The curves obtained by co-ordinating the extent of dilution with the position of the rays of the spectrum absorbed by the solution form a strongly-marked and often a highly characteristic feature of many organic compounds.

There is a curious feature in connection with the position of the absorption-bands; at the less refrangible end they either begin at line 12 Cd or line 17 Cd, and those which begin at 12 end a little beyond 17.

"On the Electromagnetic Theory of the Reflection and Refraction of Light," by George Francis Fitzgerald, M.A., Fellow of Trinity College, Dublin. The author shows that the method adopted in his former paper on Magnetic Reflection, in the "Proceedings of the Royal Society" for 1876 (No. 176), is justified, and that it is legitimate to consider an incident plane polarised ray as composed of two oppositely circularly polarised rays, each of which is reflected according to its own laws. He considers next the cases of the magnetisation being all normal to the surface, and all in the surface and the plane of incidence, and

obtains the following result:—When the incident ray is plane polarised, and the plane of polarisation is either in or perpendicular to the plane of incidence, the effect of magnetisation is to introduce a component into the reflected ray perpendicular to the original plane of polarisation, which vanishes at the grazing and normal incidences, and, in the case of iron, attains a maximum at about the angle of incidence $i=63°20'$. He does not obtain any change of phase by reflection in any case. Apart from this question of change of phase, his results conform completely to Mr. Kerr's experiments on the reflection of light from the pole of a magnet.

Chemical Society, *January* 16.—Dr. J. H. Gladstone, F.R.S., President, in the chair.

The first communication was made by W. H. Perkin, F.R.S., "On the Action of Isobutyric Anhydride on the Aromatic Aldehyds." The author has studied the reaction which takes place when isobutyric is substituted for butyric anhydride.

The next paper was by Dr. Dupré and H. Wilson Hake, "On Two New Methods for the Estimation of Minute Quantities of Carbon: (1) Gravimetric; (2) Chromometric; and their Application to Water Analysis." (1) The Gravimetric Method:—This method consists essentially in burning the small quantity of carbon in a stream of oxygen in an ordinary combustion-tube containing some granulated cupric oxide. The method is found to be quite accurate enough for determining the carbon obtained from a litre of a first-class potable water. The chromometric method, or, as the authors name it, the nephelometric method, consists essentially in burning the carbon, but the carbonic acid is conducted into a standard solution of basic acetate of lead, and estimating the turbidity produced, as compared with that produced by the carbonic acid evolved under similar circumstances by a known and nearly equal quantity of carbon, the difference between the two being estimated by a Mills's colorimeter. This method is extremely delicate, and a difference produced by 3-100ths of a milligrm. can be clearly estimated. The authors suggest their use for estimating the carbon in iron and the carbonic acid in air.

Dr. Frankland read a communication "On Stannic Ethide," by E. Frankland and A. Lawrance. In endeavouring to prepare stannous ethide by the action of zinc ethyl upon dry stannous chloride the authors discovered that the reaction was—

$$2SnCl_2+4ZnEt_2=Sn+SnEt_4+4ZnEtCl,$$

and that by its means stannic ethide could be prepared more conveniently and in larger quantities than has hitherto been possible.

The next paper was by R. S. Dale and C. Schorlemmer, "On Aurin," Part II. In previous researches the authors stated that by the action of alcoholic ammonia on aurin, rosanilin was

obtained, and a dilemma arose, that if aurin had the formula $C_{20}H_{14}O_3$, rosanilin could not have the formula which Hofmann had proved it to have, $C_{20}H_{19}N_3$. The authors therefore again prepared pure aurin and confirmed their previous analysis. Zulkowsky states that the method employed by the authors yields wretched results, and cannot be used on the manufacturing scale. The authors, however, affirm in the present paper that under certain conditions, which they are not at liberty to divulge, a fairly good yield can be obtained. They have used two methods to purify aurin; one by conversion into ammonia aurin and the decomposition of the latter by hydrochloric acid, and a second by taking advantage of the fact that the solubility of aurin in alcohol decreases with the removal of the by-products. The analyses of pure aurin thus obtained completely confirmed the formula $C_{20}H_{14}O_3$. Some beautiful specimens were exhibited in connection with this paper.

A paper was read by W. Carleton Williams "On the Derivatives of Di-isobutyl."

The last paper was "On the Action of Chlorine upon Iodine," by J. B. Hannay. The author has re-examined the question as to the existence of the compound ICl_4, and has come to the conclusion that such a body has no existence, for two reasons. First, that the reaction for its formation is impossible, as no high chloride of iodine can exist in the presence of free iodine; and, secondly, that careful experiments by which chlorine is added to iodine in the most advantageous manner for the formation of a high chloride fail to indicate such a body,

Meteorological Society, *January* 15.—The annual meeting of this Society was held at the Institution of Civil Engineers—Mr. C. Greaves, President, in the chair.

The report of the Council showed that the chief features of the proceedings during the year 1878 had been the final completion, on a comprehensive and well-organised basis, of the arrangements for systematic inspection of the Society's stations—an object which has engaged the sedulous attention of successive Councils for the last four years—and the delivery of a series of lectures on meteorology by certain members of the Council. The total number of Fellows now amounts to 425, 41 having been elected during the year.

The President having delivered his address "On Dryness versus Humidity," the Officers and Council for the ensuing year were elected.

January 18.—The usual monthly meeting of this Society was held at the Institution of Civil Engineers The following papers were read:—Abstract of "The Meteorology of Bombay Presidency," by C. Chambers, F.R.S., communicated by Sir G. B. Airy, K.C.B., F.R.S., Astronomer Royal. "Experiments with Lowne's

Anemometer," by Captain William Watson, F.U.S.; "Meteorology of Bangkok, Siam," by J. Campbell, Staff-Surgeon, R.N.; "Results of Meteorological Observations taken at Calvenia, South Africa," by Kaufmann J. Marks, F.U.S.

PHYSICAL SOCIETY, *January* 15.—Prof. G. C. Foster, Vice-President, in the chair.

Dr. Erck exhibited a constant bichromate of potash battery. It consists of a narrow lead trough, 12 ins. long by 3 ins. wide and 1 in. deep, lined along both sides with the carbon plates. The zinc plate, 10 ins. long, is immersed in the solution to the depth of an inch midway between the two carbons. A continual circulation of the bichromate solution is kept up by allowing fresh solution to drop into the cell at one end, and the exhausted solution to drop away by a tap at the other end. As the space between the two carbons is only about half an inch wide, there is merely a thin layer of solution between the positive and negative poles. The internal resistance of the cell is therefore very low when short circuited, only about $\frac{1}{4}$ ohm. To obtain the maximum current, about 8 ozs. of solution per hour should be supplied.

Dr. Guthrie, F.R.S., described some of the results he had obtained from experiments on the vibration of metal rods or lathes fixed in a vice at one end and free to vibrate at the other. The experiments were carried on by dusting sand on the rod and observing the nodal lines formed by it when the rod was vibrated so as to give out notes determined by a monochord. Dr. Guthrie's results showed that the two final segments at the free end are equal in length to the inner segment at the fixed end. It appears from these experiments that if a free lathe, vibrating with a node in the middle but having an even number of segments, be clamped at where there is a node we alter its conditions of vibration. When the lathe is half free the end segment breaks up into two parts together equal to the segment at the fixed end. In the case of torsional vibration of the lathe the position of the longitudinal nodal lines depended to some extent on the clamping of the lathe in the vice.

Prof. Foster pointed out that in a natural node the direction of the tangent is varying, whereas in an artificial node it is always horizontal.

Prof. Unwin explained that the sand accumulated at nodes because the particles when thrown off the lathe make certain horizontal excursions, which tend to move them nearer the points of repose of the lathe.

NOTES.

Agriculture.

The *Phylloxera* Commission of the French Academy have resumed their labours after having suspended them for two years. They have been roused into action by the increased ravages committed on French vines during the last summer and autumn. The Minister of Agriculture has allowed the Commission a certain sum for expenses. Let us hope that their present labours will be more fruitful than their former ones.

Biology.

The blood of the Octopus contains—instead of hæmoglobin—hæmocyanin, an organic compound containing copper, which here assumes the function fulfilled by iron in the circulation of the Vertebrates.

The chestnut trees of the Cevennes are perishing in consequence of their roots being attacked by a fungus. The tannic acid present in the juices of the trees exudes, and forms black stains on the trunks and on the surface of the soil, whence this new epidemic is locally known as *Maladie de l'encre*.

The celebrated botanist F. Cohn, in speaking of *Anastatica Jerochontica*, said, "The rose of Jericho,"—which, in the first place, is not a rose, and which, secondly, is not found near Jericho.

The well-known butterfly *Colias Edusa* (clouded yellow), which was in 1877 more plentiful in England than ever before, has been last year as remarkably and exceptionally scarce.

M. V. D. Costes, in the "Correspondance Scientifique," suggests that ants have already passed through their stage of social progress, and, having reached the highest perfection consistent with their faculties, have now become stationary, and incapable of further advance.

According to Mr. F. P. Pascoe (Entom. Mo. Mag.) a new species of *Siderodactylus* is ravaging vines in the Island of Ascension. It seems to have been imported from South Africa.

M. L. Fredericq, in a paper communicated to the Academy of Sciences, maintains that the changes of colouration in the skin of the Octopus have no correspondence with the facts of mimetism, and resemble rather the changes produced in the human face by the emotions of fear or anger.

M. Dareste has experimented on the suspension of life in the embryo chicken. He finds that in eggs taken from under a hen

after three days' incubation, and exposed to the temperature of the atmosphere, which was then 20° C., the movement of the heart was not completely arrested until after the lapse of seven days.

Haltica nemorum, a tiny yellow-striped beetle, proves to be an enemy to barley.

Prof. Turner, of Edinburgh, in his Memoirs on the "Comparative Anatomy of the Placenta," points out essential differences between the structure of this organ in the sloth and in the lemurs, especially in the *Protopithecus* of Madagascar.

M. Bordier considers atmospheric pressure as one of the principal agents in the transformation of species.

According to M. P. Bert the periodic movements of plants and their heliotropism are due to variations in the quantity of glucose contained in the moving part, in consequence of its state of hydration and its consequent degree of tension.

Prof. Haberlandt considers that the chlorophyll in the cotyledons of *Phasiolus vulgaris* is formed from starch. The starch-granules are gradually surrounded with a layer of protoplasma, which is at first colourless, but gradually turns green, while the starch grains disappear.

M. Yung has communicated to the Academy of Sciences the results of his experiments on the action of different coloured light upon the development of animal ova and larvæ. Their action, beginning with the most favourable, may be arranged thus:—Violet, blue, red, yellow, white, green. The red and green light appeared positively hurtful. Between yellow and white light there was little difference. Tadpoles kept without food died most quickly in the violet and blue rays. The incidental mortality appeared greater in the coloured rays than in white light.

Chemistry and Technology.

Wines on analysis often display properties not suspected from their taste. Sorts rich in alcohol are often pronounced light, and others strongly acid seem mild, &c. These seeming contradictions are, according to Drs. Mach and Patele, due to the fact that wine is a most complicated liquid, whose components are by no means all known. Even among the recognised and more prominent ingredients, it is not so much the quantity of one or the other as their relative proportion which determines the flavour.

Some new phenomena observed in "plastering" wines and musts are recorded by Prof. E. Pollacci. He finds sulphate of lime reacts solely upon the cream of tartar, producing acid sulphate of potassa, which remains in solution, while tartrate of lime is formed and is chiefly deposited. The reaction between

the two salts is not, however, complete, since there may be found in the liquid both sulphate of lime and cream of tartar. If the gypsum is in large excess a part of it is deposited among the tartrate of lime.

Dr. Theodor Schuchardt, of Goerlitz, has introduced a new body, which he calls silicium strontium. It is formed from the preparation of metallic strontium by electrolysis. The compound is a grey powder with a slight odour resembling phosphuretted hydrogen. When mixed with dilute hydrochloric acid a rapid evolution of the spontaneously inflammable siliciuretted hydrogen takes place. If obtainable in quantity this compound will probably be the readiest source of siliciuretted hydrogen.

A ready means of detecting the adulteration of saffron is suggested by Messrs. Domeier and Co. To detect adulteration with *Calendula* flowers (Feminelle) it is merely required to moisten a few flowers, and to rub them singly with the finger on white paper. The genuine flowers will give a fine rich yellow colour, whilst the Feminelle will only yield a violet reddish hue. It can also be easily detected by soaking the suspicious flowers in pure —or, better still, distilled—water. The real saffron will retain its fine red colour after hours, whilst the Feminelle will lose its artificial tint within a short time. To detect an admixture of honey and barytes it is merely required to put a pinch of saffron in a tumbler with pure clear water, agitating it for a few minutes. Adulterated saffron will at once turn the water cloudy, and even small particles of dust may be seen falling to the bottom, which, on pouring the water carefully out, will be found to be a slimy, sand-like mass. With pure saffron the water will remain clear, showing a fine pure yellow colour, which, according to the quality of the flowers, will be more or less intense. Five or ten minutes suffice for these experiments.

Metallurgy.

An admirable sketch of the present position of the French Iron-trade, drawn up by M. S. Jordan, Professor of Metallurgy at the Ecole Centrale, for the use of his foreign colleagues at the Paris meeting of the Iron and Steel Institute, is given in the October number of the "Revue Universelle des Mines." It begins by describing the different French coal-basins. The iron-ores native to France are then described, and the analyses of most of them are given. We are next presented with a series of tables showing the consumption of French ores both native and foreign. The smelting processes also receive attention, and are described in eleven geographical groups.

Physics.

Mr. Louis Schwendler has reported to the India Office on the suitability of the electric light for India. We shall refer at length to this report in our next number; meanwhile we may

mention that Mr. Schwendler advocates the employment of the electric light at Indian railway-stations.

The German Government lost no time in the practical employment of the telephone. There are now 272 telephonic circuits, and communication is satisfactorily carried on over a distance of from 30 to 40 miles.

Mr. Stroh, the well-known mechanician, has observed that if a telephone, with the circuit of its coil left open, be held to the ear, and a powerful magnet be moved gently up and down along the length of the magnet, and at a distance of an inch or two from it, a faint breathing sound will be heard; the recurring pulses of sound keeping time with the up and down motion of the magnet. The sound may, according to the "Telegraphic Journal," be aptly compared to the steady breathing of a child, and there is a striking resemblance between it and the microphonic sounds of gases diffusing through a porous septum, as heard by Mr. Chandler Roberts, F.R.S.

Writing on the subject of Gas *versus* Electricity, to "Nature," Mr. W. H. Preece observes that we know no more of the electric light now than we did in 1862, when as great a display was made in our Exhibition of that year as was made in the French Exhibition of last year. The recent experiments have, he says, shown both the strength and weakness of the position of the gas companies. Their strength consists in their being in possession of the ground; their weakness consists in their producing only a poor light—and a very poor light—when compared with electricity. But it would almost seem, from the experiments that have been made, that the quantity of light to be produced by gas is only a question of the quantity of gas consumed in a given space. There are now burning in the Waterloo Road two brilliant gas-lamps, giving a light of 500 candles; and this is greater, in point of fact, than the intensity of the light developed by any one of the electric lights that are now on trial in the thoroughfares of London. There is, however, a defect in gas light which remains to be eradicated, and that is the colour of the light. The one great advantage which the electric light has over gas is that the electric light, owing to its very high temperature, produces rays of every degree of refrangibility, and therefore as an illuminating power it is equal to that of the sun. A very marked advance towards perfection in this direction in gas-lighting has been made in the albo-carbon process, by which the gas burnt is enriched with the vapour of naphthalin—a refuse of gas manufacture. By this process the intensity of the light of a gas-burner is improved at least five times, and in some experiments witnessed by Mr. Preece the improvement was as much as twenty times. There are three points which all electric lights for general purposes should be required to attain: the first is a brilliancy far exceeding that of any known lamp; the second is

a durability greater than that which would be required for night operations in England; and the third is absolute steadiness, to enable work to be conducted without affecting the eyes. There is no electric light that has yet been introduced which supplies us with these desiderata.

The first of the Friday evening discourses, at the Royal Institution of Great Britain, was given on the 17th ult., by Professor Tyndall, his subject being "The Electric Light." The electric light had, he said, been known for seventy years, as in 1808, and again in an improved form in 1810, it was shown to audiences at the Royal Institution. Prof. Tyndall quoted Faraday's saying, that he would rather occupy himself with finding fresh effects than spend his time in exalting those effects. But it was the exaltation of these effects which he first studied in a simple way which has led to the present possibilities of our electric lighting. Prof. Tyndall gave a historical sketch of the various arrangements, beginning with that of Mr. Holmes in 1862. The different "candles" in use were also described. Prof. Tyndall said he did not believe any fresh scientific discovery was needed to make the electric light of general application to large places. The scientific man knew what different natures of machines were required to do the different kinds of work to be done. It remained now for mechanical skill to carry out the work. In conclusion he pointed out the mistake of those who, like Cuvier, spoke with contempt of those whose practical skill carried to utility the experiments of the philosopher.

The Philosophical Society of Glasgow have instituted a triennial lecture in memory of the late Prof. Graham, Master of the Mint. The Council have also caused a portrait medal of the value of £10 to be struck, and this they propose to award every third year for the best original investigation, either in chemical physics or in pure or applied chemistry, which may be communicated to the Society or to its chemical section in the interval of the awards. The choice as first Graham Lecturer appropriately fell on Mr. W. Chandler Roberts, F.R.S., who had so long been associated with Graham's work, and who was enabled to employ much of his original apparatus. The lecture was given on the 22nd ult., Mr. James Young, F.R.S., presiding. Mr. Roberts described in detail the experiments that enabled Graham to establish the law of the diffusion of gases, and he illustrated experimentally the passage of gases through porous bodies, such as unglazed earthenware and artificial graphite, as well as through a layer of the hard translucent variety of opal known as hydrophane. Mr. Roberts pointed out that the Graham law of difusion forms the basis of the science of molecular mechanics, and his measurements of the rates of diffusion prove to be the measure of molecular velocities which have been investigated mathematically by Clerk Maxwell, Clausius, and Boltzmann, and experimentally by Losehmidt in developing the dynamical theory of gases.

The liquefaction of gases formed the subject of Graham's earliest paper in 1826, and it occupied his attention at intervals during his life. He held the view that hydrogen, when absorbed by palladium, is reduced to the metallic form—a supposition which has received strong confirmation from the success that has attended M. Raoul-Pictet's efforts to solidify this gas. At the conclusion Mr. Roberts said the Graham lecture had been instituted in honour of the labours of a life the memory of which will be as enduring as its work, and to stimulate others to investigate as patiently and earnestly the varied phenomenon whose basis is molecular mobility.

Mr. F. H. Ward, F.R.M.S., has endeavoured to get rid of the objectionable black lines across the spectrum caused by dirt adhering to the jaws of the slit, by substituting a right-angled prism with the apex of the right angle carefully ground off and polished, forming what he terms a solid slit. With glass prisms, although it answered well for bright line spectra, it was an entire failure so far as continuous spectra were concerned. The spectrum was as full of lines as with a very dirty slit. The cause was evident upon microscopical examination, the edges being splintered. Better results were obtained with quartz prisms, which promise well when more skill is obtained in polishing the very delicate edge.

The value of telescopes for viewing objects at short distances appears to have been overlooked. The writer frequently employs a telescope for examining plants on cliffs, in ponds, and other inaccessible places at distances as small as 15 ft. The effect is almost microscopic, details of flowers and venation of leaves being clearly brought out. The instrument is a small one by Ross, having an object-glass of 1·35 ins. aperture and about 12 ins. focal length, the ample length of slide allows such near objects to be distinctly focussed; while its more legitimate performance is all that can be desired, and its corrections perfect enough to allow many celestial objects to be seen, bearing well a tolerably deep eye-piece.

THE MONTHLY

JOURNAL OF SCIENCE.

MARCH, 1879.

I. THE KEYS OF DEATH.

IS strychnin, or prussic acid, or arsenic a poison? The answer is ready. What is a poison? The oracles are dumb. We have in these days almost ceased playing at the game of definitions. We no longer ask what is gravitation, or motion, or light, feeling that our answers to such questions can only bewilder. Definitions of life are still gravely asked for, and as gravely propounded. But they, too, will soon be given up, and neither Science nor daily life will find cause to regret them. As concerns a satisfactory definition of poison the case is very similar. Two attempts have, however, been lately made to supply this deficiency. Certain physiologists, speaking in the interest of a certain philanthropic movement, contend that the idea of poison is purely qualitative, and that whatever is poisonous in large doses is poisonous also in the very smallest proportions, the only difference being that the injury done may escape observation unless the dose is often repeated. We cannot for a moment accept this view. We maintain that it is perfectly unchemical to ignore the ideas of quantity and concentration. It would be easy to give numerous instances where—if the reagents employed are less in proportion, or weaker than the required standard—we obtain not a smaller quantity of the product sought for, but a substance totally different, or, in certain cases, no action at all. Or if we, coming at once to the mark, fix our attention upon the higher animals, we find that for them all oxygen is the first necessary of life. If deprived of it for but a few minutes they inevitably perish. Yet Davy proved

that animals placed in an atmosphere of pure oxygen are thrown into a morbid state, which, if the experiment is sufficiently prolonged, terminates fatally. Here, therefore, we see that dilution converts oxygen from a poison to a necessary of life. Or take sulphuric acid. If swallowed in a concentrated state it destroys all parts of the system which it touches. Dilute it largely with water, and it becomes a pleasant summer beverage of tonic properties. Open an animal which has swallowed such dilute acid, and examine the œsophagus and stomach with the most powerful microscope, and you will find no trace of those phenomena caused by the undilute acid. The results differ not in extent, not in degree, but in kind. These instances, which might be indefinitely multiplied, prove that the idea of poison is not essentially qualitative.

Let us now examine the second notion which has been brought forward to help us to a precise definition of poisons. It is alleged that whilst articles of food undergo decomposition in the stomach, poisons are not thus decomposed, but are absorbed unchanged, exist in the tissues unchanged, and in cases of recovery are finally expelled still in the same state in which they were first ingested. This explanation sins in the first place by implying that all articles taken into the system must be either food or poison. But, further, will chromic acid undergo no change if taken internally? How will the chlorides of arsenic or of cyanogen behave? There is again a liquid, neither rare nor entirely strange to chemists, which agrees with the features here ascribed to poisons as well as does alcohol. This liquid, if swallowed either in large or small doses, remains undecomposed in the stomach, is taken up by the absorbents, and enters the blood and the tissues, still without having undergone decomposition, and is finally eliminated as such in the urine and the perspiration. If, therefore, the above definition of poison be correct, this liquid is a poison; but if it is found by common experience harmless and necessary, what of the definition? The liquid in question is water, which has never been proved to suffer decomposition in the system, and which therefore *ought* to be a poison!

Leaving these discussions for more profitable considerations, we find included under the general name of "poisons" two classes of bodies having little in common save their injurious effects upon the living organism into which they are introduced. We have, on the one hand, certain ferments—"germs," or by whatever other name they may be

known—which, when absorbed by an animal, bring on such diseases as rabies, small-pox, cholera, dysentery, typhoid, and the whole class of so-called zymotic affections. On the other hand, there are the true poisons, such as arsenic, strychnin, aconitin, hydrocyanic acid, &c. The substances of this latter class are well-defined chemical individuals, and even when, as is the case with all the most deadly, they are of vegetable or animal origin,—an awkward fact for the charlatans who guarantee the "purely vegetable" nature of their life-pills,—their efficacy has no connection with vitality. Poisons of this class when introduced into the system set up morbid action almost immediately, and if the dose be sufficient, and if no remedial measures are adopted, the symptoms go on increasing in violence without intermission until death ensues. A characteristic feature is that their noxious power may be decreased, or even totally extinguished, by dilution. If a pound of arsenic, or even of aconitin, were dissolved in the head-waters of the Thames, not the least danger would result to any of the thousands of people who drink Thames water as supplied to certain districts of London. The ferments, on the contrary, are not definite chemical principles, capable of being isolated, of entering into combination with other bodies, and of being separated again. So far as we know they are living organisms of low type, belonging to that "debatable land" that lies on the margins of the animal and the vegetable kingdoms, and they retain their virtues, or vices, only so long as their vitality endures. If swallowed, or otherwise introduced into the system, no evil effect is perceived for some time. There is a so-called "period of incubation" which in the case of *rabies* may extend over months, or even years, before the symptoms of disease make their appearance. Another characteristic is that the poison is multiplied in acting. If we kill an animal by the administration of a dose of strychnin or of arsenic, we cannot obtain from its body any more poisonous matter than we put in; but if we inoculate an animal with a ferment-poison we find in some cases its blood, in others its secretions and excretions, capable of reproducing the same disease in other animals. The ferment grows and fructifies in its victim, just as does wheat in a field. The ferments, too, lose none of their deadly power by dilution. If, returning to our former example, we placed a quantity of the excretions of a cholera patient or of a victim of typhoid fever in the head-waters of the Thames, there would be great danger to persons sup-

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plied with such water in London. By this dilution-test it has been established that the poison of the cobra—and in all probability of other venomous serpents—is not a ferment, but a true chemical principle. Hence all anti-zymotics—such as benzoic, salicylic, carbolic, and cresylic acids—are necessarily impotent in the treatment of snake-bites.

A distinction was formerly attempted to be drawn between poisons active wben swallowed and others supposed to be potent only when introduced directly into the blood. To this latter class the venomous secretions of snakes were supposed to belong. This is now known to be an error. The poison of the cobra, if in a sufficient dose, is as deadly when swallowed, or applied to a mucous membrane, as if conveyed into the blood. Some of the disease ferments are certainly active when swallowed, and, although there may be doubt concerning the poisons of rabies, of carbuncle, and of yellow fever, discretion is in all such cases the better part of valour.

The common sense of mankind has always deemed poison to be specific, like food, but in an even more decided manner. It has concluded from observation that a substance, harmful or even deadly to certain animal species, may be to others perfectly innocent. Certain modern writers, undertaking somewhat prematurely to explain the respective nature of foods and of poisons, have more or less explicitly denied this view. In the very teeth of experience they have asserted that any given substance is poisonous or otherwise in virtue of its own chemical attributes, no regard being had to the nature of the animal into whose system it is to be introduced. This opinion is no less contrary to general analogy than to direct observation. We bring together certain reagents in a glass vessel, and we obtain a given result; but if we perform the same experiment within the stomach or in the tissues of an animal, the result will in many cases be seriously modified. Nor can we even from an experiment performed upon one species conclude with certainty what will happen in the case of another. Nay, even within the limits of one and the same species we encounter in this respect more or less diversity. This is notably the case with the poisons of unwholesome mushrooms, and of certain species of fish and mollusks. It is no uncommon thing for a number of persons to partake jointly of such esculents, and while some of the party sicken, even fatally, the remainder are not in the slightest degree inconvenienced. Such idiosyncracies cling even to the life-

less remains of animals. If different samples of wool are prepared in the same manner, and dyed together in the same pan, some of them will not only be darker in shade than others, but even a difference in tone will be occasionally detected.

Coming more directly to the point, we find certain animals feeding heartily upon plants which to other species, and especially to man, are decidedly poisonous. Snails and slugs devour the leaves of the foxglove as safely, and relatively in as large quantities, as a cow consumes grass, and they are also fond of the Solanaceæ, including the belladonna. The caterpillars of the following Lepidoptera feed upon poisonous plants:—*Gonopteryx rhamni* upon *Rhamnus catharticus; Thats polyxena* upon species of *Aristolochia; Danais Archippus* and *Chrysippus* upon various Asclepiads; *Deilephila Galii, Nicea,* and *Euphorbiæ* upon species of *Euphorbia; Chœrocampa Nerii* upon the oleander; *Sphinx ligustri* upon privet and laurel; *Noctua baja* upon belladonna; *Polia cappa* upon staves-acre; *Heliothisa armigera* upon tobacco; and *Chrysoptera moneta* upon monkshood. Turning to the Vertebrates we find that domestic fowls eat the foxglove; the hornbill consumes with perfect impunity the deadly fruit of species of *Strychnos;* whilst the hare and the rabbit nibble the belladonna and gnaw the bark of the mezereon.

We are by no means attempting to furnish a catalogue of species which feed upon plants noxious or fatal to man, but merely give a few of the best-established instances in proof that what is poisonous to one species may be to another perfectly harmless. There are doubtless poisons which are fatal to all. No animal save the serpent which secretes the poison has been found able to withstand the venom of the cobra.

In looking over the instances above given, and seeking their explanation, we may suppose that the deleterious principles of the plants mentioned may never be really absorbed. Like gum and cellulose, they may simply pass along the intestinal canal and be rejected with the solid excreta without ever entering the system at all. But this hypothesis, even if demonstrated, would fail to solve the question. We should still have to ask, why are these substances absorbed in one animal and in others harmlessly rejected? Further, in many cases we know that the poison is absorbed. When the common viper bites a hedgehog we know that the poison enters the blood. What renders it inert, as witnessed in

the experiments of Dr. Lenz? Morphia if swallowed by certain apes enters their circulation, and can be readily detected in their urine in large quantities. Why is it in them eliminated without occasioning harm? We note, further, an important distinction: the exemption which any animal enjoys from the effects of some particular poison may be merely relative, as compared with the action of the same drug on man, or it may be absolute. To poison a hedgehog with cantharides would be as hopeful a task as to poison a Neapolitan with macaroni. But when it is loosely said that a horse cannot be poisoned with arsenic, or an ape with morphia, it is meant merely that these animals can endure larger doses than can we without perceptible harm.

There are cases in which copper—a well-known irritant poison—is not merely absorbed, but assimilated, and appears even to be an essential of life. Harless detected copper in the blood and in the liver of certain mollusks, especially cephalopods and Ascidiæ. His experiments show that it "stands in an essential relation to the blood-corpuscules." Indeed in the blood of the cephalopods there has been detected an organic copper compound, known as hæmocyanin, which fulfils the function due in the higher animals to hæmoglobin. Von Bibra detected copper on certain crustaceans, such as *Cancer pagurus*, and found that it occurred in an inverse ratio to the iron which it more or less completely replaced. Dr. Genth quantitatively determined the copper in the blood of the *Limulus*, a crab found on the coasts of North America. The blood of the healthy females, taken just before laying their eggs, was of a deep blue, and when dried the incinerated residue contained 0·295 per cent oxide of copper, and but a mere trace of oxide of iron. How strangely would the appearance of mankind be altered had we, like the *Limulus*, copper instead of iron in our blood! A rich azure would mantle on the cheek of beauty; we should extol in song the charms of sea-green tresses, and liken the lips we love not to coral, but to malachite. Perhaps the dealers in cupriferous pickles and preserved fruits and vegetables are striving to bring about this consummation.

Copper in an organic compound named touracin has been detected by Prof. Church in the feathers of the touraco. That it must consequently be normally present in the blood of this bird follows of necessity.

We may conceive it possible, both as regards disease-ferments and poisons properly so-called, that strains o

beings indifferent to their action may gradually be formed by a process of natural selection. Scarcely any other hypothesis will explain the facts that certain races of men resist disease-poisons to which others succumb, and that an epidemic on its first visit to any country commits such frightful ravages. A striking instance of this was shown by the late spread of the measles in the Fiji Islands. It is exceedingly probable that the resistance of the slug to digitalin and atropin may have been produced in the same manner as the negro's non-susceptibility to yellow fever. Indeed one of the most curious facts connected with poisons is that individual men, and doubtless animals, by beginning with small doses, can gradually habituate themselves even to the most deadly compounds. The legend of Mithridates contains a basis of solid truth. Without depending upon disputed cases, such as that of the arsenic-eaters of Styria, the history of opium-eating gives abundant evidence of this acquired immunity. We at one time knew a gentleman, connected with the Inland Revenue in a town of the West Riding, who was no more affected by a wine-glassful of laudanum than is an ordinary man by the same quantity of sherry. Very similar is the case with tobacco, which, though it produces severe constitutional disturbance in persons unaccustomed to its use, is afterwards borne without the slightest inconvenience. The question may, indeed, be raised whether a similar immunity from snake-poison might not be acquired by habituating the system to doses at first infinitesimal, and gradually becoming larger and larger. Our knowledge concerning the physiological action of such poisons is as yet exceedingly rudimentary, and it is quite possible that there might be some serious objection to such inoculation. Still we should recommend this idea to the experimental notice of the medical profession in countries such as India and Australia, where venomous snakes abound, and where biological research has not yet been fettered at the bidding of a hysterical humanitarianism.

We mentioned above that on the entrance into the system of a true poison morbid phenomena at once make their appearance. To this rule there is an exception, *i.e.*, when the poison is administered in successive doses too small to produce the usual alarming symptoms, but too large or too frequent to allow the system to become habituated to their effects. Certain poisons indeed, such as lead, accumulate in the system, whilst others—colchicin being perhaps the best example—are cumulative in their action. We have

thus a group of facts of which it is hard to speak without denying the facts we have just mentioned on the acquired toleration of poisons. We have, indeed, here to deal with very complicated effects. We know that the drunkard or the opiumist can swallow, without apparent result, doses of alcohol, of methyl- and amyl-compounds, or of laudanum, which would be rapidly fatal to a person "unseasoned." Still we know that this immunity is bought at the expense of his general health.

These considerations lead us to the question of "slow poisoning." Popular tradition and imaginative literature often introduce us to a very interesting villain, who gives his victims a dose calculated with such marvellous precision as—whilst producing no immediate action—to bring on inevitable death after the lapse of a certain number of months. Much of this may be safely pronounced impossible. Nevertheless a few ugly instances have occurred lately which warn us that in this department, as in many others, modern Science has not yet thrown an electric light into all the dark hiding-places of Nature. We know some of the details of the case of a retired Indian officer, who died not long ago at a town in one of the Eastern counties, and who displayed symptoms utterly irreconcilable with any recognised disease, and scarcely intelligible on any other hypothesis than that of an unknown poison administered previous to his return home.

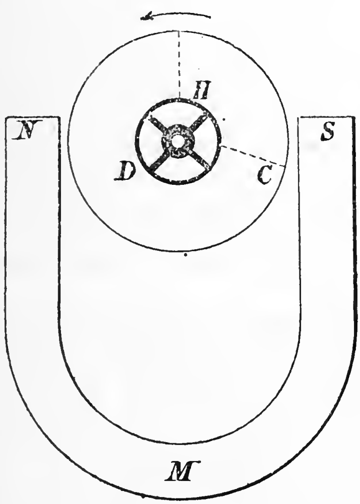
We have thus taken a hasty and necessarily superficia survey of a subject which presents to the chemist and the biologist numbers of unsolved problems—problems not merely of high scientific interest, but of the greatest practical importance. This tempting harvest-field is now, however, closed to Englishmen. Our laws, which with inimitable logic once deemed that "in a liberal construction copper is tin," now see "vivisection" where there is no "section" at all.

II. A CURIOUS THERMO-MAGNETIC MOTOR.

By Profs. EDWIN J. HOUSTON and ELIHU THOMSON.

DURING investigations by the authors, concerning the increase in the coercitive force of steel by changes of temperature, the following curious thermo-magnetic motor was devised. This motor, though devoid of practical value, will, no doubt, be of sufficient scientific interest to warrant a short description :—

In the figure, a disc or ring of thin steel, D, is mounted on an axis, so as to be quite free to move. The edges of the



wheel are placed opposite the poles, H and S, of a magnet. In this position the wheel of course becomes magnetised by induction.

If, now, any section of the wheel, as H, be sufficiently heated, the disk will move in the direction shown by the arrow. The cause of this motion is as follows :—The section H, when heated, has its coercitive force thereby increased, and being less powerfully magnetised by the induction of the pole S than the portion C, immediately adjacent to it, the attraction exerted by the pole S on the latter portion is thereby sufficient to cause a movement of the disk in the direction shown by the arrow. If a constant

source of heat be placed at H, a slow rotation in the direction shown is maintained.

To ensure success the disk must be sufficiently thin as to prevent its acquiring a uniform temperature. If the source of heat be at the same time applied at diametrically opposite portions of the disk, as at H and D, adjacent to the poles, the same effect will be produced. Since the amount of heat expended in producing motion of the disk is so enormous when compared with the force developed, it will be readily understood that this motor is of no value as such, but must be regarded as an interesting example of the interconvertibility of force.—*Journal of the Franklin Institute.*

III. THE HEAT OF THE COMSTOCK MINES.*

By Prof. JOHN A. CHURCH, E.M., Columbus, Ohio.

ONE of the most striking phenomena connected with the mines on the Comstock lode is the extreme heat encountered in the lower levels. This heat proceeds from the rock, which maintains constantly a temperature very much higher than the average of the atmosphere in Nevada.

The heat of these mines is a matter of more than usual interest, for they are the only hot ones now worked in the United States, and both in the present temperature encountered and in the increase which is to be expected as greater depths are reached, they appear to surpass any foreign mines of which we have a record.

The rock in the lower levels of the Comstock mines appears to have a pretty uniform temperature of 130° F.

The low conductivity of minerals to heat forbids the supposition that a rock of 130° F. temperature can lose heat

* Abstract of a Paper read at the Chattanooga Meeting of the American Institute of Mining Engineers, May, 1878, by permission of Lieut. Geo. M. Wheeler, Corps of Engineers, U.S.A., in charge of U.S. Geographical and Geological Surveys, west of the 100th meridian.

sensibly to any depth in the course of twenty-four hours. The shallow holes which were made use of always lay in new ground, and exhibited results which may be accepted with as much confidence as if they were 20 feet or more deep. Very often they were in ground which had been exposed only one or two hours, having been sunk immediately after a blast which threw off 4 or 5 feet of the rock. The surface which was thus thrown down had not been exposed more than twenty-four hours. The high temperature and small flow of air in the heading forbid the supposition that any sensible diminution of heat could have taken place at the bottom of the drill-hole under such circumstances. The surface of the rock exposed to the air of the drift was found to be about 123° F., the experiment being made near the "header" or end of the drift. The air itself was found to show considerable uniformity when its temperature was taken under circumstances that were at all similar. In freshly opened ground it varied from 108° to 116° F., and higher temperatures are reported at various points, reaching in fact as high as 123° F. in the 1900 level of the Gould and Curry.

The temperature of the air is subject to more fluctuations than that of the rock, for the simple reason that it is artificially supplied to the mine, and varies according to the distance to which it is carried, the quantity, velocity in the pipe, and its initial temperature. All of these elements of the problem vary within wide limits. The initial temperature of the air which supplies a particular drift will, for instance, depend upon whether it is drawn from the surface, the bottom of a shaft, where it is often cooler than above ground, or from some old air-way, where it has had time and opportunity to take up heat.

In the Comstock mines it is the custom, without exception, to blow the air through galvanised iron pipes, the diameter of which is usually from 8 to 20 inches. The size most used is 11 inches in diameter, and the usual amount of air blown is about 700 cubic feet per minute, this being the supply for two to six or more men, working in one or two "headers."

In most cases the air is not sent down from the surface, but taken from some point in the incline or at the bottom of the shaft. Its temperature may be assumed at about 80° or 90° F. in summer, though it is sometimes higher than this. Its velocity in the air-pipe is not very far from 1000 feet per minute. From these data it will be seen that about 15 or 20 degrees of heat are added to the air, in a period of time

varying from half a minute to two minutes. The iron of the pipe is so thin and its conductivity so great that there is practically a slender current of air moving through a body of hotter air.

The iron receives heat both by immersion in the hot air and by direct radiation from the still hotter walls. The currents confined in it must be thrown against its sides by eddies, and the air is thus made to absorb heat by contact as well as by the transmission of heat-rays through it.

Drifts that do not exceed 200 or 300 feet in length are *usually* not above 110° or 112° F. in temperature, and more often they are below this. But when the length increases to 1200 and 1500 feet the temperature may rise to 116° F. without any other change in the circumstances.

So far as the author's personal experience goes, the latter temperature has not been exceeded in any drift into which a good current of air is blown. By a "good current" he means one of not less than 700 or 1000 cubic feet a minute. Still he has no hesitation in asserting his confidence in the higher temperatures which others have sometimes obtained. The view which he takes of the phenomenon and its cause admits of such exceptional heat at particular points as a rational consequence of the forces at work. But he regards them as exceptional, and believes the average temperature of those drifts which are considered to be distinctively "hot" is usually not above 108° to 112° F., though rising to 116° F., when they are very long.

These limits are, however, not in the least degree true of the water which enters the drifts from the country rock, and also from the lode rocks. That approaches more nearly 150° F. The large body of water which has filled the Savage and Hale and Norcross mines for more than a year, and from which it is safe to say a million tons of water have been pumped within twelve months, gave a temperature of 154° F. Even after being pumped to the surface through an iron pipe exposed, in the shaft of the Hale and Norcross, to a descending current of fresh air for more than 1000 feet, and then flowing for 100 or 200 feet through an open sluice in a drain-tunnel which discharges into a measuring-box, the water in this box was found to have a temperature of no less than 145° F.

But the water varies in temperature in different parts of the lode, like the rock and the air. In the East cross-cut 2000 feet level, of the Crown Point Mine, which is noted for its extreme heat, the water, after flowing for nearly 150 feet over the bottom of the drift, was found to have a tempera-

ture of 157° F. On the contrary, the water in other places is much less hot, but it is as a rule always hotter than the air, and in many cases it appears to be hotter than the rock is found to be, except in especially hot spots.

The East cross-cut of the Crown Point 2000 feet level, which was temporarily abandoned and boarded up on account of the heat, gave an *air* temperature of 150° F., the thermometer being thrust through a crack in the boarding. At the head of this cross-cut the heat was proved to be higher than this.

Another hot spot is in the Imperial Consolidated Mine. In this mine the Black Dyke splits, sending a shoot off to the north-east, and a drift has been run on the 2000 feet level, along the eastern side of this branch dyke.

This proved to be a very hot spot indeed. Rock, air, and water were all so much above the usual limits of temperature, even in these hot mines, that the work of cutting the drift must have been extremely severe.

The Belcher south incline has a hot belt of rock, quite narrow, a short distance above the nineteen hundred station, and similar hot places are found in most of the mines.

The author inclines to the opinion that, as a general rule, these hot areas lie in belts, and are not irregular or promiscuously placed in the mass of East country rock. Where this seems to be disproved by the distance run in the superheated rock, it will, he thinks, probably be found that the drift, or incline, and the hot belt have the same direction.

Hot belts are also found at the contact of the diorite and propylite in the Virginia mines. The diorite is itself in active decomposition, and mines which have carried drifts in or near it are very hot. The Julia has explored a quartz seam, which appears to lie entirely in the diorite, and this has proved to be one of the hot belts.

This apparent concentration of the heat in the line of contact of two rocks is not supposed to be due to any thermal or electro-thermal action, but to depend merely upon the fact that in this neighbourhood the ground is more broken and the surfaces of the rock increased. These conditions are obviously favourable to the action of atmospheric waters.

Belts of excessively hot ground are not the only noticeable phenomena in these mines. More remarkable still are the belts of unusually *cold* rock: these are fewer in number than the hot belts, but they are strongly marked; they are always wet, and the water that drips through the crevices

of the shattered rock that composes them is noticeably cold to the touch, and cools down the air of the drift. Such a wet, cold belt of rock exists on the 800 feet level of the Justice Mine, and there is a very decided change of temperature in passing from one to the other side of it. Water drips from the rock in numerous places in these as in most mines, and that usually it is hot, or at least warm.

Other cold belts are found in the mines which are not so cool as that in the Justice, but are perceptibly cooler than the rock at a short distance from them. They complete a well-linked chain of heat phenomena, extending from rocks that are sensibly cold to the touch, and may not have a temperature above 50° or 60° F., through rocks that have the average atmospheric temperature, and those which are as hot as surface rocks ever become in Nevada, to those which have a temperature of 157° F.

Finally, in the chain of testimony relating to this phenomenon is to be noted the condition of the rock. Wet places have been spoken of, but the rock cannot be considered as generally wet. There are water-ways, and many of them appear to reach the surface, but they are of limited breadth, like the belts of hot rock. This water is usually hot, but sometimes cool or tepid.

Very often, usually in fact, the rock is perfectly dry, though very hot: that is the case in all the mines. Wet rock is the exception, and dry rock the rule, through the whole lode. In the drifts cut through this hot, dry rock, the walls of the freshly exposed surfaces are painful to the hand, and the air is often filled with dust. The rock is both hard and tough, but, in spite of its strength, it gives an impression of fine porosity to the touch, due probably to its trachytic character. It often has the odour of clay, but not always. It may be slightly adherent, or the impression of dryness upon the tongue may be due to its heat, or to the fine dust which covers every fragment.

The heat in the Comstock and other mines similarly situated is quite generally spoken of as the feeble remnant of a temperature that once reached the point of rock fusion, but the facts observed have led the author to refer the high temperatures encountered in the mines not to the internal heat of the earth, nor to the residual heat of the rocks, which were once melted, but to chemical action now maintained in the erupted rocks.

This action is not a combustion, for the oxidisable minerals in the lode and its accompanying rocks, the metallic sulphides, are little altered. In fact, the total quantity of

pyrite and other sulphides is not large for the neighbourhood of a mineral lode, but on the contrary, strikingly small, and not sufficient to maintain the heat of the rocks and water, except under circumstances of unusually rapid oxidation. That no metallic oxidation of any moment goes on in these rocks is susceptible of proof. The metallic sulphurets in the rock show little sign of decomposition; and this is true even in layers of the propylite, that are fissured and seamy and drenched with water, whether hot or cold. In fact, the preservation of the sulphur compounds, in presence of so much heat and moisture, is a noticeable fact, which the author has frequently remarked in all the mines. The analyses of such of the mine waters as he has been able to find confirm this statement.

The author says the quantity of water pumped from the mines the past year must have been as much as 350,000 or 400,000 tons a month. If its temperature is assumed to be only 135° F., and the average temperature of the air for the year 50° F., we have in the year, say, $350{,}000 \times 12 = 4{,}200{,}000$ tons of water raised 85 degrees in temperature; or, as the usual expression is, $4{,}200{,}000 \times 85 = 357{,}000{,}000$ ton-heat units have been absorbed by the water. If the heating-power of anthracite coal is estimated at 7500 heat units to the ton, the heat in this water is as much as would be obtained from the combustion of 47,700 tons of coal. A cord of pine wood weighing 2700 pounds will probably give about 4300 heat units in practice, so that 84,000 cords would be necessary to keep up the heat withdrawn from the rocks in the mine waters alone.

If 10 tons of air pass through the mines collectively each minute, or 14,400 tons daily, and the air when discharged from the mines has an average temperature of 92° F., the total quantity of air for the year will be 5,256,000 tons, and the average rise in temperature 42 degrees. The specific heat of air being 0·267, we have—

$$5{,}256{,}000 \times 0{\cdot}267 \times 42 = 58{,}940{,}784 \text{ ton-heat units}$$

for the amount of heat absorbed by the air. This corresponds to an expenditure of 7859 tons of anthracite coal, or 13,707 cords of wood. The total quantity of heat carried out of the mines yearly by the water and air is therefore 416,000,000 ton-heat units, to produce which, in ordinary industrial operations, would require 55,560 tons of anthracite, or 97,700 cords of wood.

The number of men employed under ground in the mines of the upper Comstock is less than three thousand, and the

heat from their bodies, together with that produced by the burning of the large numbers of candles, could not account for any considerable proportion of this heat. Indeed it may be assumed, in the absence of calculations, that all the heat from these and other ordinary sources of heat in mines is no more than sufficient to compensate for the large amount of refrigeration produced by the liberation of the compressed air which is employed in every mine to work numerous underground machines. This heat absorption has not been taken into account in the above calculations.

In another respect, also, these calculations are defective, and give results very much too low. Usually the air enters the mine dry and leaves it saturated with moisture, the evaporation of which indicates an amount of heat absorption, which would probably increase the above figures surprisingly.

These calculations, imperfect as they are, show that the source of heat is one that acts on a magnificent scale, and also that it cannot reside in the small quantity of pyrite which is oxidised. That source is probably the chemical alteration of the felspathic minerals of the propylite and other rocks. This change consists apparently in the process of transforming felspar to clay, technically known as kaolinisation, from the fact that china clay, or kaolin, is produced in this way.

With regard to the future increase of the heat it is judged that, until water temperatures above 154° F. are observed, nothing in the present condition of things indicates the certainty that the heat will ever rise to the boiling-point of water, 212° F. It is rational to suppose that the access of atmospheric air and water must diminish in proportion to the depth after a certain point is reached. At that point the temperature will be at a maximum. Below it there will be a state of equilibrium, probably for a very considerable depth. Below that the heat may diminish even to a point below that of the highest of the three zones. There must be some point where the absence of drainage allows the water to act like a blanket over the rocks, protecting them from the action of air or gases from the surface. The known depth required for the production of a temperature amounting to 130° F. is so great that we may fairly doubt whether air or water penetrate to lower depths in quantity sufficient to maintain mineral decomposition with the activity necessary to obtain the boiling temperature.

The author's explanation of the heat phenomena connected with these remarkable mines supposes the existence of a

cold, and what may be called a burnt out, layer of rocks, extending for 1000 feet below the surface, a zone of hot rock still in active decomposition, which has been found to exist for a depth of about 1500 feet more, and no doubt extends thousands of feet further, and, finally, a mass of cold rock at a great depth, which has not yet begun to decompose.

The peculiar bands of hot and cold rocks which the author describes are simply layers of rock in which decomposition has been delayed or hastened. When the texture of a rock is such that it resists decomposition longer than other layers in its neighbourhood, it will be at its maximum temperature long after its fellows have passed theirs and cooled down, and this the author conceives to be the situation of the hot bands. They are individual layers of rock undergoing delayed decomposition.

On the other hand, when a rock is peculiarly susceptible to the action of the air and water, its alteration will proceed more actively than that of the surrounding rock. It will, therefore, pass its maximum temperature sooner, and be cooled down by the time that its neighbours begin to be at their hottest. This is the state of the cold bands. These bands, in fact, offer at several places in the mines examples in miniature of the action that is going on upon a grand scale throughout the whole system of rocks.

All the known facts strengthen the supposition which is advanced in this report, that the heat in the mines is subject to a steady and moderate increase as their depth is increased, this comparatively regular progression being broken by the passage through belts of rock heated above the average of the "country."

The author then considers the relation of temperature to depth, and concludes his paper by remarking that the Comstock mines offer a greater promise of discovery in this matter of rock temperature than any other he is acquainted with. The extraordinary rapidity with which their operations are prosecuted, the extent of the works, and the fact that they open to inspection a great eruptive mineral lode thoroughly for two miles in length, and partially for many thousand feet more, give them unusual value as a field for investigation. They not only follow an eruptive dyke throughout its course, but they also explore a parallel system of eruptive rocks by crosscuts, which are often from 300 to 500 feet long, and sometimes stretch out to 1000 feet and more.

They are also certain to be opened to much greater depths than now, and with a rapidity that will no doubt make them

foremost in deep mining within a few years. These conditions, combined with the peculiar susceptibility of the country rock to decomposition, give good reason for expecting that they will before long be the scene of thorough and perhaps conclusive studies in this interesting subject of earth temperatures. This was but a secondary part of the author's work, which was chiefly confined to a geological study of the lode.

IV. MATTER ACTIVE.

"Tandem venitur ad formam substantialem qua adhuc abstracta per intellectum, remanet quoddam valde occultum quod est prima materia."—ALBERTUS MAGNUS.

JOHN DALTON.—You spoke to me lately of matter being dead. Death implies previous life, and in matter you will allow also that it implies capacity of living. We cannot after all expect matter to be alive of itself,—*i. e.*, isolated from the universe,—and is it not true that physicists have a general belief that when heat is gone the worlds cease? Human beings may be taken to illustrate this. If we consider any act—our walking, for example—it is only in part an act of our own; we rise up, and immediately gravitation draws us down; and if we look at dancers this mutual action between the earth and man is more apparent. The dancer seems to be glad because, for part of a second, he has overcome the attraction of the great earth; but he need not exult long, since down he must come, and the rhythmic struggle is a graceful tune of motion. It is a combined action of man and external nature, and similarly the elements are inert only when there is no sympathetic power to act with them.

Roger Bacon.—The analogy requires more than sympathy to complete it. The human being has a force within himself, and do we not find that analogous preparation in the molecules? The attraction of the earth is only a mean part of the work in dancing; the sun, too, if it does warm the elements and allow them to act, does no more; it gives no vitality that we know of, even when it removes that stiffness

of cold which, as we before agreed, brought death to the matter which we see. Why, then, do people preach to us about the sun being the life of the earth and the source of all life? To those who say so I turn, with apparent contradiction, and say—No, the life is quite as much in the molecules. The sun gives probably several motions, but it cannot transfer its vitality to stones; the composite organism receives it, and disposes of it; the poor organism of the flint only becomes warm; the complex organism of the man glows with all his soul, the less complex molecules are moved in a still varied way. If they are of one original material, it may be asked, why does not heat set them in motion in exactly the same way? But we find them showing nearly seventy characters, *i.e.*, we have seventy bodies with fundamental differences to external appearance and tests. This is certainly not according to the development theory as we find it taken for granted in early writers, who uphold the existence of a *prima materia* or an undefinable *Yle*. Without heat it was agreed that there would be no action; with heat the elements develope many and peculiar characteristics. The first is simply that the motion which we call heat is transferred to them, an action of the same kind in all bodies whether solid or gaseous elements; by it iron and oxygen both have their particles more violently agitated. The next motion is different; it arises from a characteristic of the molecule—the oxygen and the iron unite. This shows a compound character in these substances; one for motion simple (let us suppose) in heat, another for combining. How many more we do not know, but certainly the second has very many modes of showing itself. If heat warms up the molecule its hidden qualities come out (just as we find when a starved man is set by the fire to warm), and all the various properties of the elements come forth more thoroughly than character from seventy people. In other words, the molecules have character; and is it not a general fact in Nature that character of any kind has some organisation to produce it? If we wish to give a compound mechanical nature to a bar of iron we make out of it several parts, and produce a more or less composite machine, and according to the work it does is the amount of organisation given to it. In animals there are found organs, material embodiments of character, and so in plants, although when we go down very low in size, or high in subtlety, we cannot see them. If a molecule can do several things, is it not probable that it is because of these several parts? Heat cannot make a simple piece of iron do the work of an engine; why, then,

should we suppose a new law for a molecule, making a simple body do complex work?

John Dalton.—Allowing this argument to have some force, I cannot look on it as a final proof, and, even if it were, it is not any proof whatever that the original substance from which these atoms, as I call them, may have been made is the same for all; we may make our wheels of wood, iron, or brass.

Roger Bacon.—True; but all the organisations of plants and animals are made of the same materials, and it would appear that the organisations of the universe are also made of the same substances as the earth. If we seek a more simple substance, we may be satisfied that the same will do for all. There seems an analogy between your atom and a mechanism, and for my part I look on all the molecules of the elements as little heat engines; they do nothing until they are driven. This seems at least to apply to their chemical qualities. Interrupt a fly-wheel, and we have friction and heat; interrupt a molecule by some other molecule with which it combines, and we have heat also, and a stoppage of its ordinary motion; work in this case is done, and this work is the fabrication of a new product, a facture by the hands of Nature—a manufacture.

John Dalton.—I could agree to this facture, as you call it, in a sense; but are you not simply going back a stage to meet the difficulty as great as ever? If you make the usually received atoms of Newton to combine, do you not really do enough, giving them at their origin characteristics now found impressed on them by Nature?

Roger Bacon.—I imagined that an answer had been given to this: if we do so we make a great many compound bodies as the act of the first creation, and I might as well say why not suppose all the compound rocks to be made at once? why not allow the fossils to be the result of "a striving of inorganic nature after organic forms"? why do not all men rise ready grown or made from the red earth? This does not seem to be the will of Nature in any of the departments we have observed; it is unfair to suppose it in the small masses we speak of. This is an argument of probability, the compound character of your atom is an argument of fact, to my mind.

John Dalton.—You have quite forgotten one point. The atoms are not dependent on heat; they have the power of making it. They all gravitate, whether hot or cold, and if they were only allowed space they would rush together and produce heat in abundance, to give scope for all their chemical activities.

Roger Bacon.—We spoke lately of the very ingenious speculation of making supposed matter from ether; it agrees with the opinions of—let us say, for example—Albertus Magnus, who said, as the heading of this article denotes, that having taken away all the accidental forms we arrive at a substantial form, which being removed by the intellect there remains a something very occult, which is the first matter.

John Dalton.—I know something of the search for the *prima materia*, the something more than the elements, the fifth essence. Do you mean me to return to that mode of thinking from which, so happily as I think, Europe is freed?

Roger Bacon.—If our present atoms are compounds you must return so far as to seek for a substance out of which they were made, and I think that you are called on to do so intellectually even now, although I do not see a clear way of experimenting definitely. You have alluded to gravitation as presenting a mode of producing heat and setting all the phenomena of chemistry in motion, and certainly that power is wonderful. But if we were right before, then gravitation comes in after the atoms—molecules—are made. If the atoms were made from some other substance, we have no idea what that is, and cannot at any rate suppose it to gravitate; if it did we should find it accumulated surely; or if it could not be found by our usual observation it would be, to say the least, different from all our matter as we find it in the elements. We are therefore driven to a something which we cannot perceive, and which is not known to gravitate. If it does not gravitate it could not make these concussions to which you allude, and which produce heat. This matter, ether or otherwise, cannot therefore have made the suns by such means, and as a consequence the suns would not be the original source of the elements. This logic is not absolute, because we can suppose that some other power has brought the matter together; that, however, takes us another step back.

John Dalton.—You mean to say that you do not know anything to make atoms of, and you rush to a supposed something. I have them already made out of nothing at once.

Roger Bacon.—The elements all differ among themselves, of course; but some are like others, and show a brotherhood distinctly, as if the children of the same parents. Then the atomic relations distinctly point to parts made up in some manner, as distinctly as any of the qualities of com-

pound bodies point to more than one part, as distinctly as potash and soda pointed to a decomposition in the eyes of Davy. We are therefore compelled to look for a something out of which they were made, and it is quite fair to suppose that body to be hydrogen, at least for the present, and that is a body with weight. My opinion, however, is that it is too like other elements to be the original one; it is too complex in character. I look to a simpler. I have called it *Yle*, or simply matter if you choose, matter abstractly with none of the known qualities of elements. These must be added in the school of creation. I should like a power to make the elements out of this Yle. You offer me the attraction of gravitation, but Yle does not gravitate. I have tried to get the sun to make them, and I confess it is a power so tremendous that it is a fair field for much speculation. There may be various stages of heat, and the atoms may become various at these stages, and there may be other powers besides heat, but so long as we cannot tell what will produce heat in ether without the finished atoms our speculation is imperfect.

John Dalton.—In any case you are driven back a step, and your difficulties are not diminished. You have yet found no power to make your little engines, your molecules, your atoms of the present. For my part I see nothing beyond, and am satisfied to begin my study of creation as late in its history as their facture.

Roger Bacon.—I know that I am driven back, and I recognise a time of creation when all was waste and void, when matter reflecting light did not exist, when atoms with chemical properties were not, when suns were not present to attract and planets for moving round were not formed, when dust itself did not show its presence in space, and life was of course far from appearing. I have arrived at this point at which so many have arrived before me, looking at the eternal ether, if not in the same way as Aristotle did, and looking at the primitive material as far more subtle than our grosser bodies, partly as the alchemists did, but by no means exactly so. I gave this idea in my works—that Yle produced all things, and that through it everything could be changed into everything. You will readily see that, if the elements are made of an antecedent, the step can be by the same power reversed, and matter may lose its existence and pass into ether. We can then suppose the sun to be a great agent for breaking the elements up into their primary parts, as well as we can suppose it making elements out of the *prima materia*. In whatever way we come to this *materia*,

whether by beginning with it or returning to it, we are obliged to think of power—By what means did it produce atoms? The ether as it exists, be it ether or gas, conveys power; it therefore holds it in possession for a time; heat and magnetic influence from the sun are proofs. Some men tell us that power exists only in connection with our elements; this is not known to us; we judge of its amount and quality by its action on our elements, but we cannot suppose it arriving from the sun without passing the intermediate space. Power, therefore, exists of a kind very quick and subtle, and of a kind different from heat, and that power or these powers may be able to transform the elements, whether that itself be convertible into heat or not. There may be many powers made out of one original power, and this would seem better proved than that there are many elements made out of one original element. In any case their wondrous effects are partly seen, and the existence of these agents give us some clue to the very numerous phenomena which Nature produces—all of them, one might say, quite inexplicable. He that says that he understands one of them, let him explain. The object of this is to show that heat by concussion is not the original phenomenon; that heat also is not the original in all probability, but that elements must be formed before either exist. Something out of which they were formed, or are being formed, must have existed, or probably does exist, and power at any rate we must have to do anything, and that power is as mysterious as ever. If this is right reasoning, the history of the Universe has other stages than have been conceived; and as the body of man decomposes and composes, producing his life, so the Universe may be breaking up its matter and forming new matter at the same time. By this idea another glimpse is given us into the mode of comprehending the existence of time. We dare not say anything of the past and dread inconceivable eternity.

John Dalton.—When you get into that vague and poetical region I am obliged to assent, meaning that I believe that all is wonderful and mysterious; but have we made anything out of the discussion except this, that matter can do nothing without power, and that the power is not in the matter, such as we have it in our elements. If you mean so, I most gladly agree.

Roger Bacon.—We have made out that the power began not by forming elements, but an original something very subtle, and I repeat this to fix it on your mind, because I feel sure that you will soon give up supposing our atoms to

be simple. This is an important conclusion, because it not only means that these little engines—atoms—cannot work without power, and they could not be made without power, so that, instead of being the actual powers that govern the world, they are in a double stage of helplessness, both requiring to be made and driven like other machines.

John Dalton.—You certainly gain a view farther back in creation, and have an idea of evolution of matter by this mode of thought, and no length of stages can change my knowledge that power must exist originally. Even those who tell us that heat, and therèfore life, is got by gravitation merely reason in a circle; for what is gravitation? I am quite ready to believe that it is one of those characteristics that have been given to the elements, from which you are fairly entitled to call them compound; for even in my system I give a very compound nature without having thought of a compound structure to account for it.

Roger Bacon.—We must not stop here. The farther we go back the less does it appear to us that our matter has great power; it seems neither to make nor to unmake itself, and those who look to matter as beginning with excess of power and gradually dying out, only renewing itself partially by concussions until the final destruction, make a strange supposition, because the supposition involves a beginning of power made by matter, and a beginning of eternity, if we may so speak, as well as an end, so far as active matter is concerned. I see neither and can comprehend neither, and it seems to me that we are led in this reasoning back to power quite inconceivable developing matter: why this power did it so once and does not do it again there seems no reason for thinking, and it is possible to suppose this creative power as eternal more easily than it is to suppose the mere work which it performs to be eternal. If it worked before our mountains and our earth, it may work after them. It is just possible that the sun may prove a focus for Yle, to which to rush and be made into elements, and thus accounting for gravitation to the sun; but this would not account for mutual action. It is also possible to think that the sun may be using old material, making it new, and thereby lighting up creation with the rubbish. But whatever be the case our matter is not active enough to keep the Universe in motion of itself, and at best it may retain an impetus which may keep it going forward for a time, but of itself it is a slave; it works only when it is driven; and we are driven to conclude that neither from matter of our elements nor ether is obtained the power or forces that rule them.

We have been obliged to go farther back than the formation of our elements to seek the power, and lest the clue of the reasoning be lost I shall repeat the stages :—

The elements known to us will not act without heat.

They are therefore without life in themselves.

Heat is conveyed to them ; from the sun, for example.

There must be an agent of communication if heat be mere motion.

This agent we call ether,—let us spell it æther,—and to convey vibrations it must be material. (In excessive cold, communication of heat even from solid to solid can only be by means of æther.)

It would seem that æther does not gravitate ; gravitation is therefore not an essential of matter.

If we freeze down our elements to the most inert condition known, they still gravitate.

Therefore they are not the simplest matter ; they have a quality beyond that which æther has.

If made of anything we suppose the simplest thing we know of—we call that æther, but there may be a simpler still —let us say Yle.

Then as to priority. Heat made by the rushing together of the elements could not have been the original act of creation, because the elements, as we have them gravitating, require first to be made ; for we have seen that æther, or matter, is free from one of the most general qualities of our elements. It is simpler.

Heat therefore is not the great fundamental power of Creation.

Heat is a power to drive the elements when made.

We come, then, to an original, or at least an early, matter, less like matter than our own, and we seek a power to make elements from it.

Heat does not give life or activity to the elements ; it only produces one of the conditions in which they are able to act. Having various characters they must be variously organised, and heat sets them in motion accordingly.

Whenever we follow matter carefully we come to helplessness, immobility, death, unless revived from without. It is set in motion by a something which is manifestly separate from itself. But there are many intricacies in the thought, and we shall leave it for a while, if you please.

John Dalton.—There is nothing in your ideas opposed to mine. I was contented with that which I saw, but I must confess that you are more likely to be right than I was, in boldly making the atoms engines, as you call them, or com-

pounds. I called them simple, and yet in reality I gave them so many properties that they became each a focus of powers instead of mere atoms, and I am willing to look farther back as you do for the beginning of things.

V. SOME NEW OPTICAL ILLUSIONS.*

By Silvanus P. Thompson, B.A., D.Sc.. F.R.A.S.,
Professor of Experimental Physics, University College, Bristol.

IN the Transactions of various learned bodies—the Royal Society, the British Association, &c.—papers have appeared from time to time describing various Optical Illusions. Some of these illusions have depended upon the duration of retinal impressions, some upon the formation of accidental subjective images, some upon the dispersion or irradiation of the eye, and some upon the phenomena of binocular vision. The illusions to be described in the present paper do not fall exclusively under any one of the heads enumerated, though they depend upon the duration of visual impressions, and upon a further and less perfectly understood property of the retina. They are all dependent upon *motion*, either of the object or of the observer, or of both. In each case that will be here brought forward there is a movement of the object across the field of view, and consequently of the image across some portion of the retina.

The most frequent illusions which arise thus are those in which one form of motion apparently takes some other form. As a most familiar instance of this kind of illusion we may take the case of the apparent motion of trees, hedgerows, and houses, as seen from a rapidly-running railway-train, the deception of the senses being most complete when the personal sense of motion is least.

When the train in which you are seated is drawn up beside another train, and then moves slowly forward,

* The greater part of this article was read before the British Association, at Plymouth, in September, 1877, an abstract only having as yet been published. A few additional facts were recently communicated to the British Naturalists' Society, and are embodied herewith.—S. P. T.

smoothly and without jolting, it is extremely difficult to tell whether your own train or the other one is in motion.

So when light clouds are drifted across the moon, one can frequently hardly resist the notion that it is the moon that is sailing along amongst fixed clouds; and if the drifting of the clouds be due to an upper current, while the lower air is still, the impression that the moon is sailing along past the clouds asserts itself with remarkable force.

I have observed an illusion closely akin to this at Clifton. Underneath the famous Suspension Bridge a zigzag path winds up to the top of the cliff, shaded overhead by trees. Walking up this path you see the bridge at intervals between the boughs, and, as the body rises and falls with the motion of each step, the bridge appears to be swaying violently up and down, as if it were blown about in the wind.

Many illusions akin to these very simple phenomena have been recorded from time to time. Three times—in 1845, 1848, and 1861—the late Sir David Brewster drew the attention of the British Association to some phenomena seen in railway travelling. If from the window of the carriage you look out at the pebbles and stones lying beside the line, you catch merely vague stripes, due to the rapid motion of their images across the retina; but on suddenly shutting the eyes "a motion is perceived in a direction transverse to the real impressions on the retina; and there is the appearance of lines complementary in the same transverse direction."* This Sir David subsequently referred to a subjective opposite motion going on simultaneously, and so causing a compensation of the impressions moving on the retina. In 1861 he returned to the observation, and compared the phenomenon with that obtained by watching the motion of a rotating disk with radial markings, directing the eye first to a point near the circumference, and then afterwards to a point near the centre, where the motion was slower. He concluded that there was a neutral line across the retina at which the compensation of the subjective impression was complete.

In the "Philosophical Magazine" for 1834 (p. 373) R. Addams described a peculiar optical phenomenon. After looking for some time at a waterfall, and then at "the sombre water-worn rocks immediately contiguous," he "saw the rocky surface as if in motion upwards with an apparent velocity equal to that of the descending water." This he ascribed to an unconscious recurrent movement of the

* Brit. Assoc. Report, 1845.

muscles of the eye-ball, continuing after the gaze had been directed to the rocks, and thus occasioning a displacement of the images on the retina.*

This illusion becomes more remarkable in the slightly varying case now to be mentioned. Watch the water of a rapid river, such as the Rhine immediately above Schaffhausen. The middle stream is running forward very rapidly. After watching it fixedly for some time, transfer your gaze to the slower stream near either bank. It actually seems to be running back.

I have also noticed, after watching a procession, that stationary objects appeared for a moment to be moving in a contrary direction.

In the "Journal of the Royal Institution" (vol. i., p. 609) an anonymous writer records a curious observation, that from a slowly-moving railway-train, while the stones and sleepers beside the line appear to fly back past the train, the neighbouring set of rails seems to be flying forward and keeping pace with the train. This he refers, and doubtless rightly, to the fact that the rails are of nearly uniform tint, and destitute of markings that would produce upon the retina impressions like those of the adjacent objects.

The railway affords many other instances of optical deception, and of these I will mention a few of which I am not aware that any specific notice has hitherto been taken.

When a landscape is observed from a moving railway-train, all distant objects from the near hedgerows to the distant hills appear to be moving past in the opposite direction, the nearer objects having the greater apparent velocity. Consequently, if the attention be fixed upon any object at some distance from the line, all objects beyond will relatively appear to be moving forward with the train, while objects nearer appear to be moving backwards. The combined effect is to make the entire landscape appear to be *revolving centrally* round whatever point we fix our attention upon.

Falling rain seen from a moving train always seems to fall obliquely (except in a *very* strong gale in the direction of the train's motion) in a direction opposite to that of the motion of the train. But if another train happen to pass in an opposite direction, and we look out at this and follow it with our eyes, rain-drops falling between the two trains will seem to be flying forward with ourselves.

* An account of a very similar observation was communicated by Mr. J. Aitken to the Royal Society of Edinburgh, in November, 1878, apparently without any knowledge of the observations of Addams, Brewster, or of the author of this article.

If we stand on the platform of a station and watch a train approach, the end of the engine appears to enlarge or swell up as it approaches and occupies a larger area of the field of vision. Conversely the end of the last carriage of a retreating train appears to shrink down and contract as it diminishes in apparent magnitude. Stationary objects by the side of the line similarly appear to swell up as we approach them in a train, and to shrink together as we retreat from them. Curiously enough, this motion is also one which calls forth a certain "compensation" in the action of the retina. For, suppose we have been watching objects enlarging as we approached them, and then suddenly transfer our gaze to the side of the carriage opposite to us, we shall observe that it is apparently shrinking together and retreating from us. The opposite effect—that of apparent enlargement and approach—is produced as a subjective compensative action after watching objects from which we are retreating. The effect is more amusing if, after observing either of these cases of motion, we transfer our gaze to the face of a fellow-passenger sitting opposite.

An observer at some slight elevation above a railway, seeing two trains pass along the lines simultaneously in opposite directions, will receive the impression as of one long train moving round a circle. For when you look at a revolving wheel nearly edgewise, the nearer edge is seen moving past the farther edge, and in an opposite direction. The apparent motion of the two trains is the converse of this impression.

If from a similar situation two trains are observed, one moving rapidly, the other slowly in the same direction, the slower train may appear indeed to be moving in an opposite direction—a phenomenon similar to that of the Rhine above Schaffhausen already noticed.

Dr. F. Guthrie has noted the following illusion :—" Looking at the arms of a windmill in motion, in the twilight, or at such a distance that their attachment to the mill is obscure, we can, when the aspect is very oblique, easily imagine the arms to be turning in the opposite direction. We then fancy we are looking at the other side of the mill: so that if the sails are actually towards us in their descent, we fancy them away from us in their descent, which gives the notion of rotation in the opposite direction. This hallucination can, after a little practice, be as readily controlled by the will as can the introversion of a linear drawing representing a solid." *

* GUTHRIE, Magnetism and Electricity, p. 243.

An analogous illusion is produced by illuminating certain vacuum-tubes with the sparks of induced electricity discharged alternately in opposite directions,* when the tube appears to be rotating about an axis perpendicular to its length and to the line of vision.

A crow flying along at dusk, seen against the sky at a low altitude, shows, when passing the observer, his wing above and beneath his body alternately. The effect of this alternation is as if he had but one wing, which seems to revolve round like the blade of a screw-propeller about its axis.

I have frequently stood upon the lofty suspension-bridge over the Avon, at Clifton, when large ships have been passing beneath. Under these conditions a curious illusion may be observed. If you look perpendicularly down on to a ship, as it emerges from beneath, it appears to be heeling forwards on to its bows; for as the masts emerge from under the bridge, and you see them growing longer as the foreshortening effect passes off, the mind cannot resist the notion that—like the windmill-sails—they are revolving round a centre. I have pointed out this effect to several persons, who have expressed much surprise at the completeness of the illusion.

The last set of illusions which will be described took their origin in an observation made by the writer early in 1876. He had been drawing a series of concentric circles in black and white, for the purpose of testing the astigmatic condition of the eye. Happening to shake the paper upon which the diagram was drawn, he observed a peculiar motion of apparent rotation of the circles. This illusion is extremely curious, and very easily reproduced. Let concentric circles in black and white be described upon a piece of card. If this be held firmly between the thumb and finger of the hand, and then a slight but rapid circular shaking motion be imparted by the wrist and elbow, the circles will appear to rotate upon the card. The hallucination succeeds best if the circles be clear and sharp at their edges, and the successive rings of black and white of equal widths. Their number and width is immaterial, but there seems to be a particular distance from the eye for each width of successive rings, at which the illusion succeeds best. Finely-drawn narrow rings must be held near, to produce a maximum effect; while to enable a number of persons to see the illusion at once the rings may be half or three-quarters of an inch in width, and to the number of fifteen or twenty.

* See S. P. Thompson in Phil. Mag., 1876.

The radius of the circle of imparted motion should equal the width of a black or of a white ring, and the rapidity found most successful is that when each rotation occupies from one-sixth to one-fourth of a second. The rings appear to rotate once for every complete motion of the hand and card in the circular path, and in the same direction as the imparted motion. In this experiment each ring is displaced to a distance equal to its own breadth in every direction successively around its centre; and as the impression remains a short time on the retina, the optical effect is equivalent to that of a ring eccentric to an equal amount and actually rotating. Hence the illusion.

I have constructed a large number of patterns of curvilinear, circular, elliptical, eccentric, and concentric lines, many of which exhibit, in whole or in part, the same phenomena of apparent rotation. One of these is a single black circle, having a number of internal cog-teeth, upon a white ground. This circle, when shaken circularly in the manner described, appears to move round in the opposite direction to the imparted motion, and to move round through a distance of but one tooth for each successive complete motion. For circles possessing this property I have suggested the name of "Strobic Circles." Their motions are best seen when the eye is directed not exactly at the circles, but at some point near them. I have therefore found it more effective to have two strobic circles drawn side by side upon one card. That circle rotates most obviously on which the gaze is *not* fixed.

Further, I have noticed that if a strobic circle be "rotated," while a number of other circles are lying stationary within the field of view, when the eye was directed to the moving circle the others also began to "rotate."

This last observation cannot, I think, be explained on any supposition of unconscious muscular movement. In fact I entirely doubt the validity of this hypothesis in the case of Addams's observation upon the waterfall before cited.

I am inclined rather to attribute these effects, and those of "compensation" in general, to waves of nervous disturbance moving over the retina; these waves, being of two orders,—one primary, and in the same direction as the objective motion of the images upon the retina; the other secondary and later in time,—giving rise to the subjective motions of compensation. I do not see how on any other supposition the phenomena noted in an earlier paragraph relative to compensative shrinking or expanding of objects can be explained. Such a hypothesis, will, I believe, also embrace all the other phenomena of apparent motion, except those which

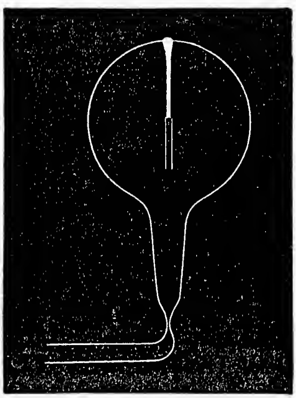
are the result of mental associations alone—such illusions, in fact, as those of the windmill and the flying crow. Such waves of nervous disturbance have, it would seem, a definite rate of propagation, probably not independent of the nature of the moving image with respect to colour, relative luminosity, and apparent magnitude. But whether these waves of sensible impression are due to a physical motion of any structures of the retina I am not yet prepared to offer an opinion.

VI. ON ELECTRICAL INSULATION IN HIGH VACUA.*

By William Crookes, F.R.S.

THE following experiments were suggested in the course of an investigation on the passage of an induction current through highly exhausted tubes. The main branch of the research being likely to occupy my attention

Fig. 1.



for some time, I may be unable to return to these less important offshoots.

A pair of gold leaves were mounted, as for an electroscope, in a bulb blown from English lead glass tubing. The leaves

* A Paper read before the Royal Society, February 20th, 1879.

were attached to a glass stem, and the lower part of the bulb was drawn out for sealing to a Sprengel pump, as shown at Fig. 1. A stick of ebonite excited by friction was generally used as the source of electricity, but any other source will do equally well, provided it is not too powerful.

No special attention was paid to the action of electricity on the leaves in air or at moderate vacua, as it agreed with what is already well known. The exhaustion was pushed to a very high degree (about the millionth of an atmosphere), when it was found that the excited ebonite had a much greater effect on the gold leaves than at a lower exhaustion; for a long time, however, I was not able to charge the leaves permanently, in consequence of their falling together as soon as the source of electricity was removed.

When a hot substance was brought near the bulb facing a gold leaf, so as to warm the glass, molecular repulsion took place, and the leaves retreated from the warm spot, standing out at an angle of about 45°. As the glass cooled the leaves resumed their former vertical position.

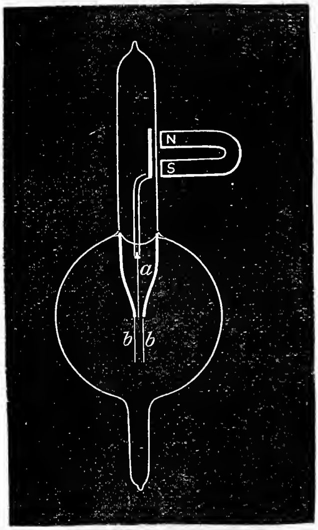
While the leaves were repelled from the hot glass the excited ebonite had a very powerful action on them, and if it were brought near hastily the leaves flew off to the side of the glass, destroying the apparatus. By careful management and repeated trials, however, the ebonite could be brought near the warm spot of glass, the leaves suddenly extending at an angle to each other. The appearance was as if a spark had been able to pass across the bridge formed by the line of advancing and retreating molecules connecting the hot glass with the gold leaves. On the ebonite being removed and the glass allowed to cool, it was found that the repulsion of the leaves was permanent. The rubbed ebonite would attract and repel them as it was moved to and fro, but the angle formed by the leaves with one another remained unchanged. A warm body brought near the glass opposite one leaf would repel the pair as a whole; on then warming the opposite side of the glass repulsion on that side took place, the angle of the leaves being somewhat diminished, but on cooling the leaves opened again to their former extent.

When the glass bulb was strongly heated by a spirit flame the leaves suddenly discharged and fell together.

Another bulb (Fig. 2) was prepared, containing a plate of mica, *a*, which could be suddenly placed between the gold leaves, *b b*. The plate of mica was longer and wider than the gold leaves, and was connected with a small piece of iron wire, capable of moving up and down a tube sealed into the

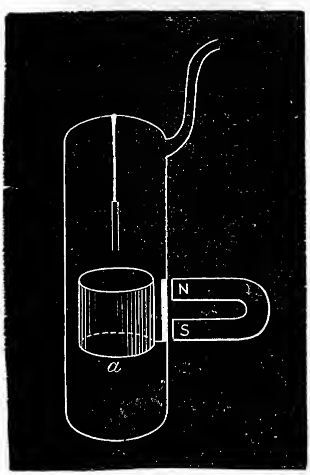
top of the bulb. By means of an outside magnet the mica plate could thus be lowered between the gold leaves or raised

Fig. 2.



out of their way, as desired. The tube was exhausted to about the millionth of an atmosphere, the mica plate being

Fig. 3.



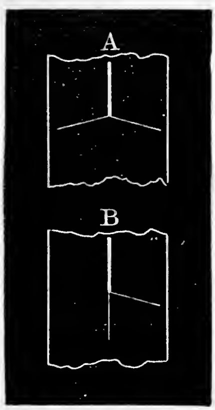
held quite above the leaves. One side of the bulb was then heated, and the leaves permanently charged by means of the

excited ebonite. The mica plate was now carefully lowered. As it came between the gold leaves they diverged farther apart, and kept so as long as the mica plate was between them. On removing the plate the leaves re-assumed their former divergence. This could be repeated any number of times.

A similar piece of apparatus (Fig. 3) was made, only instead of a mica plate coming between the leaves, a mica cylinder, *a*, capable of being raised and lowered outside the divergent leaves, was employed. I was not able to get entirely concordant results with this, owing to the friction of the mica developing electricity on the inner surface of the glass tube; but in all cases, when the cylinder was raised until it covered the electrified leaves, it had the effect of diminishing the angle which they formed with each other.

The following experiments were also tried:—The leaves being separated about 160°, as at Fig. 4, A, one side of the tube was slightly heated by a spirit-flame. The leaf on that side fell to a vertical position, and remained so when all was

Fig. 4.



cold, the other leaf sticking out as before, as at B. This would seem to show that the divergence of the leaves in this case was not so much due to their mutual repulsion as to an attraction exerted on each of them by the inner surface of the glass tube. The remaining divergent leaf could be slightly lowered when the glass tube above it was warmed with a bunch of cotton-wool dipped in hot water. On cooling the leaf rose again to its original position. When this side of the tube was also heated with a lamp, the leaf was repelled down, but not so readily as the other had been, and

when the tube got cold it rose to nearly its former position. This was repeated several times with uniform results. When the leaf was repelled down the vertical leaf also moved away, so as to keep the same angle between them. It is therefore evident that the leaves themselves were also charged.

FIG. 4.

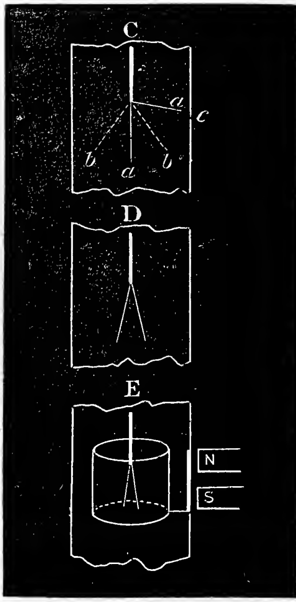


Fig. 4, C, shows the two positions of the leaves, *a a* before applying heat to the side *c* of the tube, and *b b* after heating the glass at *c*.

The tube was now heated on both sides, causing the leaves to come nearer together, as shown at Fig. 4, D. While the glass was warm the cylinder was raised so that it surrounded the leaves: this caused them to get a little closer together, and they kept in this position, shown at E, after the whole apparatus was quite cold.

After remaining thus for some time the cylinder was lowered, and the leaves widened out and took up the position shown at *b b*, Fig. 4, C. They did not return to the position *a a*, showing that their divergence was now owing to their own mutual repulsion, and not to an attraction of one or other to the electrified glass.

In December, 1877, I totally immersed one of these exhausted glass bulbs in a vessel of water, the gold leaves

having previously been charged, and standing at an angle of 112° from one another, as at Fig. 5. The water was connected electrically with "earth," and the whole was set aside in a cabinet on the 1st of January, 1878.

At the present time, after having remained in this condition for thirteen months, the leaves form exactly the same angle with one another which they did when they were first put in the cabinet.

Fig. 5.

From this experience I think we may consider that at an exhaustion of a millionth of an atmosphere, air is an absolute non-conductor of statical electricity. It is therefore legitimate to conclude that the vacuum of interstellar space offers equal obstruction to the discharge of electrified bodies, without necessarily interfering with their mutual repulsion if similarly electrified. It is possible that in these facts an explanation may be found of some obscure celestial phenomena.

VII. SPIDER'S WEB FOR MICROMETERS.

THE web used is that of the common garden spider; the spinner of those geometric webs is abundant in summer and autumn. The reason why this spider is selected, rather than the house spider, may easily be seen by anybody who watches the proceedings of each when a fly is caught. The house spider seizes his victim and spins around him a winding-sheet of web, before carrying him

into the larder. The garden spider binds around his captive a cord of web. It is this cord that is used for collimation.

In the autumn the mathematical instrument maker goes on his spider-hunting expeditions, generally on Sundays. He carries some pill-boxes in his pocket; selects well-fed, full-grown specimens, and puts each in a separate box, knowing the savage habits of his six-legged friends; for if two or more were put together in the same box, only a collection of amputated limbs and mangled bodies would be found on returning home,

The webs are secured for use and storage by making a fork of iron wire, 4 or 5 inches long, and $1\frac{1}{2}$ to 2 inches between the bifurcations. The spider is held in the left hand and allowed to drop, which he readily does when dissatisfied with his quarters, but before falling he glues an end of cord to the finger, and then lets himself down easily by gradually spinning it out and hanging by it as it lengthens.

The instrument maker catches this cord across his fork, and, by turning, attaches it to one side; then he goes on turning the fork and advancing it, so that as the spider continues paying out his cable a series of obliquely crossing threads are wound upon the fork, which, when charged, is carefully laid in a box or drawer for use. The elasticity of the iron wire keeps the webs sufficiently stretched, and they are applied to the stop by simply laying the fork over it in such wise that one of the stretched webs shall fall upon the mark made on its face. When thus in position, a drop of varnish or glue, made by dissolving shellac in alcohol, is let fall upon each side; the spirit rapidly evaporates, and the web is fixed.

One of the odd results of this use of spiders is that many workmen become spider fanciers, and keep choice domesticated specimens that learn to spin their webs in convenient places above the work-benches, or in the bedrooms of their masters, who lovingly supply them with the fattest of blue-bottles. Though I forsook the trade on the expiration of my apprenticeship, I have not yet lost my affection for these animals—I never wilfully kill a spider. A sad story is told of the desolation of the late Mr. Troughton, who on one occasion engaged a new housemaid, and allowed her to commence professional operations without receiving the usual injunctions concerning his pets.—W. MATTIEU WILLIAMS, *in Journal of the Society of Arts.*

VIII. THE "JUMPERS," OR "JUMPING FRENCHMEN."

DURING the past year I have been investigating a manifestation of the Involuntary Life of the most novel and interesting character. It is found among the French Canadians, and is there known under the expression "Jumpers," or "Jumping Frenchmen." It appears, according to my researches, that a certain proportion of that people—mingled French and Indian blood—have acquired the permanent habit, which they cannot control, of jumping, or striking out with their hands, when commanded to do so, suddenly and authoritatively, by anyone who chances to be near them. The habit appears to have been acquired, in the first instance, by tickling one another, in the winter camps where they cut lumber in the Maine woods. They are a somewhat degraded race of beings; have few resources, very little intellect, and no mental discipline; cannot, usually, read or write, and, in their camps, while away the long winter evenings by playing upon each other's ticklishness until some of them get into a state of abnormal susceptibility that compels them to obey, automatically and instantly, any sudden order, as to strike, or to catch, or to jump, or even to vomit; they are at the mercy of their companions, and are frequently so much annoyed that they have to leave the camp where they are employed. The more they are played upon the worse they become; for the habit grows with exercise. This condition is not an epidemic, but a fixed and permanent state, and, so far forth, is different from the phenomena so often witnessed in revivals. It is, in fact, as I have elsewhere stated, a liability to be entranced on slight excitation; differing from the allied trance to which all of us are liable only in this, that it follows a very much milder irritation. When these "Jumpers" are excited to jump or strike, or to perform any of their peculiar automatic acts, they present the appearance of entranced individuals; their faces turn pale, their eyes are fixed and glassy, and sometimes their limbs tremble. One of these Jumpers is a waiter, and when told suddenly to "drop it," he at once drops whatever he may have in his hand, though it may be on the head of one of the guests, or on the floor. Another has so susceptible a stomach that he at once throws up his

meals when any one but "gags" or makes the motions of vomiting in his presence; thus he has grown thin, and at one time was almost starved. One Jumper, when told to "strike," struck against a red-hot stove and burned himself. Accidents of this kind are quite frequent in their camps. One man, standing on the shore of a pond with a five-dollar gold piece in his hand, was told to "throw it;" he threw the money—a large sum for him—into the water. Another was standing near a kettle of fish; he was told to "jump," and he jumped into the kettle. When one of these Jumpers is addressed sharply and quickly in any language with which he is not familiar, he, at once and automatically, responds in that language. Thus, in numberless ways, they are abnormally susceptible to stimuli which, in the same degree, would have little or no effect on others.

In its relation to the subject of inebriety these extraordinary phenomena are of interest as illustrating the power and extent of the involuntary life, showing how varied and complex and subtle are the manifestations of this side of human physiology. These Jumpers, in the acts here referred to, are absolute automatons, utterly without volition or responsibility. Whatever responsibility there may be in these cases belongs to the time when the habit began to be formed—their first playing and trifling with themselves and others in the loneliness of their winter camp-life; they are no more to be blamed for their acts than are patients afflicted with St. Vitus's dance, or hysteria, or epilepsy, or with any form of insanity. The treatment, if any is used, should consist in removing the victims from the temptations of camp-life; they should be isolated, or, at least, kept away from those who are similarly afflicted, or who would take pleasure in playing upon, and thereby increasing, their weakness; at the same time, everything that educates and developes their higher cerebral centres will be of service. Indeed it has already been noticed that they grow worse by aggregation, and better by isolation. Their habit is a real and serious affliction to these people,—a source of anxiety and positive torment; they would rejoice to be delivered from it.

On the other side of the world, among the Malays, in the Island of Java, according to the "London Medical Record," phenomena precisely similar to those exhibited by the "Jumping Frenchmen" are seen. A woman carrying a child, and seeing one even pretend to drop any article, may at once drop that child. Many other interesting illustrations are given.

Very recently, also, my attention has been directed to some allied phenomena connected with a religious revival now in progress in a certain town in Vermont. The victims of this excitement roll on the floor or ground in most absurd and undignified attitudes; whence they are called "Holy Rollers." Unlike the Jumpers, however, these Rollers are not in a permanent liability to their disorder; when the excitement is over they will spontaneously recover. Just at present, in the height of the public enthusiasm, they are, on this subject, pure automatons. In the same line are the cases of starving girls, hysteria, and hysterical trance, like that of Mollie Fancher, of Brooklyn, which is now exciting so much enthusiasm. I have studied a number of similar and allied cases, and I never share the popular prejudice against them. They are without volition, practically irresponsible, and to be blamed—if blamed at all—for the *beginnings*, not the endings of their disease. Like inebriates, they are to be treated by taking them from their home and friends, and giving them a radically new environment.—G. M. BEARD, M.D., *of New York.*

NOTICES OF BOOKS.

Practical Physics; Molecular Physics and Sound. By FREDERICK GUTHRIE, F.R.S., Professor of Physics in the Royal School of Mines. (London Science Class-Books Series.) London: Longmans and Co. 1878.

FIVE-AND-TWENTY years ago physical laboratories were almost unknown in this country. At the present time all our large educational institutions possess a laboratory of this nature side by side with one devoted to chemistry. The fine physical laboratories at Oxford, Cambridge, Glasgow, and South Kensington may be taken as examples of what such places should be like, and of how they should be worked. At the latter some dozens of students are successfully put through a course of practical physics every year, under the guidance of Prof. Guthrie, who has given us some of the results of his experience in the handy little volume before us. An admirable manipulator himself, he knows well how to instruct others; and this work will not only be welcomed by the student, but also by the older man of science engaged in research, who will find many useful hints as to the preparation and use of apparatus.

Starting with some definitions and experiments relating to the cohesion of solids, the author passes on to the cohesion of liquids and gases, introducing us to some of his beautiful experiments on bubbles and drops. We object, however, entirely to the term "cohesion of gases," and we cannot at all understand the scope or object of the single paragraph (No. 19, p. 18) devoted to the subject, which ends with the sentence—"A more exact method is to place timed chronometers under bell jars containing various gases, and also *in vacuo*." The term "volume-elasticity," in the next Section, is awkward; the piezometer, on the same page, could only be constructed and worked by a very skilful manipulator, and when in action would scarcely repay the infinite labour expended upon it.

The subjects of Effusion, Diffusion, and Occlusion are ably discussed, and the most recent researches in each case are introduced. The short but comprehensive chapter on Specific Gravities abounds with practical hints, and details all the necessary processes. The ingenious way in which Prof. Guthrie has applied singing flames to the demonstration of the refraction of sound by gaseous lenses (pp. 81, 82) is deserving of notice; also the graphic representation (p. 102) of the cause of the clap, rattle, roll, and boom of thunder. The subject of Interference is ably and clearly treated.

The Appendix is not the least valuable part of the book: it contains a number of useful hints as to glass-working, fusing platinum wires into glass, and so on. Finally, a list of the apparatus and materials necessary for experiments in sound and waves concludes a volume which we cordially recommend to the notice of all students of physical science.

A Dictionary of Chemistry and the Allied Branches of other Sciences. By Henry Watts, F.R.S. Assisted by Eminent Contributors. Third Supplement, Part I. London: Longmans and Co. 1879.

This very valuable and standard work, which is without doubt the most exhaustive Dictionary of Chemistry in any language, furnishes, by the aid of its Supplements, a complete record of chemical research, within a short time of its appearance in the journals of scientific academies. Research is so abundantly on the increase that this closely-printed volume of 838 pages forms only the first half of the Third Supplement. The second half will appear before the end of this year, and will bring down the record of chemical discovery to the end of 1877, including, however, the more important discoveries of 1878.

Among the noticeable articles in Part I. we may mention Dr. Mills's account of Cumulative Resolution, Dr. Thorpe on Flame, and Mr. Warington on Barley and on Forest Trees. But the great feature of the volume is, without doubt, the very exhaustive article—presumably by the Editor—on the Benzenes and Benzoic Acids, extending over more than 150 pages. This includes an account of Körner's elaborate researches on the Orientation of the Benzene Derivatives, published in the "Gazzetta Chemica Italiana," and now for the first time made known to English readers. To this subject we venture specially to direct the attention of chemists, as one which is well worthy of their complete consideration.

The subject of "Cumulative Resolution" (a term proposed by Dr. Mills) has been developed by Wurtz, Watts, and Mills. The action is defined as "the combination of a substance, or mixture of substances, with itself n times, a particular portion of it being lost each time, according to some fixed law. Thus, bismuthic nitrate, when decomposed by a gradually increasing quantity of water, yields a series of bodies, which are less and less nitrogenous, and more and more bismuthic. Having regard to the denitration alone, we write—

$$n(Bi_2O_3.3N_2O_5)-(n-1)N_2O_5=Bi_{2n}O_{3n},N_{4n+2}O_{10n+5},$$

and by giving various values to n, from 0 to ∞, we shall obtain the formulæ of all possible compounds between $Bi_2O_3.3N_2O_5$ and

$Bi_2O_3.N_4O_{10}$." It is afterwards applied to various minerals derivable from two silicic hydrates, of which the second is the cumulate of the first.

The present volume goes down to the end of F: it embraces all new matters of any importance in Inorganic and Organic Chemistry, and in Mineralogy. We do not notice any articles on physical or technical subjects, and the great mass of matter relates to Organic Chemistry.

An Elementary Text-Book of Petrology. By FRANK RUTLEY, F.G.S. London: Longmans and Co.

THE study and recognition of rocks may fairly be considered as the alphabet of geology, without a knowledge of which the student may certainly become a reader, but never a worker in the science. In other words, he may peruse handbooks and manuals, may fix their contents in his mind, and pass an examination with credit, and yet be utterly unable to make an original trustworthy observation or to verify the theories which may be put forward. It is therefore strange that certain geological text-books—to particularise would be invidious—overlook this part of the subject altogether. The reader is supposed to be born with the power of recognising the respective species of rocks, and therefore at the end finds himself no more of a geologist than he was at the beginning. Mr. Rutley, in the text-book before us, makes a useful and praiseworthy attempt to supply the knowledge thus found wanting. Into chemical methods for the diagnosis of rocks and their constituent minerals he does not enter at length, the rather as all needful information on this head may be found in well-known and accessible works. He occupies himself the rather with physical, and especially optical, characteristics. The microscope—thanks to the exertions of the late David Forbes, of H. Witham, and especially of Mr. H. C. Sorby—is now no less essential to the geologist than to the student of animal and vegetable life, and our author explains clearly and fully the applications of this instrument in petrological research.

We can strongly recommend this work to all who are entering upon the study of geology, and who wish at the outset to lay a firm foundation.

The Realistic Assumptions of Modern Science Examined. By T. M. Herbert. London: Macmillan and Co.

We were under the impression that men of Science fully, though tacitly, recognise the necessity of a general postulate underlying all their researches and speculations; to wit, that phenomena are what they seem, and act as they seem to act. No one, to our best belief and knowledge, ever supposes himself capable of furnishing an absolute logical demonstration that the impressions of our senses are an invariably accurate copy of anything external. What everyone thus fully concedes Mr. Herbert sets himself to prove. In reply to the man of Science who taunts metaphysicians with the vanity and baselessness of their endeavours, he brings a counter-charge, which is merely a learned and elaborate version of the vulgar retort "You're another!" But that he adds any degree of certainty to human knowledge, or shows how we may escape from the difficulties he has so acutely pointed out, we are unable to perceive, and consequently we can only regret that he did not employ his great powers of mind in some other and more fruitful direction.

CORRESPONDENCE.

THE CHARACTER OF THE SEXES.

To the Editor of the Monthly Journal of Science.

SIR,—Mr. Murphy, in his "Habit and Intelligence," remarks that in man only do we find a reversal of the usual character of the sexes. "In all other species beauty is developed in the sex where the passions are strongest, and consequently generally in the male." Other writers, in discussing the origin of man, seem to take a somewhat similar view. But if we could ask the lower animals, would not each declare that its own species was the single exception to the rule that the male sex is the more beautiful? Civilised travellers, while fully recognising the beauty of women of the higher race, not unfrequently pronounce that among the lower savages the men are passable, but the women utterly repulsive.—I am, &c.,

S.

TRANSFORMATION OF SPECIES.

To the Editor of the Monthly Journal of Science.

SIR,—I see it mentioned among the "Biological Notes," in your last month's issue, that M. Bordier considers atmospheric pressure as one of the principal agents in the transformation of species. If variation is most rapid where pressure is most intense, should not the deep-sea fauna be richer and more varied than it has been found to be during the late *Challenger* Expedition? Are there any observations to show whether variation is greater or less at the sea-level than on table-lands and mountain-slopes?—I am, &c.,

J. W. S.

PROCEEDINGS OF SOCIETIES.

Royal Society, *January* 23.—A paper by C. William Siemens, D.C.L., F.R.S., "On certain means of Measuring and Regulating Electric Currents," was read. It is well known that when an electric current passes through a conductor, heat is generated, which, according to Joule, is proportionate in amount to the resistance of the conductor, and to the square of the current which passes through it in a unit of time. Dr. Siemens proposes to take advantage of this well-established law of electrodynamics, in order to limit and determine the amount of current passing through a circuit. The most essential part of the instrument employed for this purpose is a strip of copper, iron, or other metal, rolled extremely thin, through which the current to be regulated has to pass. One end of this thin strip of metal is attached to a screw, by which its tension can be regulated; it then passes upwards over an elevated insulated pulley, and down again to the end of a short lever, working on an axis, armed with a counterweight and with a lever, whose angular position will be materially affected by any small elongation of the strip that may take place from any cause. The apparatus further consists of a number of prisms of metal, supported by means of metallic springs, so regulated by movable weights as to insure the equidistant position of each prism from its neighbour. The current passing through the thin strip of metal passes through the lever, and the line of prisms to the terminal, without encountering [any sensible resistance. A second and more circuitous route is, however, provided between the lever and the terminal, consisting of a series of comparatively thin coils of wire of German silver or other resisting metal, connecting the alternate ends of each two adjoining springs, the first and last spring being also connected to the lever and terminal respectively. Suppose that the current intended to be passed through the instrument is capable of maintaining the sensitive strip at a temperature of say 60° C., and that a sudden increase of current takes place in consequence either of an augmentation of the supply of electricity or of a change in the extraneous resistance to be overcome, the result will be an augmentation of temperature, which will continue until a new equilibrium between the heat supplied and that lost by radiation is effected. If the strip is made of metal of high conductivity, such as copper or silver, and is rolled down to a thickness not exceeding 0·05 millim., its capacity for heat is exceedingly small, and its surface being relatively very great, the new equilibrium between the supply of heat and its loss by radiation is effected almost

instantaneously. But with the increase of temperature the position of the regulating lever is simultaneously affected, causing one or more contacts to be liberated, and as many additional resistance coils to be thrown into circuit: the result being that the temperature of the strip varies only between very narrow limits, and that the current itself is rendered very uniform, notwithstanding considerable variation in its force, or in the resistance of the lamp, or other extraneous resistance which it is intended to regulate. The resistance coils, by which adjoining contact springs are connected, may be readily changed, so as to suit particular cases; they are made by preference of naked wire, in order to expose the entire surface to the cooling action of the atmosphere. In dealing with feeble currents, Dr. Siemens uses another form of regulator, in which disks of carbon are substituted for the wire rheostat. The electrical resistance of carbon varies inversely with the pressure to which it is subjected. A steel wire of say 0·3 millim. diameter is drawn tight between the end of a bell-crank lever, and an adjusting screw, the pressure of the lever being resisted by a pile of carbon disks placed in a vertical glass tube. The current passing through the steel wire, through the bell-crank lever, and through the carbon disks, encounters the minimum resistance in the latter so long as the tension of the wire is at its maximnm; whereas the least increase in temperature of the steel wire by the passage of the current causes a decrease of pressure upon the pile of carbon disks, and an increase in their electrical resistance. The instrument first described may be adapted also for the measurement of powerful electric currents. The variable rheostat is in this case dispensed with, and the lever carries at its end a pencil pressing with its point upon a strip of paper drawn under it in a parallel direction with the lever by means of clockwork. A second fixed pencil draws a second or datum line upon the strip, so adjusted that the lines drawn by the two pencils coincide when no current is passing through the sensitive strip. The passage of a current through the strip immediately causes the pencil attached to the lever to move away from the datum line, and the distance between the two lines represents the temperature of the strip. This temperature depends, in the first place, upon the amount of current passing through the strip, and, in the second place, upon the loss of heat by radiation from the strip; which two quantities balance one another during any interval that the current remains constant. The thin sensitive conductor may be utilised either to restrict the amount of electricity flowing through a branch circuit, within certain narrow limits, or to produce a record of the amount of current passed through a circuit in any given time.

January 30.—" A Comparison of the Variations of the Diurnal Range of Magnetic Declination as recorded at the

Observatories of Kew and Trevandrum." By Balfour Stewart, F.R.S., Professor of Natural Philosophy in Owens College, Manchester, and Morisabro Hiraoka. In a previous paper by one of the authors a table is given exhibiting monthly means of the Kew diurnal declination-range, corresponding to forty-eight points in each year, or four for each month, that is to say, approximately one every week; and, in another paper, another similar table exhibits monthly means of the Trevandrum diurnal declination-range for weekly points. In the present paper these two tables are compared together. A comparison of the curves appears to lead to the following conclusions:—

(1.) Generally speaking, maximum points or risings in the one curve must be associated with maximum points or risings in the other, rather than with minimum points or depressions. Indeed, the researches of Broun and others, from a different point of view, strengthen this conclusion, which is, moreover, abundantly supported by a glance at the curves themselves;

(2.) The oscillations of the Trevandrum curve are greater than those of the Kew curve;

(3.) In many cases where there is a want of striking likeness between the oscillations of the two curves, there are yet noticeable traces in the one curve corresponding to the oscillations of the other. There are, however, a few cases where there is a want of apparent likeness.

(4.) In general, though not invariably, the oscillations of the Trevandrum curve follow rather than precede the corresponding oscillations of the Kew curve.

"On the Determination of the Rate of Vibration of Tuning Forks," by Herbert McLeod, F.C.S., and George Sydenham Clarke, Lieut. R.E. The paper commences with a description of the time-measurer adopted. The tuning-fork apparatus consists of a brass drum resting on friction wheels, and driven by a weight and train. Uniformity of motion being of great importance, an air-regulator, consisting of a fan enclosed in the lower compartment of a cylindrical box, is employed. Round one end of the drum are wrapped strips of paper on which white equidistant lines have been so ruled that they are parallel to the axis of the drum when the strips are in position. The strip most frequently used has 486 lines round the complete circumference of the drum. Opposite this graduated strip is placed a microscope with its axis horizontal. In the sub-stage is placed a 2″ objective, producing an image of the graduations at the focus of the object-glass of the instrument. At the common focus of the two lenses is placed the tuning-fork, the stem of which is held vertical in a vice. If when the fork is in vibration the drum is made to rotate with such a velocity that one of the graduations passes over the interval between two adjacent graduations in the time of one vibration of the fork, a stationary

wave is seen of length equal to the length of that interval. To determine the number of vibrations of the fork in a given time, it is only necessary, therefore, to be able to count the number of graduations which pass in that period. An electric counter gives the number of complete revolutions accomplished by the drum in any given period; and a fine-pointed tube, containing magenta, is carried by a saddle above the drum, and, being actuated by an electro-magnet, makes a dot on a piece of white paper wrapped round the drum at the beginning and end of the experiment. The distance apart of these dots gives the additional fraction of a revolution accomplished by the drum during the period of the experiment.

In illustration of the method a determination of one of Kœnig's forks was given:—

Duration of experiment, 5 minutes.
No. of 1st mark, 163. Number of line on circle, 301·0.
" 2nd " 164. " " " 199·5.
Temperature by thermometer, 16·8°.
Number of revolutions shown by counter, 158.
Number of lines on circle, 486.

During the experiment the wave had risen in the field of the microscope one-half a wave-length above the hair.

158×486=76,788 lines passed in 158 revolutions.
301 − 199·5=101·5 lines between two marks on drum.
101·5 − 0·5=101 lines corrected for movement of wave.
76,788+101=76,889 vibrations of fork in 5 minutes.

$\frac{76,889}{300}=256{\cdot}2966$ vibrations per second.

When this determination was made four other measurements were carried out, lasting for different intervals of time.

5 min.	... ...	256·297	at 16·8° C.
2 "	... ...	256·298	" 16·9
3 "	... ...	256·293	" 17·0
1 "	... ...	256·292	" 17·0
4 "	... ...	256·292	" 17·2
Means ...	...	256·294	16·98
Correction for error of therm.			− 1·4
			15·58

Clock was losing about 0·8 second per day. Correction for clock rate—

$$256{\cdot}294\times\frac{86,399{\cdot}2}{86,400}=256{\cdot}292.$$

The maximum difference between the above numbers is 0·006, or 0·00234 per cent.

As frequent bowing might affect the rate, two sets of measurements were made, in one of which the fork was bowed every 20 seconds, and in the other every 5 seconds.

Duration of Experiment.	Bowed every 20 seconds.	Bowed every 5 seconds.
5 min....	512·470	512·460
3 "	512·460	512·452
3 "	512·460	512·456
Means ...	512·463	512·456

No change of phase has ever been observed on the application of the bow. The same result was obtained by Lissajous, although Poske has since found that bowing produces an alteration. The authors have never seen any sudden jump of the wave during bowing. To test this in another manner two forks were arranged, one horizontally and the other vertically, so that both could be seen simultaneously in the microscope (the horizontal one being beyond the sub-stage). The Lissajous figure is seen at the angle made by the two forks. When bowed no change of the form of the figure took place; its amplitude merely was increased. This arrangement of the forks is suggested as a method for comparing two forks of nearly the same pitch, or of a fork with its octave. A difference of amplitude produced no appreciable change in the rate of the fork.

	Large Amplitude. 0·37 m.m.	Small Amplitude. 0·15 m.m.
	256·277	256·275
	256·273	256·274
Means ...	256·275	256·2745

The mode of fixing the fork in a vice might have changed the rate: to test this two sets of measurements were made, in one of which the fork was fixed in the vice, and in the other on a sounding-box.

	On Sounding-Box.
	512·489
	512·482
	512·477
Mean ...	512·483

This result may be compared with that above given in which the fork was bowed every 20 seconds. It will be seen that the rate is rather less when the fork is held in the vice. In the previous paper the coefficient of change of rate for temperature is given as 0·00011 for each degree centigrade: this number has been fully confirmed by numerous measurements.

A set of Kœnig's forks have been measured with the following results, when corrected to 15° C.:—

A 256 fork measured in May gave	256·309	mean of	19 expts.
Same fork in June and October ...	256·310	,,	43 ,,
Another 256 fork	256·306	,,	12 ,,
A 320 fork	320·372	,,	18 ,,
A 384 fork	384·437	,,	24 ,,
A 512 fork	512·451	,,	30 ,,

These forks would be correct at a temperature of about 25° C.

An old fork of Sir William Thomson's, made by Marloye, gave 255·253 instead of 256. This is the mean of ten measurements. This fork would he correct at about 13° C.

Mr. McLeod informs us that the original suggestion of these experiments was made by his colleague, Lieut. Clarke, and that they carried out the experiments together.

Physical Society, *Annual Meeting, February* 8. — Prof. W. G. Adams, President, in the chair.

The following gentlemen were elected as Council and Officers for the ensuing year:—President—Prof. W. G. Adams. Vice-Presidents—Prof. G. C. Foster, Prof. R. B. Clifton, Lord Rayleigh, Dr. Spottiswoode, Sir W. Thomson. Secretaries—Prof. A. W. Reinold and Mr. W. Chandler Roberts. Treasurer—Dr. E. Atkinson. Demonstrator—Prof. F. Guthrie. Other Members of Council.—Capt. W. de W. Abney, Dr. Warren de la Rue, Major E. R. Festing, Prof. Fuller, Dr. Huggins, Prof. A. B. W. Kennedy, Prof. McLeod, the Earl of Rosse, Mr. G. Johnstone Stoney, Dr. Wormell. Honorary Members—Prof. G. R. Kirchoff, Dr. J. Plateau.

Dr. O. J. Lodge read a short paper on a method of calculating the curve of temperature in a rod along which heat is being conducted.

Mr. Shoolbred gave an account of electric lighting, illustrated by diagrams of the most recent magneto and dynamo-electric machines and examples of the lamps in vogue. The only surviving magneto-machine is that of De Meriten's, which is incomparably superior to the older ones of Nollet and Holmes. The dynamo-electric machines described were the continuous-current machines of Siemens, Gramme, Wallace-Farmer, and the alternating-current machines of Wilde, Gramme, and Lontin. Wilde's machine is the first of these, or parent machine, and Lontin's so resembles it that the latter cannot be used in England. In these machines the current from a continuous machine is passed through a second machine, which yields the alternating currents. In Lontin's machine, also, a number of distinct currents are generated in separate circuits, each of which is capable of feeding several lights. There is now one in use on the Western Railway of France which gives three distinct currents,

each of which supplies four distinct lamps, making a total of twelve lights. The American Brush machine was also mentioned. The Dubosq lamp, which was the first regulator, is well adapted for laboratory purposes, but for practical purposes the Serrin is preferable. Rapieff's lamp is used in the *Times* office. The De Mersanne, which was highly spoken of at the Paris Exhibition, moves the carbons by bevelled gearings. The Wallace-Farmer lamp, though durable, is unsteady, perhaps because only inferior gas carbon has yet been used. Jablochkoff's candle was found to be defective from the solid insulator, such as plaster, used between the carbon. This made it very expensive also. Experiments in Paris had shown that whereas Jablochkoff's system cost 10d. per hour per light, the other systems cost only one half of that. In Wilde's candle the solid insulator was dispensed with, air taking its place, the arc always tending to keep at the top of the candle by electro-dynamic repulsion. In the De Meriten's candle three strips of carbon were used, the intermediate one being a stepping-stone to the arc which passes between the two outer ones. Werdermann's and Reynier's so-called incandescent lamps were also shown. Mr. Shoolbred, after alluding to the fact that the upper (positive) carbon takes a crater form, and hence becomes a reflector shedding the light downwards, stated that experiments had proved the line of maximum intensity of light to pass downward at an angle of 60° to the axis of the vertical carbons. By giving the positive carbon a horizontal displacement behind the lower negative one, Mr. Douglas, of the Trinity House, had been able to raise this line till it became horizontal, an advantage in lighthouses. He also pointed out that, whereas in Paris the Jablochkoff waxed for a period short compared to that in which it waned, in London it waxed for longer than it waned, which was of course an improvement; and Mr. Shoolbred suggested that it might be due to the fact that the engine worked at a speed nearer to that of the machine, and that the machine was founded more solidly in London than in Paris.

In the discussion which followed the reading of Mr. Shoolbred's paper,

Mr. Werdermann maintained that it was as easy to produce 500 lights as 10 from the electric light by sub-division, as he hoped soon to show, and stated that the size of the carbons greatly controlled the intensity of the light.

Prof. Ayrton held that the obstacle to the sub-division of the electric light was not an electrical one, but was due to the fact that the amount of light produced by the current is not in direct proportion to the amount of the heat produced.

Prof. Silvanus P. Thompson pointed out that residual magnetism in the cores of the bobbins of dynamo-electric machines lowered their efficiency, and hence short cores, as in the Wallace-Farmer machine, were an improvement.

NOTES.

Biology.

An anonymous writer in a contemporary, raising some interesting questions concerning the sting of the hive bee, declares the queen "stingless"—a novel doctrine.

The question of parthenogenesis among bees is still not absolutely decided, though the observations of Ziernon, confirmed by Sanson, decidedly support the affirmative view.

According to "Les Mondes" a young man died lately from having struck a match upon his finger-nail. A particle of phosphorus got under the nail, occasioning so virulent a burn that death ensued after twenty-seven hours.

It appears, from the "Royal Gazette" of British Guayana, that legislative measures have been taken to protect birds from the ravages of feather-hunters.

Prof. Belucci, in a carefully conducted series of experiments recorded in the "Gazzetta Chimica Italiana," has refuted Clermont's admission of the presence of peroxide of hydrogen in plants.

It is found that the human digestive organs are by no means able to extract all the nitrogenous compounds present in vegetable matters. Hence analytical results throw little light on their true nutritive value.

M. L. Couty has studied the physiological action of matê. He finds that its action is localised in the parts subservient to organic life, and especially the organs relatively least dependent on the nervous centres and specially on the brain. Upon the latter it has no apparent action.—*Comptes Rendus*, lxxxvii., p. 1091.

Dr. J. J. de Lanessan complains strongly of the neglected condition of French institutions for the study of the natural sciences. Thus at Dijon the Professor of Zoology is not even provided with a microscope.

M. Lacerda has laid before the Academy of Sciences certain results concerning the poison of serpents, as obtained from experiments on a *Crotalus* (species not named). He considers that the poison is a ferment, but not of the solid class, containing bodies resembling bacteria, and that it reproduces itself in the blood of animals which die from the bite of the serpent. As an antidote he recommends alcohol, both taken inwardly and injected

under the skin. M. Quatrefages, in presenting the memoir, expressed serious doubts as to the author's determinations.

M. P. Geddes has presented to the Academy of Sciences a memoir on the function of chlorophyll in the green *Planariæ*. These animals, on exposure to sunshine, decompose carbonic acid, and give off bubbles of a gas which was found on analysis to contain from 45 to 55 per cent of oxygen, the residue being nitrogen. A chemical examination of their bodies, after extraction of the chlorophyll, demonstrated the presence of a considerable quantity of ordinary starch.

It is not generally known that there exists a vegetable organism. *Hygrocoris arsenicus*, which is developed in arsenical solutions, It appears as an opalescent cloud suspended in the liquid, and if examined under the microscope appears as a glassy mass scattered over with brilliant points.

M. A. Milne-Edwards has laid before the Academy of Sciences a description of *Blythonomus giganteus*, an isopod measuring 0·23 metre in length and 0·10 in breadth, and which is distinguished from all other crustaceans by the peculiar arrangement of its respiratory apparatus.

M. B. Renault has described a new group of silicified fossil stems of the carboniferous epoch. He establishes the existence of a series of types parallel to that afforded by the Sigillarineæ, but which in certain structural details approach the *Cordaites*.

In a memoir presented to the Academy of Sciences, M. J. de Seynes refers the "chestnut-disease" now raging in the Cevennes to a mycelium analogous to *Zasmidium cellare*, which he finds on all the roots of the trees.

The naturalist E. Beccari has discovered, in the virgin forests of Sumatra, a flower which surpasses all others at once in size, beauty, and perfume. It belongs to the family of the *Amorphophili*, and has received the name of *Titanum*. The diameter of the flower is as much as 83 centimetres. According to "La Lancette Belge" six chests filled with the roots of this plant have just arrived at Genoa.

Prof. Haeckel remarks that the intellectual contrasts existing between the ants and their cattle, the Aphides, are certainly greater than the enormous difference which we recognise between the divine genius of a Goethe or a Shakspeare and the poor animal soul of an Australian negro.

According to Dr. Polli the human organism undergoes, in the course of its existence, a slow oxidation, on the completion of which death ensues. According to his calculation this should happen, accidents excepted, not earlier than the hundredth year. To prolong life a few grammes of a sulphite should be taken every morning in a glass of pure water.

"La Lancette Belge" calls attention to the existence of nu-

merous poisonous fishes on the African coasts, which it fears may be brought into European markets preserved by the application of artificial cold.

According to Prof. Haeckel no expression has given rise to so many errors and misunderstandings as the word "instinct."

Physics.

M. Sergius Kern sends us an account of a very interesting experiment by M. Slouginoff, which is likely to be fruitful in physical research. He takes the two electrodes of a battery, one of which is a thin, plain platinum plate, placed horizontally, and the other is a platinum wire, placed perpendicularly to the plate and nearly in contact with it. A small quantity of water, acidulated with sulphuric acid, is next poured on to the plate. If a current of 8 to 12 elements passes through the apparatus, and the wire is made the cathode, a spot of light is observed on its point. When using 15 elements the light appears, even if the direction of the current is changed. During these phenomena the water is only slightly decomposed, and the needle of a galvanometer, if introduced into the circuit, is only slightly deflected. It was also remarked that the surface of the water under the wire is lowered, forming the shape of a cup. The bubbles of gas arising from the decomposition of the water, in this case, travel constantly round the wire, forming a very pretty figure of 8. This is caused by the movements of the surface of the water, it being alternately repelled or attracted to the wire carrying the current. The gaseous bubbles obey these movements. It was further discovered that during these actions in the apparatus the current was intermittent. It is well known that the mechanical movements in iron or steel bars, during their magnetising by discontinuous currents, may be used as a source of sound. In employing a current of 12 elements, and introducing into it Slouginoff's apparatus, a distinct sound is remarked. If the platinum plate is well polished the water is repelled from the point of the wire equally in all directions and some millimetres from it. The current is thereby interrupted, and the liquid advances to the wire; in this case the liquor will be again repulsed, and so on. Taking advantage of these actions of the current, the apparatus may be employed as a very simple form of an electrical interrupter.

At the Technical Society of St. Petersburg M. Latchinoff recently delivered a lecture on the electric light. He made some experiments with Jablochkoff's condensators, which consist of a set of tin plates placed one on another; the surface of every plate is 0·7 square metre. Between every pair of such plates a piece of silk covered with varnish is introduced. The height of the condensator is about 6 feet. On introducing two condensators into a circuit the intensity of the electric light is doubled.

The lecturer believes the new system of electric lighting devised by M. Rapieff to be a serious opponent of Jablochkoff's process. The chief advantage of the new system is that the luminating point does not change its position, and therefore this system is more suitable for the projection of the electric light at a distance. This advantage will give increase to the use of the electric light for military purposes.

At a recent meeting of the French Academy of Sciences a paper by M. Henri Becquerel, on the "Temporary Magnetic Proportions developed by Induction in certain Specimens of Nickel and Cobalt," was read. The specimens of iron used by M. Becquerel were taken from the same piece of Swedish soft bar iron, of a high degree of purity. By submitting bars of iron and nickel, of the same dimensions and shape, to gradually increasing magnetic influence, it was found that the ratio between the coercitive force acquired by the iron and the nickel is a quantity varying with the intensity of the magnetic force used. This ratio begins by being a minimum of about 0·4; it then increases to 0·75, and again decreases to a still lower minimum of about 0·2. A nickel bar becomes saturated much more quickly than a similar iron bar; the magnetism of the former remains stationary, while that of the latter goes on increasing. Similar results were obtained with a bar of nickel, which was allowed to oscillate over the poles of a powerful magnet. The experiments with the bars of cobalt were similar in their results.

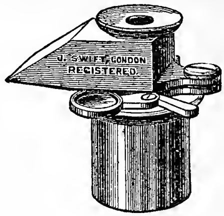
At the same meeting M. Héraud sent in a paper "On a New Voltaic Pile," in which the exciting liquid is a saturated solution of sal-ammoniac, to which one-tenth of liquor ammonia has been added, the depolarising substance being mercuric proto-chloride. With a closed circuit the ammonic chloride is decomposed, the chlorine going to the zinc and the ammonia splitting into ammonia and hydrogen, which reduces the mercury salt to the metallic state, ammonic chloride being once more formed. Each cell is hermetically sealed. After being in action for 227 days, a pile of 9 elements gave an electromotive force equal to 0·73 per cent of the original strength, and at the end of 984 days 0·50 per cent.

Four kinds of camera lucida were brought before the Royal Microscopical Society at their December meeting. Dr. Hoffmann's instrument takes the place of the usual eyepiece. The image of the object is viewed by two reflections; the first by a plate of silvered glass; the second by a plate of transparent glass, through which the paper and pencil are viewed directly. The instrument has the disadvantage of interfering with the employment of the ordinary eyepiece, which has to be removed before the camera can be used, and only permits drawings to be made with the microscope in either vertical or horizontal positions.

A somewhat complex arrangement of prisms for the same purpose, by M. Pellerin, was also described, but no drawing is

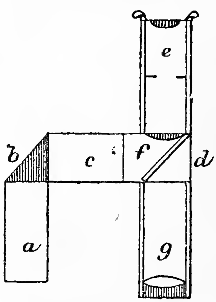
given, and indeed the author's original paper is without illustrations.

Mr. Swift's apparatus is a modification of a well-known form by Nachet. The object is viewed in the usual way, through the eyepiece; the projecting box attached to the cap contains a prism, which reflects the image of the pencil and paper to an inclined tinted plate, where a second reflection directs the image



upwards and renders it visible with the object. This instrument has the advantage of permitting the microscope to be used in any position. A second disc of neutral-tint glass can be interposed when the light from the object requires to be still more subdued to render the pencil point visible.

In Dr. Russell's instrument a tube (*a*) containing a right-angle prism (*b*) fits into the microscope. The image is reflected to a tinted plate (*f*), which again reflects it through the eyepiece (*e*). The tube (*d*) is prolonged downwards, and carries a telescopic

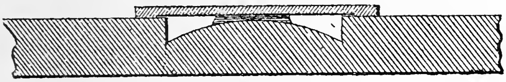


object-glass (*g*), which forms an image of the paper and pencil combined with that from the microscope. The image of the paper requires inversion, either by a reflecting prism (*g*) or suitable lenses placed below the object-glass.

Dr. Hudson, in the discussion which followed the reading of the paper, considered ruled squares placed in the focus of the eyepiece, the contents of which were copied on paper with similar ruling, superior in simplicity and ease of working to any form of camera lucida, especially in the rapid drawing required when living objects were examined.

Mr. Weber, by a simple variation of the hollow slide, has

greatly increased its utility. The cavity, instead of being concave as usual, is ground out, as shown in the cut, having the greatest depth at the edges, the bottom of the cell being convex.



A drop of water is held by capillary attraction between ths cover and the projection; the cover may be fastened or not, at pleasure.

A writer to "Science Gossip" describes a very simple live box, constructed of two ordinary 3×1 slides, placed between two easily fitting flattened brass bands. The slides are kept apart by an india-rubber ring of any convenient thickness, and the whole contrivance kept tight by means of wedges. The contrivance is readily constructed, and applicable to many purposes where a thin glass in front is not required.

The inhabitants of Heilbronn, the native town of the late Dr. Julius Robert Mayer, so renowned for his contributions to the Mechanical Theory of Heat, have resolved to erect a suitable memorial on the spot where he lived, laboured, and died. The following gentlemen have agreed to form an English Mayer-Memorial Committee :—William Spottiswoode, Esq., Pres. R.S.; Sir Joseph Hooker, K.C.S.I., V.-P.R.S.; Prof. Stokes, Sec. R.S.; Prof. Huxley, Sec. R.S.; Prof. Tyndall, F.R.S.; Dr. William Siemens, F.R.S.; Herbert Spencer, Esq.; Sir William Gull, Bart., F.R.S.; Sir James Paget, Bart., F.R.S.; Sir John Lubbock, Bart., F.R.S.; Prof. Henry Smith, F.R.S.; Dr. Debus, F.R.S.; George Busk, Esq., F.R.S.; Dr. Hirst, F.R.S.; Prof. Frankland, F.R.S.; Prof. Dewar, F.R.S.; John F. Moulton, Esq., M.A.; Frederick Pollock, Esq., M.A. Subscriptions exceeding one pound may be sent by cheque to Messrs. Robarts, Lubbock, and Co., 15, Lombard Street. Smaller sums may be sent by post-office order to the Honorary Secretary, T. Archer Hirst, Royal Naval College, Greenwich.

Chemistry and Technology.

The substance of which the much-talked-of Swiss "self-luminous clock-dials" are composed has, according to "The Engineering and Mining Journal," been examined by Prof. Henry Morton, of the Stevens Institute of Technology, Hoboken, and has been ascertained to be the "sulphide of calcium," which is caused to adhere to the dial surface by means of some resinous varnish. This compound has long been known to possess phosphorescent qualities of a high order; but as prepared for this special use its phosphorescence has never before been equalled,

as may be judged from the fact that, after being shut up in a box for five days, it was still visible in total darkness. Prof. Morton expresses the opinion that further advances in this direction may develop discoveries of most surprising character.

We gather from the same journal that Mathey, a Neufchatel chemist, communicates the following facts relating to the same subject. The phosphorescent dials, he states, are usually made of paper or thin card-board, enamelled and covered with an adhesive varnish, upon which is dusted, with a fine sieve, powdered sulphide of barium. The sulphides of strontium and calcium possess the same property; but our authority affirms they lose their phosphorescence more quickly than the barium salt. When the dial has lost its self-luminous qualities they may be restored by an hour's exposure to sunlight, or by burning near it a few inches of magnesium wire.

In a paper presented to the Academy of Sciences, on the "Classification of Colours and the Means of Re-producing Coloured Appearances by Three Special Photographic Proofs," M. C. Gros distinguishes two categories comprised under the word "colours"—lights and pigments. The elementary lighis which by their mixture produce all kinds of shades are the green, violet, and orange rays. The elementary pigments which by their mixture produce all kinds of shades are red, yellow, and blue. To obtain directly the elementary tints of rays and of pigments it is sufficient to look through a prism at a white stripe upon a black ground, and at a black stripe upon a white ground. In the first case an orange, green, and violet spectrum is seen; and in the latter case a blue, red, and yellow spectrum. In the former case the orange, green, and violet are elementary rays; and in the latter the red, blue, and yellow are rays combined two and two. The author describes an apparatus which he names the chromometer, and by means of which he produces the photographic effect above mentioned.

A daily paper, speaking of the prizes at the Paris Lottery, uses "carbide of natrium" as a synonym for carbonate of soda. Another newspaper paragraph describes the death of a man who fell into a cistern of "caustic and potash."

According to the "Revue Britannique" ether is consumed as an intoxicant by ladies of rank in England, the grass in Hyde Park being strewed with empty bottles flung from carriage windows.

According to M. Galippe, human hair cut during life has a special odour which remains after the action of potassa or of any other reagent. Hairs which fall off naturally are inodorous, dull, not silky to the touch, and present a special phenomenon of alternating colouration. The hairs of the Chinese, even after the action of potassa, possess an odour of musk, which is intensified at higher temperatures; their section is not round or oval,

but polyhedric, and though black they appear red by transmitted light. The hairs of certain persons retain a peculiar electric condition: if the hand is approached to a bunch which contains some hairs of this kind, they will separate themselves from the remainder and follow the hand.

A curious toxicological case is reported from Hamburg in the "Chemiker Zeitung." The body of a man who died in 1867 was taken up for examination. It was thought necessary to determine arsenic not merely in the corpse in question, but in the soil of the churchyard at different distances from the coffin, and also in the body of another man who had been subsequently buried in the same grave. This latter body was perfectly free from arsenic, which, however, was found in the first corpse in amply fatal quantity (0·24 grm.), whilst in the lid of the coffin and in the adjacent soil very minute quantities were traced. Hence the conclusion was fairly drawn that the man in question had been poisoned with arsenic, and that a portion of the poison had been gradually transferred from his body to the wood of the coffin and the adjacent soil.

In a paper on Poisoning by Mushrooms, in the "Moniteur Scientifique," Mr. J. A. Palmer says that mushrooms may act as a poison in three different manners. They may act as an indigestible matter, which is the case with hard coriaceous species, and may even occur with the edible mushroom when decomposing, as it gives off sulphuretted hydrogen in quantity sufficient to cause vomiting. Or, again, they may be gelatinous or acrid. Many *Boleti*, otherwise innocent, are too gluey to serve as food. Lastly, mushrooms may contain a subtle alkaloid, devoid of smell and taste, as happens in the group of the Amanitæ. This compound is known by the name of Amanitin, and to it the fatal cases of mushroom-poisoning are mostly due. No remedy has yet been found. No immediate effects are produced by this poison; but after eight to fifteen hours the patient experiences stupefaction, nausea, and diarrhœa, followed by delirium and death. Mushrooms containing this poison seem able to communicate it to wholesome species by contact, and it may also be absorbed through the skin. The author was on one occasion seized with alarming symptoms after carrying in his hand some Amanitæ wrapped in paper.

Engineering, Metallurgy, Mineralogy, and Mining.

According to the "Colonies and India" a valuable deposit of graphite, of excellent quality, has recently been made in the province of Wellington, New Zealand.

M. F. Pisani has communicated to the Academy of Sciences the fact that Wagnerite—a fluoriferous phosphate of magnesia, originally met with at Werfen, in Salzburg—is identical with a

mineral subsequently discovered at Bamle, in Norway, and provisionally named Kjerulfine. The latter, however, contains a percentage of lime. M. F. Pisani also finds that the Russian retinite was formerly mistaken for a manganiferous garnet.

A new meteoric mineral, named Daubréelite, has been recently described by Prof. J. Lawrence Smith. In a state of purity it consists of shining black fragments of more or less foliaceous structure, somewhat resembling molybdenite. It is not in the slightest degree attacked by hydrochloric acid, cold or hot, but dissolves slowly in warm nitric acid, without depositing sulphur. Its specific gravity = 5·01; its composition being—sulphur, 42·69; chromium, 35·91; iron, 20·10.

The fifth Report of Progress by the Secretary for Mines of the Geological Survey of Victoria discloses no very salient facts, either in the department of economic geology or of palæontology. An examination has been made of certain elevated basaltic plateaux known as the Bogong and Dargo High Plains, situate on the eastern boundary of the colony—a well-watered district, but from its great elevation subject to frosts even in suumer. The region is auriferous, but the distribution of the precious metal is very unequal. Mr. Krause's Report of the Wombat Hill District shows that the auriferous leads are very rich. From 1 mile of this lead 170,000 ozs. of gold have been obtained, equal to a yield of 32 ozs. of gold per lineal foot. In the deep quartz mines of Stawell there is no falling off in gold on descending lower—a fact of considerable scientific interest. The chemist to the Survey, Mr. J. C. Newberry, B.Sc., has discovered phosphates widely distributed in Gippsland, not apparently so concentrated as to furnish an article of commerce, but sufficiently plentiful to impart great fertility to the soil. Some yellowish grey phosphatic soils, hitherto neglected, have been cultivated on Mr. Newberry's recommendation, and have given excellent results.

THE MONTHLY

JOURNAL OF SCIENCE.

APRIL, 1879.

I. IS NATURE PERFECT?

We have often thought it might be useful to exhibit, in all their well-marked contrast with the results of modern Science, those views of Nature which still prevail even among the cultivated classes, and which are very slowly retiring from the fields of literature. For this purpose let us take a man,—such as may be found in abundance in the middle and upper ranks of society,—well-educated, heedful, thoughtful, and refined, but not trained as an observer, and having no special acquaintance with recent discoveries in natural history. Let us give him a holiday, and send him for a summer ramble in the New Forest, in "merrie Sherwoode," or among the ferny coombs of Devon, and let us try the while to read his musings. Even his very first expression of feeling, the sigh of relief on finding himself rid of city-bustle, worry, and noise, is mainly the outcome of an illusion. He fancies himself in a sphere where boundless resources are dispensed with a liberality equally boundless. The heavens are full of light and warmth, and the earth is clad in rich and varied hues. Perfume breathes from every spray. On all sides is life, animal and vegetable, unworn by toil and unshadowed by care and anxiety. To the butterfly hovering over the blossoms, to the blackbird warbling on the spray, the world seems not as to man, the task-yard of a workhouse, but the banqueting-hall of a palace. The observer, even whilst he envies the insects and the flowers, who "toil not, neither do they spin," feels soothed and refreshed by the mere reflection of their supposed felicity.

Plenty requires peace as its natural complement, and our wanderer believes that he finds this boon also in the wood-

lands and the heaths. Indeed, how should it be otherwise? Where there is superabundant plenty, where every demand is more than satisfied, how should there be the thrusting, and pushing, and jostling, outward visible signs of that internecine war of each against all, and of all against each, which the "friends of peace" worship under the name of competition? The observer cannot, indeed, forget that in his realms of concord and repose, pain and death are present. He knows that at any given moment hundreds of flies must be struggling in the snares of spiders; that caterpillars innumerable are being gradually devoured by internal parasites; that many a song-bird is falling a prey to the hawk or the weasel. But, as was the case half a century ago even with eminent naturalists, he scarcely apprehends the full meaning of all this suffering and massacre. Such facts as we have just enumerated seem to him mere "rude exceptions to the general joy," departures from the order of Nature, casual, even though numerous, rather than as they really are, part and parcel of its very essence. Our friend in his ramble, and in his necessarily hasty survey, fails to perceive that not only does one-half the animal kingdom live only in virtue of the death of the other half, but that the herbivorous creature is as much a life-destroyer as the beast of prey, extirpating other animals by depriving them of food, and plants by consuming their seed or their seedlings. He overlooks the silent, quiet, but not the less deadly war waged by plants among themselves, each seeking to monopolise to itself soil, air, and light, and to crowd out, starve, or smother its competitors. In short, in his optimist contemplations, he entirely forgets that struggle for existence which—whether or not we regard it as a main factor in the development of animal and vegetable forms—we are bound to accept as perhaps the greatest, and assuredly the saddest, feature of the organic world. Who, after reading the third chapter of the "Origin of Species," can fail to be reminded of those words of St. Paul "For we know that the whole creation groaneth and travaileth in pain together until now;" * or of the sadder exclamation of one who, having no faith in the ultimate solution of this dark riddle, cries out in agony "Creation is murder!" †

But not only is strife rather than peace the order of the organic world,—strife so thoroughgoing and so widespread that it rages even among spermatozooids; not only do the

* Romans, viii. 22.

† Winwood Reade, Martyrdom of Man.

majority of seeds and ova, from one or other cause, fail to be developed; not only does every species press hard on its means of subsistence, or on the space where alone its being is possible. Even in matters where life is not directly concerned we find Dame Nature not lavish, not liberal, but more penurious than the wife of the thriftiest peasant-proprietor in rural France. Those colours which so fascinate the poet or the artist, and which seem to be spread in such royal lavishment over copse and meadow and heath, have all their purpose to fulfil; they have to serve as an attraction to insects which effect the fertilisation of the flower. The beauty and the odour which we so much admire appear only when this task is necessary, and when it is accomplished they are again withdrawn, just as at a banquet the lights are quenched and the decorations taken down when the guests have departed.

To a sensitive mind it must be saddening to find that the woods, the fields, and the solitudes offer no soothing contrast to the exchange, the workshop, or the battle-field, and that on earth peace, repose, and harmony exist nowhere. But it is the duty of Science to "perceive and declare" whether the facts and the laws recognised be joyous or grievous.

In one sense, indeed, Nature may be called lavish. But it is an unkindly prodigality. She is reckless of life; reckless and wasteful, too, of heat, the prime condition of organic existence. Passing over the fact that the bulk of the solar radiations travel out into the desert depths of space, while an infinitesimal portion alone falls upon any of the planets, very much of the heat which reaches our earth, at least, is radiated off again during the night. Carbonic acid gas, indeed, possesses the precious attribute of admitting the sun's rays freely, and of being at the same time almost impervious to heat-rays of low tension, such as those given off by the earth. But this gas forms but a very small proportion of our atmosphere, and could not be sensibly increased on account of its injurious action upon higher animal life. But if the non-poisonous gases oxygen and nitrogen had the same power as regards the radiation of heat, the climate of the world would be much improved, and spring frosts—the bane of the farmer and the gardener—would be rendered impossible. It is of course conceivable that some cause may exist which renders it impossible for oxygen and nitrogen to possess this attribute without forfeiting their characteristics in other respects.

There is another feature which, outside of scientific circles, we hear commonly ascribed both to the individual

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animal or plant, to the fauna and flora of any given country, and to the animal and vegetable kingdoms in their entirety. We refer to the attribute of "perfection." We must confess ourselves utterly at a loss to know how this notion has been reached. We have asked believers in this doctrine to tell us by what marks this perfection is to be recognised. We have invited them to take up a plant or an animal, and to demonstrate that any departure from or addition to its present standard, whether structurally or functionally, would be injurious. But the only answer we have received has been a cloud of generalities. The very idea of perfection seems to us unthinkable in reference to a crowd of species engaged, as all Nature is, in mutual conflict. It is by dint of the imperfections of the Carnivora in speed, strength, or cunning that their prey—say the deer or antelope—escapes. It is in virtue of the imperfections of the latter animals that they are captured by the wolf or the leopard. Again, were all animals and vegetables perfect in themselves and in reference to their surroundings, we may ask how it comes that so many species have been exterminated, and that others are even now in course of extermination? It may be contended that the surroundings have altered. This, then, is an admission that the adaptation to circumstances is not always perfect. But further, some other species, or at least groups, coeval with such as have disappeared, are still found surviving. Here we have consequently the following riddle to solve:—Two groups of "perfect" animals, each in "perfect" harmony with its surroundings, are given. These circumstances being altered, the one group is no longer in harmony, and consequently perishes, while the other remains equally well adapted to a different set of conditions, and survives! Yet more; admitting the perfection dogma, we must suppose the fauna and flora of any region—say New Zealand—better adapted to its soil and climate, and to all other local conditions, than any strange animal or plant can be. On introducing such strange species we should therefore see them placed at a disadvantage, and without constant human aid and supervision prove unable to exist at all. But in reality the very reverse is the case; the new comers are not only found able to exist independently of man's assistance, but to spread in opposition to his most strenuous efforts, and even to crowd out the natives. The notion, therefore, that every local fauna and flora forms a perfect whole, perfectly adapted to the circumstances in which it is placed, must be given up as a most glaring error.

Again, we often see large groups of closely-allied species, differing but little from each other, inhabiting the same country, dependent upon the same kind of food, and exposed to the same enemies. Thus there are in Britain alone fifty-seven species of the small dung-feeding beetles included under the genus *Aphodius*. Some of these are exceedingly abundant, others comparatively rare. If all these species are perfect, and perfectly adapted to their environments, why should some be so much more plentiful than others? But, descending more closely to particulars, we may show that in animals, as in man himself, there are certain *desiderata*—wants which Nature has left unsupplied. How exceedingly uncomfortable should we, for instance, feel if we were suddenly deprived of the power which we now enjoy of excluding the light from our eyes when we think proper! Yet as regards the sense of hearing we labour under a similar deficiency; it might rather be said under a greater, since to all persons who have occasion to concentrate their thoughts upon some given subject noise is a far greater nuisance than light can ever be to a healthy man. Surely, then, our inability to render ourselves temporarily and voluntarily deaf is a proof that we, in one respect at least, fall short of perfection.

We may take another instance: what a great addition would it be to man's comfort if he were personally offensive to all insects of the Dipterous order, so that they would keep aloof from him in disgust! When we consider that the mosquito, in addition to the positive irritation, annoyance, and want of sleep which its attacks occasion, is now proved to be an agent in the spread of leprosy,—when we remember that the common house-fly is a propagator at least of ophthalmia, and probably of all zymotic disease,—we surely cannot dispute that such a change, either in the nature of our cutaneous emanations or in the tastes of these pests, would be an incalculable boon, lacking which we cannot proclaim ourselves physically perfect.

Did we know the necessities of other animals as well as we know our own, we might doubtless find in like manner defects on all hands. But we have surely said enough to lead the advocates of organic perfection—who often find in this dogma an *à priori* objection against Evolution—to pause and reconsider the evidence upon which it is based.

The last of the popular assumptions concerning Organic Nature which we can here notice is the old, but still rampant notion that every plant, every animal, exists with reference to man, and for his convenience. "What use is

such or such a creature ?" is a question too commonly asked —and not by children only—of the zoologist or the botanist, When we reply that to the best of our belief and knowledge the animal or plant in question is of no use, but simply an unmitigated nuisance, the most charitable construction put upon our answer is that we are too proud to confess our ignorance. Like the doctrine of organic perfection, this dogma is not easily traceable to any substantial basis, but, having once become a current article of popular faith, its extirpation is proving a difficult task. To us it appears, in its very essence, irreverent. If the Creator had, *e.g.*, made the mosquito, or the guinea-worm, or the *Lucilia hominovora* to be of service to man, we may depend upon it that they would not have been sources of annoyance. Even a human invention, the produce of exceedingly finite reason, is condemned if, along with certain and even great good, it effects abundant mischief. How much more if the good be problematic, and the evil open and palpable? Shall we, then, adduce what is notoriously defective as an instance of Divine "contrivance"? One thing we may certainly conclude, viz., that if a maximum of earthly enjoyment and the minimisation of earthly suffering had been the objects of the Creator the world would have assuredly been constituted very differently from what it is. When we know what His objects really were it will be quite time enough to indulge in teleology and to indite "Bridgewater Treatises."

II. THOUGHTS ON OUR CONCEPTIONS OF PHYSICAL LAW.*

By Prof. Francis E. Nipher, St. Louis, Mo.

IN the short time at my disposal I wish to point out some reasons for the more general cultivation of a certain cardinal virtue which is so rare that I fear it has no name. Perhaps the words *Intellectual Modesty* would come as near as any others in expressing what I mean. The

* Abstract of an Address before the Alumni of the State University of Iowa, June 19, 1878; delivered at Kansas City, December 23, 1878, and which appears in the "Kansas City Review."

world is very full of people who are ready to make assertions upon subjects which are evidently too difficult for them—in many cases too difficult for any one—to handle with any degree of certainty; and it doubtless often happens that some who have meditated studiously for years upon some such subject, arriving at no satisfactory conclusion, are regarded as objects of profound commiseration by others, who rush upon conclusions like the unthinking horse into the battle. It is as natural that people should thus differ as that some should have darker skins, taller frames, or more irritable tempers than others. To what extent these, and other differences which we shall point out, are blameworthy, we cannot attempt to discuss, but shall study the mental habits of men in precisely the same spirit in which we would study the habits of other animals. But I wish to show some tangible reasons for thinking that there are very few subjects upon which we can dogmatise, and that in any case it is unnecessary. I wish to advocate the cultivation of intellectual modesty, not merely because it seems to me to be one of the brightest ornaments of the human mind, but because of its vital connection with another cardinal virtue—intellectual honesty.

Perhaps this end will be best attained by considering the difficulties which are met in the investigation of any subject, when the sole aim is to find out the truth of the matter, and I have thought it proper to point out some of the difficulties to which we are subject in arriving at our conceptions of physical law.

The study of physical science has endowed the human mind with an attribute which is usually ascribed to and is thought to be characteristic of the Divine Mind. I refer to the power of prophecy. The astronomer can predict the position of the planets for generations to come, basing his predictions on the assumption—an unproved assumption—that only those causes which he has considered will act in the future, or, in other words, that the present order of things will continue. His power of prediction does not, however, extend indefinitely into the future, for there are doubtless many minor disturbances or perturbations, too small to be detected by the instruments which he can command, without many centuries of observation, the effects of which will become plainly apparent after the lapse of ages, —that is to say, his power of prophecy is limited by his ignorance of certain facts, and possibly by his inability to solve the equations involved in a complete discussion of the subject.

It is for precisely the same reason that we cannot foretell the future destiny of every person in the world. But to a mind possessing all knowledge, and of infinite power, the one problem would evidently be as simple as the other.

Not only are we unable to predict for an infinite future, on account of the summing up of disturbances which cannot be detected in a short time, with our means of investigation, but, as before suggested, events wholly unexpected to our partially instructed minds—apparent breaches of continuity—are liable to happen at any time.

"A great explosion on the sun may scorch us into cinders in a second. The earth may be dashed to pieces and dissipated into gas, by collision with some immense meteorite. We may become involved in a nebulous atmosphere of combustible gas, which would ignite a moment later; in fact, as was so eloquently pointed out by Mr. Babbage, there is no catastrophe too great or too sudden to be consistent with the reign of law and the continuity of action."

In the discussion of physical phenomena we always ignore the greater part of the discussion, by neglecting those elements which are, or are supposed to be, unimportant. In so simple an operation as the weighing of a quantity of matter on a steelyard, we can discuss only the merest elements of the case. The student of physics would tell you that the weights are inversely as the lever arms, but this is far from being the whole story. During the weighing, certain parts of the steel bar are heated; other parts are cooled; still other parts retain their temperature unchanged; electrical currents are set up within its mass; its magnetism is changed; its torsion and elasticity become different: in fact, to discuss all the changes occurring within the bar during so simple an operation would infinitely transcend the power of the most gifted men.

If we could discuss completely the laws which govern phenomena we should find them represented, in many cases, not by the comparatively simple formulæ which have been found sufficient for practical purposes, but by infinite series, the first terms only of which our mathematicians have been able to deduce, and our physicists to experimentally detect.

In the study of Physics our most certain experimental results force us to ideas equally beyond our power of realisation. It is shown, beyond question, that light moves over a distance of about seven times the circumference of our earth in a single second. We must look for something marvellous in any theory which can account for so marvellous a fact. According to Newton's theory, we should have

particles of light shooting off from a distant luminous body with this immense velocity, and, falling upon a mirror, their motion would not merely be checked, but the elasticity of these light particles must be assumed to be so perfect that they rebound with an equal velocity.

According to the undulatory theory, the light consists of vibrations of a medium which fills all space. Since the velocity of transmission of these vibrations is so great it follows that the elasticity of this medium must be 10,000,000,000 times as great as that of the hardest steel. Space is not now regarded as a void, but is filled with a medium which, as Thomas Young remarked, "is not only highly elastic, but absolutely solid." And yet, as we walk through space, the solid atoms which compose our bodies experience not the slightest resistance. Such ideas, although they can be conceived, cannot be realised. We have had no previous experience with materials possessing such properties, and such ideas must necessarily appear strange to us; but they are no more strange than the phenomena of light which we directly observe, and which force us to this or to some other theory equally marvellous. Only those who have carefully examined the subject can realise how weighty is the evidence in favour of the undulatory theory of light; but where such stupendous conceptions are involved, a slavish acceptance of any theory, even by them, would be in the highest degree objectionable. We are not the friends of theories, but of truth.

It is not surprising, then, that in the progress of our sciences many errors of reasoning and in the interpretation of facts have been committed. You are all familiar with the ideas of Newton in regard to the nature of light, ideas which were not in themselves absurd, which were firmly believed in by this man of such transcendent power, but which were clearly negatived by results of subsequent experiment.

Lavoisier's idea that all acids were compounds of oxygen received a complete refutation when the constitution of prussic and muriatic acids became known. In fact the errors of scientific men are well-nigh innumerable, not because they are men of science, but because they are *men*, and we are probably justified in saying, quite in general, that if the man who never committed a mental blunder be found, we shall also find a man who never conceived a vigorous thought. The fact that the results of scientific men can usually be checked by observation and experiment perhaps diminishes their liability to err, and enables them

to discover multitudes of errors that would otherwise escape their attention. This does not tend to make the results of their investigations less weighty than results which have been reached by other processes more purely mental. If men of science, with their severe methods of research, their habits of testing their conclusions by observation and experiment, are nevertheless led into wrong conclusions, what does it prove? Simply that the human mind, even under the most favourable circumstances, is fallible! Is there a class of men less liable to make mistakes? It is precisely this experience which causes many to place a small value upon the unsupported assertions and speculations of any man, however honest, earnest, or able he may be.

On this point one of the most admirable of experimenters, Faraday, has beautifully said—"The world little knows how many of the thoughts and theories which have passed through the mind of the scientific investigator have been crushed in silence and secrecy by his own severe criticism and adverse examination; that in the most successful instances not a tenth of the suggestions, the hopes, the wishes, the preliminary conclusions, have been realised."

In the 24th series of his "Experimental Researches" Faraday describes many tedious and intricate experiments, in which he tried to connect gravitation and electricity. "He laboured with characteristic energy for days, on the clock-tower of the Houses of Parliament and in the shot-tower of Southwark, raising and lowering heavy weights connected with wire coils. Many times his great skill as an experimenter prevented him from being deceived by results which others would have regarded as conclusive proofs of his idea, and when the whole was done there remained absolutely no result." For although the results were wholly negative, Faraday could never accept them as conclusive against his idea, to which he had been led by his experiments on the relations between electricity and magnetism. His mental condition after this work was done is best described in his own words:—"Occasionally, and frequently, the exercise of the judgment ought to end in *absolute reservation*. It may be very distasteful, and great fatigue to suspend a conclusion; but as we are not infallible, so we ought to be cautious."

It is a matter of common observation that men who, like Faraday, have done much to widen the boundaries of our knowledge, are precisely the ones who are most frequently in a state of doubt, while those who have received all their knowledge at second hand are generally more ready with a

positive decision and a reason for it, not necessarily because their intellectual integrity is less, but because they *cannot* realise how vain a thing the human reason is. To imagination and reason, controlled and checked by experiment and observation, are we to look as the source of the greatest advancement in science; but we are not to look for infallibility, and in cases where the reason alone is allowed to decide, where observation and experiment are impossible, the uncertainty must necessarily be greater. In many cases the fact that the subject is so intrinsically difficult that no experimental check is possible, appears to inspire the investigator with a confidence in his conclusions that could hardly be reinforced by absolute certainty.

I have confined myself to the errors which scientific men have committed, and to which they are liable in their search for truth, not because they alone are liable to err, but because a discussion of the multitude of errors into which intellectual men of other professions have fallen would be sure to give offence. But it is not the scientific mind which stands impeached—it is the *human* mind!

To what end have we then come? It appears that all scientific results are attended with some uncertainty. Sometimes the uncertainty is very small, and we are able to obtain a numerical estimate of it. In other cases it may be possible that a fundamental misconception of the truth may have been formed. As an instance illustrating what I mean we may cite the case of the measurement of the Atlanta base-line by the engineers of the United States Coast Survey. The whole length of the base-line was nearly 6 miles, and three determinations of its length showed differences of about 3-10ths of an inch—about a millionth of the entire length. It is safe to say that if these re-measurements had shown differences of 10 feet there would have been no quarrelling in regard to which measurement was right, but all would have been rejected; and if the engineers were not dismissed as incompetent, they would, with feelings of mortification, have begun their work over again. It is curious to observe that in many cases where less skilful men attack problems infinitely more complex, reaching conclusions differing as widely as the poles, we have, instead of conscientious re-investigation or a modest reservation of judgment, dogmatic discussions, empty words. In the other case, where the error is likely to be a fundamental one, the probability of the truth or error of a conclusion cannot always be determined numerically, and will vary greatly in different minds.

In conclusion, allow me to say that it seems quite probable that human welfare does not require us to sit in judgment on the ideas of others. Thoughtful men are becoming more and more impressed with the vastness of the unknown and the comparative insignificance of human achievement, while the demonstrated fallibility of human reason leads them to temperance and modesty of thought and expression: to *appreciation*, as well as toleration, of opposition and doubt. Certain it is, that if we preserve our intellectual integrity we shall be unable to settle many of the problems that interest us most. If we decide upon some of them, and other persons still reserve their judgment or decide differently, we need not lose our tempers; they have not only decided differently from us, but we have also decided differently from them. It is impoıtant to notice that neither of these decisions has affected the *truth* in the least. If we feel called upon to defend the truth, we are, after all, only defending what we *believe* to be truth, and possibly against men as honest and as able as ourselves. But why should we defend the truth? So long as the heart of humanity shall pulsate, will not truth be cherished there? Why would it not be far better for each one to put himself in the attitude of a reverent *searcher* for truth? remembering always that the little decisions that we may reach are possibly wrong, that all of the honesty and ability in the world is not concentrated within ourselves and the comparatively few who think as we do, and that one can do nothing nobler than to make himself as intelligent and humane as possible, resolutely following out his highest convictions, and living at peace with himself and with all men.

III. THE OLD STANNARIES OF THE WEST OF ENGLAND.

By James Quick.

IT is a remarkable fact, though unnoticed by most writers of English history, that there existed in England for many centuries an assembly which held the regulation or supervision of all matters connected with what was for a long period the most important branch of English commercial enterprise. Yet such was the case. The privilege of making laws for the direction of tin mining in the West of England was enjoyed by two convocations designated

the Parliaments of the Stannaries, and which were composed of representatives elected by the miners or "tinners," as the workers in the Cornish and Devonshire mines were until within about the last fifty years commonly called. A Stannary Parliament used to meet at a place called Hengiston Down,* subsequently at Lostwithiel, and later on at Truro, to legislate for the Cornish tin-mines; and another at Crockern Tor,† and in the middle and later ages at Tavistock, for those of Devonshire; the Members of the Parliament for the former county being styled "Stannators," and those for the latter "Jurats." These Parliaments, which in some form appear to have existed from time immemorial, and probably originated in gatherings of the miners to discuss the prospects of their trade, and later on perhaps used to meet to demur at the enormous and oppressive dues levied from them by the Crown, acquired in the Middle Ages a very large amount of power over the mining interest. To become law a Bill for regulating mining affairs required to be signed by each Member of the Stannary Parliament before which it was brought, and then by the Lord Warden of the Stannaries, and by the Sovereign or Prince of Wales in his capacity of Duke of Cornwall, and as such the head of Stannary affairs.‡ During the last three centuries, too, the King or Duke could not make any law for the Stannaries without the consent of the twenty-four Members of either Parliament,|| and in 1752 (see note p. 284) it was enacted that a Bill brought into the Cornish Stannary Parliament required, to become law, to receive the assent of sixteen members of the Parliament.

For the administration of the laws made by the Stannary Parliaments there were at different places Stannary Courts,§ and "in all matters of complaint the tinners

* Near Callington. † On Dartmoor.

In the eighteenth century Mining Committees were occasionally opened at these places and adjourned to a Stannary Court town. (See note § below.)

‡ This was according to an old Charter of Edward III., which was the most important and implicit of any in its grants to tinners.

|| This was according to a Charter granted by Henry VII., and called the Charter of Pardon. Much of the information above given about the Stannaries has been taken from "A State of the Proceedings of the Convocation or Parliament for the Stannaries of the County of Cornwall, held at Lestwithiel, Jan. 29, 1750, &c." by a Cornishman; London, 1751 (see pp. 2 and 3). No Parliament appears to have previously met since 1710. See also Sir Jno. Dodridge's "History of the Ancient and Moderne Estate of the Principality of Wales, Duchy of Cornewall, &c." London, 1630.

§ The old Stannary Court and Coinage towns in Cornwall were Launceston, Lestwithiel, Truro, and Helston, and, after 1811, Penzance. In Devonshire they were Plympton, Ashburton, Tavistock, and Chagford. There were Stannary prisons at Lidford and Lestwithiel. The Stannary districts in Cornwall were Foweymore, Blackmore, Tywarnhaile, Penwith, and Kerrier, each of which sent six members to the Stannary Convocation.

were judged by their own laws before the Judges of the Stannaries," and they were "exempt from all other jurisdictions of the kingdom in all causes within the Stannaries amongst them, except in such as affected land, life, or limb." They were also exempted from serving on juries other than those connected with the Stannaries, from paying tithes to the clergy out of their wages, and could sell their their own goods at fairs or markets without paying any fees. The Stannary Courts were held every three weeks, and a Court was held once every month by a Vice- or Sub-Warden to hear cases brought from the other Courts; and from the decision of this Court there was no appeal, save to the Lord Warden, the Duke in Council, or the King in person.

The history of Stannary matters prior to the seventeenth century, owing chiefly to many valuable records having been destroyed during the Civil War, is very obscure, but during that and the following century the Stannary Parliaments were extremely active. The memberships appear to have been regarded as posts of considerable importance, and to have been eagerly sought after, possibly with some sinister motive of ulterior gain, for during those centuries a large amount of underhanded dealing in Stannary affairs seems to have been practised. The writer of an account of a Cornish Parliament which met in 1750 (see note || page 283) remarks that "during the time of an election gentlemen think it worth while to come down into the county who were never seen in it at any other time, and to neglect their business and the pursuit of their affairs for three months together, without having or pretending to have the least knowledge of tin or Stannary matters." The same writer also remarks "that whoever will take the pains to search into it will find the constitution of the Stannaries to be as well established, both by Charter and Act of Parliament, as any particular constitution can be, or even as the constitution of the kingdom itself." Little could he have thought that, within another century from the time he wrote, the Stannary Parliaments, together with most of the ancient customs and privileges of the tinners, would have fallen into desuetude, and that the Stannary Courts would have merged into a single restricted tribunal, with every prospect of that being soon combined with the County Court. Probably the last of the Stannary Parliaments was the one that assembled at Truro on 13th September, 1752.* In

* The laws made by this Parliament are given in "Laws of the Stannaries of Cornwall, &c.," published by order of the said Parliament, 1752. In 1752, also, the laws made 22 Jac. I., 12 Car. I., and 4 Jac. II. are confirmed.

1837 the different Courts were consolidated into one Court, which now periodically meets at Truro, and exercises in many respects similar jurisdiction over Stannary matters to that of the Courts of Chancery, Common Law, and Equity over others. The right possessed by the monarch of pre-emption, or of buying any tin raised in the two counties at his own price and in preference to anybody else, and often exercised in the Middle Ages to a very oppressive extent, was, together with many other Stannary prerogatives, abolished by two Acts passed in the reign of William and Mary,* whilst the dues on tin payable to the Duchy of Cornwall were only cancelled so late as the year 1838.†

One of the oldest and most extraordinary privileges of the West of England tinners was the very widely observed custom of "bounding," which may perhaps remind our readers of the practice of "beating the bounds" still observed by many municipal authorities in England. In Cornwall and Devonshire all land was formerly considered either as being in "severall" or in "wastrell," the former term being applied to lands enclosed or built upon, and the latter to those lying waste or open. By the "bounding" custom any person might enter upon the "wastrell" of another, and mark out by four "boundaries" any certain area. A written description of the land bounded, and the name of the "bounder, had then to be recorded in the nearest Stannary Court, and a notice given to the owner of the soil. The "bounding" had then to be proclaimed in three successive open Courts, and if no objection were raised against it a writ‡ was given to the bounder, investing him with the exclusive right to search for and appropriate to his own use all tin or tin-ore found within the described limits, upon the condition that he pay to the landowner (in Cornwall) a fifteenth part in kind of the quantity of tin raised. The four corners of the land bounded were generally marked by a pole being erected at each, with a furze-bush at the top, and it is told amongst the miners that on May-day

* 1 Wm. and Mary, c. 30, and 5 Wm. and Mary, c. 36.

† By Act 1 and 2 Vic., c. 102, which substituted for the dues an equivalent sum payable from the U. K. Consolidated Fund. For further particulars on this point see Appendix to the Stannary case of Vice *v.* Thomas (London: Saunders and Benning, 1843). The Stannary duty in Cornwall from the time of Richard Earl of Cornwall (granted to him by Charter in 11th of Ed. III.) had been 40s. for every 1000 wt. of tin raised, and in Devonshire from time immemorial 15s. 6d. for every 1000 wt. of tin raised. There was also a custom of stamping or "coining" each block of tin raised and smelted in the district with the Duchy arms. This was also abolished in 1838.

‡ Several of these are still in existence. One is preserved in the Museum of Practical Geology, and Royal School of Mines, Jermyn Street.

mornings the poles used to be decorated with flowers,* and that youths and maidens used to dance around them.

The right of bounding, which could be disposed of by the possessor in the same manner as other property, might be preserved to an indefinite period, either by actually working the land and paying toll, or by annually "renewing" the four boundary marks on a certain day, by performing the operation of cutting a turf a square foot in size, and piling thereon some loose earth in the form of a mole-hill,† at each corner of the land bounded. Neglect to renew the bounds in any one year forfeited the right to them, and evidence before the Equity Law Courts of recent years‡ has confined the area boundable to narrow limits, although such limits have never been accurately defined. As to the origin of the bounding custom we incline to the opinion of Dr. Borlase,‖ that the privilege was granted for the encouragement of the tinners. Possibly it may have been at first confined to the "wastrell" land of the King, who would, of course, derive benefit from such land being worked for tin, and afterwards became applied to all wastrell. It soon, however, grew to be a very oppressive custom for the ordinary landowner. The injustice of one man being able to prevent another from raising the tin that might be in the latter's ground was certainly very great, and it is a matter for surprise that anyone upon coming into possession of land in Cornwall did not at once take up his own bounds, for we cannot find any Cornish Stannary Law which confined the holding of bounds to the tinners. The bounding custom in Devonshire differed from the custom of Cornwall in that the tinner could search in "severall" as well as in "wastrell," and was not compelled to pay any toll or compensation to the lord of the soil. Tin-bounding in the two counties has now, however, been in disuse for many years, and no mine at present worked is held under its tenure.

* It is stated in a footnote at p. 318, vol. iii., of BRAND's Antiquities, that the poles were decorated with flowers on St. John's Day. An old miner told the above-given particulars to the writer of this paper.

† See PRYCE, Mineralogia Cornubiensis.

‡ Further particulars of the bounding custom in the West of England will be found in a Treatise on the Laws relating to Mines, by R. P. COLLIER, Barrister-at-Law, &c. (London, 1849), which work is a valuable digest of Stannary Law. See also Britain's Metal Mines, by J. R. PIKE (London, 8vo., 1860); also PRYCE's Mineralogia, pp. 137—139, and BORLASE's Natural History, p. 167. The Charter which principally confirmed the privilege was one of 33 Edward I. (A. D. 1305), and others referring to the custom are still extant.

‖ See his Natural History of Cornwall, Oxford: 1758.

IV. A NEW THEORY OF TERRESTRIAL MAGNETISM.

By Profs. Perry and Ayrton.

At the meeting of the Physical Society on the 7th inst. Mr. Ayrton explained, on behalf of Prof. Perry and himself, a theory of terrestrial magnetism which has the great novelty that it makes the existence of the earth's magnetism depend solely on the earth's daily rotation, and does not require, as do all other theories based on electro-magnetic phenomena, the existence of other bodies in the universe. In fact, they have arrived at the result that if any body of any material has a static electric charge, and if it rotates about an axis, then *per se* there will be a magnetic field in the interior of this body as well as in the neighbourhood outside.

In 1876 Mr. Rowland, working in Professor Helmholtz's laboratory, proved experimentally that a quantity of electricity in mechanical motion acted like an electric current in deflecting a magnet, and it is on this result that Profs. Perry and Ayrton have based their whole theory. For they point out that since the points near the surface of the earth have different linear velocities from those in the interior (although all the points have the same angular velocity of rotation round the earth's axis), it follows that if the earth had an initial electrical charge, residing of course in accordance with the well-known electrical law, on its surface, the electrified particles would have velocities relative to the remainder; hence, as a direct consequence of the results of the experiments published by Professor Helmholtz, the interior of the earth would be a magnetic field, quite independently of its interior constitution, and precisely similar reasoning, of course, proves that outside the earth's surface there would also be a magnetic field.

In order to solve the difficult problem of determining the electro-magnetic potential at any place, they have calculated the magnetic forces produced at any point, first by the rotation of a small electrified area on the surface of the earth, and then, by summation, the force produced by the rotation of the whole electrified surface; and they have shown that these forces are the same as would be produced by certain definite distributions of attracting matter over the earth's surface, from which, by the use of spherical harmonics, they have arrived at the conclusion that if the earth had an angular

velocity of rotation around its axis w, and a uniform electric density σ, then there must be an electro-magnetic potential at any point inside the earth equal to—

$$\frac{4\pi}{3} 2\sigma w r \cos\theta,$$

and at any point outside the earth—

$$\frac{4\pi}{3} 2\sigma w \frac{1}{r^2} \cos\theta,$$

where the unit of length is the earth's radius, r the distance from the earth's centre of the place in question, and θ its co-latitude.

Now this electro-magnetic potential will be accompanied with certain magnetic forces varying from point to point, and the magnitude of these forces will depend on the interior constitution of the earth. As an example of their conclusion they examine what would be the distribution of magnetic intensity if the earth consisted of a hollow iron shell, and they have, using Poisson's formula, arrived at the following result :—If such an iron shell has an initial uniform charge of static electricity, and if it has an angular velocity of rotation round a diameter, then, independently of all other bodies in the universe, and independently of the coefficient of magnetisation of the iron, there will be at any point on the surface of the sphere, having a latitude λ, a magnetic force proportional to—

$$\sqrt{1+3 \sin^2\lambda}.$$

But this result is the same as that given by Biot's well-known law for the distribution of magnetic intensity on the earth's surface, hence giving considerable probability to their theory.

It is important to notice that in previous examinations of the earth's magnetism it has been usual to start with the known law of distribution of magnetic intensity, and then deduce what arrangement of magnets, &c., inside the earth would lead to this distribution; but in this new theory of Profs. Perry and Ayrton they start merely with an experiment described by Prof. Helmholtz, of the effects of a rotating electrified body, and show from this that the earth by its rotation alone *must* be magnetic, and next prove that if there be an iron shell, thick or thin, in the earth then the distribution of magnetic intensity on the earth's surface will be the same as is known to exist from observation.

Next as regards the sign of the electric charge on the earth's surface required to produce the earth's magnetic polarity—Is it in accordance with the known phenomena of atmospheric

electricity? To produce the earth's magnetism we must have, in accordance with the known laws of electro-magnetism, a *negative* current flowing from west to east or in the direction of rotation of the earth. In the language of the new theory, therefore, the surface of the earth must be *negatively* charged, but Sir William Thomson has proved, by observations with his electrometer, that all the phenomena brought to light by atmospheric electricity, on a fine day, would be observed just as they are if the earth had a *negative* charge.

Lastly, in order to get a rough approximation of what sum be the difference of electric potential between inter-planetary space and the earth so that its own electric charge alone combined with its known rate of rotation round its axis shall produce the earth's magnetic moment, as determined by Gauss, Profs. Perry and Ayrton take as an example a solid sphere of iron of the size of the earth, and rotating with the earth's known rate of rotation, and they prove mathematically that something like a difference of potential a hundred million times the electromotive force of a single Daniell's cell would be sufficient for this purpose. They notice that there is no difficulty in imagining such a difference of potential to exist between the earth and inter-planetary space, seeing that there exists between the earth and the planets an enormous region of space having an insulation far higher than that of such a vacuum as experiment has shown will not allow the passage, from one point to another very near it, of a spark produced by a large induction coil.

Lastly, they draw attention to the fact that they have assumed the distribution of the electric charge on the earth's surface to be uniform, and so have arrived at a distribution of magnetic intensity dependent only on the latitude of the place, but that since it is possible that the sun and planets may have potentials differing immensely from that of the earth, it might be expected that the distribution of electric charge on the earth's surface, and consequently the magnetic intensity, would have variations like those of the tides, in fact such variations as are known to exist; also, that it would be anticipated that any sudden formation of vapour on the earth's surface, or alteration of the sun's atmosphere, or anything causing change in the lines of *static electric induction* from the sun to the earth ought to cause disturbances such as we know as magnetic storms. The alignments of planets with the sun and earth, again, or the proximity of planets to the sun having less than the average difference of electric potential from it, ought to diminish the disturbances

in the solar envelope and produce the alteration of sun-spots and terrestrial magnetism which is known to accompany such planetary motions. And, lastly, the authors point out that the known lagging of changes in terrestrial magnetism behind variations in the sun-spots would be explained if the great pressure to which the iron in the earth's interior is subjected produces, as is very probable, considerable coercitive force, for such coercitive force would necessarily cause changes in the magnetic intensity to lag behind the disturbances in the earth's electric charge produced by alterations in the static induction of the sun and planets.

V. THE JABLOCHKOFF CANDLE; ITS PRACTICAL RESULTS IN LONDON.

By Charles W. Quin.

It is now nearly three-quarters of a century since Davy first exhibited to an astonished and delighted audience, at the Royal Institution, a voltaic arc, of no less than 4 inches in length, playing between the terminals of the great 2000-plate battery with which he obtained illuminating and heating effects exceeding anything of the kind which had been heard of up to that time. For many years the most brilliant of terrestrial lights received no practical application, owing to its defects being numerous and apparently insurmountable. As to its adaptation to the wants of every-day life no really serious move was made in the right direction until within the last few years, up to which period it had only been used for lighthouses, building works carried on at night, a few factories, and some startling theatrical effects at the Paris and Vienna Operas, the efforts to apply it to street-lighting and other general purposes being sporadic and generally unsuccessful. Many of its original defects have long since disappeared, but much still remains to be done. The rapid waste of the charcoal terminals was obviated by Foucault in 1842 by the use of hard gas-retort carbon. The gradual weakening and ultimate cessation of the current, and the proportional diminution and final extinction of the light, were got rid of by the inventors of the different forms of constant batteries. The unequal wearing away of the two poles, and the consequent flickering and frequent extinction of the light, were almost entirely triumphed over in 1846 by Staite, who invented the first regulator or electric lamp. The enormous expense caused

by the rapid consumption of zinc in the battery was reduced to a minimum by the application of Faraday's great discoveries in magnetic electricity by Holmes, who made the first offer of his machine to the Brethren of the Trinity House in 1857, and their ultimate acceptance of it for lighthouse purposes on the recommendation of Faraday formed a new era in the history of the light.

The next important step in the development of the subject was the great increase of magneto-electric force obtained by means of the Siemens armature, in which the wire was wound longitudinally instead of transversely. Perhaps the most important step of all was the discovery—made apparently independently by Siemens, Wheatstone, and Varley—that the small amount of magnetic force contained in every bar of wrought-iron was capable, by action and reaction, of producing heating and lighting effects far superior to those obtained with artificial magnets. Another difficulty, the expense involved in having to provide a certain number of artificial magnets which were constantly losing strength, was now done away with, the cost of the current being thereby still further reduced.

In 1876 M. Denayrouze startled his colleagues of the French Academy by laying before them an invention of a young Russian officer which he designated an electric candle. Until then no one had thought of keeping the tips of the carbons always at the proper distance by placing them side by side. This was the most simple form of lamp yet invented; there was no clockwork to get out of order; there were no trains of wheels to stick when they ought to move; no delicate but capricious magnets; but simply a couple of brass sockets, with a double-stemmed candle, consisting of two rods of carbon separated by a seam of kaolin, thrust into them.

This is neither the place nor the time to inquire whether the Jablochkoff candle is better or worse than any other form of electric lamp. Be this as it may—and the truth can only be arrived at after long experience—the Jablochkoff candle was the means of bringing the question of electrical illumination before the world in a very prominent manner. It was first applied in Paris to the illumination of the show-room of the Magasins du Louvre and the adjoining court-yard, the Place and Avenue de l'Opéra, and the Hippodrome, thereby becoming the most prominent system of electric lighting known to the general public. From Paris its fame spread to London, and within the last three or four months its capabilities for public use have been put to the test in the metropolis in four different places—the Thames Em-

bankment, the Holborn Viaduct, the British Museum Reading-Room, and the fish-market at Billingsgate, under the able direction of M. Berly, the agent of the Paris Société Générale d'Electricité.

The experiment on the Thames Embankment is the most extensive and important that has yet been tried in England, both with respect to the number of lamps used and to the elaborate data which are being collected with regard to its cost, its virtues, and its defects. The portion of the Embankment which is lighted is that lying between Westminster and Waterloo Bridges, a distance of about three-quarters of a mile. The experiments have been carried out by the Metropolitan Board of Works under the direction of their engineer, Sir Joseph Bazalgette, and their consulting chemist, Mr. T. W. Keates, who have both determined that as far as in them lies the experiments shall be thoroughly exhaustive and their results conclusive. The engine and machinery for generating the currents are placed in a wooden shed on the west side of the Charing Cross railway-bridge, the conducting-wires being led under the roadway to the subway which runs under the footway next the parapet-wall of the Embankment, and so up to the lamps, which are carried by every alternate gas standard on the wall. The engine, which is by Ransome, Sims, and Head, is one of 20-horse power nominal, working at about 45 lbs. pressure. It is provided with a newly-invented governor, acting on the expansive principle, and has worked uninterruptedly from the 16th of December until the present time without a single hitch. It is to the extreme regularity of this engine and to the solid foundation which has been laid for it that we must attribute the great superiority in the steadiness of the Jablochkoff lights on the Embankment as compared with those shown elsewhere. The dynamo-electric apparatus consists of a Gramme continuous-current exciting machine and an alternate-current distributing machine of 20-light power. The number of lamps is twenty, divided into four circuits of five. They are, with one exception, supported by the gas standards on the river parapet, and, roughly speaking, are from 40 to 45 yards apart. From the engine-house to the farthest lamp on the Westminster side the distance is about 700 yards, and to that on the Waterloo side about 450 yards. Each lamp contains four candles, which are switched into their places as they are required by an automatic commutator, the holders being surrounded, with one exception, by opal glass globes 20 inches in diameter. Two lamps near the railway bridge are provided with flat reflectors, 4 feet in diameter, placed on the top of

the globe so as to throw the light downwards. One of these reflectors is turned inwards at an angle of 30 degrees for the space of 6 inches all round. The lamp under the bridge is provided with a very coarsely frosted globe, which shuts off much less light than the opal globes. Each candle lasts one hour and a half; the lamps therefore burn for six hours every evening.

The three months' trial ended on the 16th of March, but the Board of Works, determined that the experiments should be as complete and conclusive as possible, have renewed their contract with the Société Générale d'Electricité for an additional period of three months. It is rumoured that before long the whole of the Embankment from Westminster to Blackfriars will be illuminated by Jablochkoff candles. This could easily be done by providing an extra pair of dynamo-electric machines to work in the shed, the engine being at present worked at less than half its strength.

About the beauty and efficiency of the electric light, as applied to the illumination of the Embankment, there can be only one opinion, and that a most favourable one. The sternest opponent of the electric light, on account of its "weirdness," its "ghastliness," and so on, need only stand in the middle, and glance alternately at the Victoria and Albert Embankments, to be thoroughly converted, unless he be a gas engineer in what theologians call "a state of invincible ignorance." With men like Sir James Bazalgette and his colleagues controlling the results we may be sure of obtaining data that will be absolutely unprejudiced and indisputable, and their report will be awaited with impatience by all who take an interest in the great battle of the lights.

The City Commissioners of Sewers have acted very differently to their brethren of Spring Gardens, and have refused to carry on any more experiments in lighting the Holborn Viaduct, their engineer and surveyor, Colonel Haywood, having apparently frightened them by reporting that although the electric light was seven times as bright as gas, it was seven and a half times as dear. These city Solons would evidently like to buy diamonds at the price of paste, or shall we say real turtle at the price of mock? According to Colonel Haywood's Report, made on the 28th of February, to the Streets Committee of the City Commissioners of Sewers, arrangements were entered into in November last with the Société Générale d'Electricité for lighting the Viaduct from the western end of Newgate Street to Holborn Circus, with sixteen Jablochkoff candles, for a period of three months, beginning on the 14th of December.

Fifteen of the lamps were placed at alternate intervals on each side of the road, and one in the centre of the carriage-way at the junction of Newgate Street with the Viaduct. As in the case of the Embankment the existing lamp-posts were utilised, the gas lanterns being removed, and replaced by the Jablochkoff candles and their accompanying opal glass globes. Measured crosswise the distance between the electric lamps was about 110 feet, or about one-tenth farther apart than the electric lamps on the Embankment. The light from each lamp, according to Colonel Haywood, covered an area of 888 square yards of public way. The lamps were lighted at sunset and continued burning until midnight, and replaced eighty-six gas-lamps, sixty-one of which were lighted at midnight. The electric lamps were fed by two Gramme machines of a similar pattern to those used on the Embankment, driven by a Robey engine of 20-horse power nominal, the whole of the machinery being contained in a temporary shed erected on a piece of vacant land on the western side of Farringdon Street, close by the Viaduct. The lamps were arranged in four circuits of four each, and the conducting-wires were laid in tubes beneath the road and carried up inside the lamp-posts to the candles. The arrangement of the commutators was the same as on the Embankment.

With respect to the cost of the experiment, from the 14th of December to the 18th of February, the figures given by Colonel Haywood are as follows ;—The Company agreed to provide, fix, and fit up the engine, machinery, conductors, and lamps for £236 8*s*. 4*d*., and the Sewers Commission agreed to provide the shed and its enclosures and to do all the work underneath the roadway for about £267. From this must be deducted £50 10*s*. 10*d*. allowed by the Gas Company for gas not burnt. In case of sudden extinction—which happened on four different occasions—a lamplighter of the Gas Company was in constant attendance from sunset to midnight (an average of seven hours per night), at the comfortable wages of a few pence over £2 per week, which can hardly be considered exorbitant considering the onerous nature of his duties. The sixteen lamps were lighted from sunset to midnight at a charge of £5 per night, the average time which they have been alight during the experiment being (according to Colonel Haywood) about seven hours. The total cost of the experiment therefore, after deducting the saving in gas, cleaning the lamps used, &c., was £785 6*s*. 5*d*. The rest of Colonel Haywood's calculations we need not enter into minutely. He tells us that, at

£5 per light for the sixteen lamps, the cost per lamp is 10*s.* 7*d.* per hour, or about £3072 per annum for 4300 hours; the eighty-six gas-lights originally used costing during the same period £419, which is about two-fifteenths of the major sum. Colonel Haywood seems to have made no photometric experiments on the Viaduct light; his estimate that the sixteen electric lights give out about seven times more light than the eighty-six gas-burners is based on the observations of others. It will be seen from this that Colonel Haywood's data of the relative value of the two kinds of light are somewhat hazy; it would be interesting to compare his figures with those contained in the report of his *confrère*, M. Cernesson, Surveyor to the Paris Municipality, but the exigencies of space prevent it.

The lighting of the City thoroughfare was as successful in its way as that of the Embankment; the passage of the wayfarer from the yellow gloom of Newgate Street into the "sweetness and light" of the Viaduct was hardly more striking than his emergence out of the gentle moonlight of the Jablochkoff candles into the darkness visible of Holborn.

The third experiment was a highly successful one, and proved most satisfactorily that by employing a sufficient number of lights the British Museum Reading-Room might be made available to readers by night as well as by day. Fifteen out of the nineteen reading-desks were lighted by eleven candles, enclosed in an opal globe, and placed on a pedestal 15 feet high, fixed in the exact centre of each desk, a twelfth lamp being placed in the centre of the room: four of the desks were consecutively lighted, the remainder alternately. The reading-room was kept open until 7 o'clock on the evenings of the 3rd, 4th, and 5th of March, in order that the new light might be thoroughly tested in a practical manner by those for whose benefit the experiment had been made. The readers showed their hearty approval of the innovation by remaining behind in considerable numbers. On the first night a strange incident occurred. At a few minutes before 6 o'clock, when the twelve Jablochkoff candles suddenly flashed forth, the readers, led by a well-known poetess, and forgetting they were in the reading-room, evinced their admiration of the beauty of the light by a burst of applause—a sound hitherto unknown under that dome of silence.

Mr. E. A. Bond, the Principal Librarian of the British Museum, to whose initiative these experiments are due, has been at some trouble to collect information as to the opinion

of the readers, and there appears to be a general agreement that at the four desks each lighted with a candle it was perfectly possible to read, write, trace, draw, and even colour, with the greatest ease and comfort. The source of electricity on this occasion was a 20-light Robey portable engine of 16-horse power nominal. Four circuits of four lamps each were used,—that is to say, twelve in the reading-room, one in the entrance-hall, one in the portico, and two in the machine and engine shed. This experiment was remarkable for being the first occasion of the introduction of an improved Jablochkoff candle, in which the kaolin seam is replaced by a composition which is a feeble conductor. One of the great defects of the electric light, re-lighting by hand after sudden extinction from accidental causes, is thus entirely done away with. The use of carbon priming for lighting or re-lighting is no longer necessary, and one, two, three, or the whole of the four candles may be lighted or extinguished by turning the handle of a suitable commutator, or if one goes out it lights itself automatically without extinguishing its neighbours.

The Société Générale d'Electricité deserves great credit for the public spirit they have shown in gratuitously supplying everything necessary for making this interesting experiment, which will be carried into practice as soon as the consent of Parliament has been obtained.

The experiments at Billingsgate were a complete failure, from a somewhat ludicrous cause—the Jablochkoff candle was literally too good for the work it had to do. Business at Billingsgate, as most people know, begins between 3 and 4 in the morning, most of the bargains between the salesmen and their customers being struck by gaslight. If the beautiful goddesses and fairies of the stage that used to enthral our hearts in the days of our youth had been suddenly transferred from the yellow glow of the footlights into the open daylight the disenchantment would have been painful—Titania would have become converted into a Hecate, and Venus into a Megæra. The Billingsgate fish suffered a similar transformation under the searching rays of the electric light: soles that would have fetched a shilling a pair by gaslight looked dear at sixpence, while turbot fresh from the sea looked a week old. The result was a general outcry. The copious and ornate dialect of the locality was enriched by a number of notable additions during the few days that the electric light had sway; so, for fear of a revolt among the "bummarees," as the fish salesmen are called, the Corporation was obliged to restore

to them their beloved yellow gaslights, and, through no fault of its own, the Jablochkoff candle know the sons and daughters of King Belinus no more.

VI. THE TORNADO AT WISCONSIN IN 1878.

NEARLY five years ago* we gave as full a description as could be obtained of a tornado which occurred in Iowa and Illinois on May 22nd, 1873. Singularly enough a storm, accompanied by a tornado extending nearly across Wisconsin, which for extent and violence surpassed any storm previously recorded in the history of the State, swept over Iowa, Northern Illinois, and Southern Wisconsin only one day later of the same month in 1878. We are indebted to Prof. W. W. Daniells, of the University of Wisconsin, for a complete account of the tornado of May 23rd, 1878, and we regret that the space allotted to a single article in the "Monthly Journal of Science" will only permit of a very brief abstract of his description of its character and extent.

With regard to the origin of the storm it has been impossible to obtain data leading to any definite conclusion. At about six o'clock of the same day a tornado from a south-westerly direction struck the earth near Barrington, in the north line of Cook county, Ill., passing over Highland Park half an hour later. The same evening at half-past six a tornado occurred north-east of Quincy, Ill., in Adams and Brown counties. The occurrence of these several tornadoes, together with the fact that heavy rains fell throughout eastern Iowa, northern Illinois, and southern Wisconsin on that afternoon, shows that the Wisconsin tornado was not entirely the result of local conditions, but that some cause producing unusual atmospheric disturbance was widespread. The weather chart accompanying the Monthly Weather U.S. Signal Service indicates an area of low barometer for May 23 extending over the entire Lake region.

The evidence obtained by Prof. Daniells seems to prove conclusively that the tornado was a whirlwind of unusual

*See Quarterly Journal of Science, vol. v. (N.S.) p. 339.

proportions, having its motion of revolution in a direction opposite to that of the sun, or from the east to the north, west south, to the east again.

According to the most reliable information as to the rate of the forward movement of the storm, it struck the earth at about 3 p.m., between which time and 5.30 p.m. it passed over a distance of sixty-four miles, which would be at the rate of twenty-six miles per hour. Any conclusions upon the velocity of the wind within the tornado can, as Prof. Daniells points out, only be reached by indirect methods. The wind's rotatory velocity south of the axis would be increased, while north of the axis it would be diminished by the progressive movement of the storm. The amount of increase or diminution would, however, differ greatly in different parts of the whirl. That the velocity of the motion of revolution increased from the outer edge toward the centre is shown by the fact that buildings near the centre were reduced to the dimensions of kindling wood in a way which never occurred near the borders of the track. Hence the horizontal velocity of each particular volume of air was continually changing as it occupied different positions relative to the axis and to the centre of the whirl. Again the wind had an upward motion, the perpendicular component of which acted upon bodies to hold them in suspension. The velocity of this upward motion also increased toward the centre of the whirls, and was comparatively slight near its limits. The actual velocity of the wind at any point would be the resultant of the horizontal and perpendicular velocities at that point.

The perpendicular velocity can be approximated by the lifting force exerted upon bodies while holding them in suspension. For instance, a horse weighing about 1100 lbs. was carried over twenty rods. Another of about the same weight was carried about eighty rods. A horse of this size would not expose a lifting surface to the wind of over 14 square feet. To lift such an animal would require then an upward pressure of the air of $\frac{1100}{14} = 78{\cdot}5$ lbs. per square foot. This pressure is produced by wind moving with a velocity of 124·6 miles per hour. In the township of Jefferson a granite boulder 15 in. square and more than 6 in. in thickness, and weighing more than 130 lbs., was carried 15 rods. To hold this rock in suspension would require a lifting force of 852 lbs. per square foot of surface. This force would be exerted by wind moving with an upward velocity of 129 miles per hour. These examples show the

great velocity with which the wind moves in such a storm.

There were three distinct storm paths west of Rome, Jefferson county, which is as far as Prof. Daniells's personal observations extend. It was, he remarks, impossible to trace any connection upon the surface of the earth between the different storms, although the ground between them was all passed over and most diligent enquiries made. The evidence gathered in regard to the time of occurrence of these different tornadoes shows conclusively not only that they were distinct but also that the second and third were contemporaneous with the eastern portion of the first one, and also that the three were dissipated very nearly at the same time.

The effects of these tornadoes as pourtrayed by Prof. Daniells was most disastrous. In the City of Mineral Point alone the amount of damage is estimated at 3,904,500 dols. The largest individual losses in this city were those of Mr. Gillman and Mr. John Spensley, respectively 20,000 dols. and 11,000 dols. The house of Mr. John Spensley, a large new frame house, with all its contents, was torn to fragments and swept away, some portions having been found a mile distant. Twelve persons were in the house, all but one of whom were in the cellar. Mrs. Waller, who remained to find a child she supposed had not gone to the basement, with the family, was instantly killed. No one in the cellar was injured. Mr. Spensley's barn and carriage house were completely demolished, and his carriages and sleighs broken past repairing, yet a horse standing in the farm escaped without injury. Another house belonging to Mr. Spensley, occupied by George Leonard, was destroyed, and Mrs. Leonard instantly killed. Seven other houses and five barns were utterly destroyed, and four houses and a large brewery unroofed and otherwise injured. Mrs. Myers and Mrs. Bohan were killed, as their respective residences were destroyed, and several others received injuries. Three horses and some other stock were killed within the city limits. Had the storm continued the course it was on, half a mile west of the city, it would have passed for three quarters of a mile over a part of the town very largely populated, in which case the loss of life would have been very great, and the suffering and destruction of property many times greater than it now is.

As the storm passed on its way a school house containing fifteen children and the teacher was destroyed, everything above the floor being carried away. The teacher was carried about 15 rods, but not materially injured. Two children

were killed, one injured, the remaining escaping without injury. The school record was found next day over sixty miles distant.

At about three quarters of a mile from the township of Waldwick the barn and blacksmith shop of Stephen Terrell were razed to the foundation. A waggon and threshing machine and many other farm implements were broken, a corn cultivator was carried twenty-five rods and completely demolished. Mr. Terrell's house, near the centre of the storm's path and in a very exposed position, was scarcely injured. Seeing the storm coming some distance across the prairie, Mr. Terrell hurried his family into an embankment cellar, remaining outside himself until it was but a short distance away. He described the cloud as reaching to the ground, the lower part so black and opaque that nothing could be seen within it, while at a height of 200 or 300 feet the air appeared to be filled with trees, rails, boards, hay, leaves, and other débris, all rapidly whirling, and shooting upward and downward in terrible commotion.

The storm passed into the township of Primrose. The house and outbuildings of M. Obermbt were swept away. The house was torn to pieces and scattered to the south and south-east. Mr. Obermbt and seven children were in the house at the time, and were thrown into the yard with the flying fragments of the house. One boy, fifteen years of age, was carried about fifteen rods nearly south into a ravine. Although the ground was so thickly strewn with the ruins as to be literally covered for 100 yards to the south and south-east, no one of these eight persons was seriously injured. The farm waggon before the storm stood six rods east of the house. After the storm it was in ruins twelve rods west of the house. Fifty rods south of Obermbt's, where a granary was being built, a waggon loaded with lumber was broken to pieces. One wheel was carried a quarter of a mile directly east, and another a mile and a quarter in the same direction.

Nearly half a mile east of Obermbt's the house and outbuildings of J. Osmonson were destroyed. Mr. Osmonson seeing that a severe storm was approaching, left the field where he was at work, that he might not get wet. Becoming somewhat alarmed at the roaring, the continuous lightning and thunder, and the very threatening aspect of the sky, he waited at the stable only long enough to unharness one horse, hurried into the house and told his wife they must hasten to the cellar. A boy of fourteen years and a girl of eight got into the cellar, and Mrs. O., with an infant

three months old, were partly down when the house was taken bodily. At this time Mr. Osmonson, with a child in each hand, aged respectively four and six, stood at the cellar door waiting for the mother and her babe to get fully down. Besides these, there was in the house a girl twelve years old. This girl was found thirty yards distant north of east, senseless, nearly buried in mud, with two severe scalp wounds, and her right arm broken, three times between the shoulder and elbow. About four rods north of the house was the border of a large field of second growth oak and poplar timber, from 20 to 40 feet in height. The house was carried over the timber, with Mr. Osmonson and the two children, whom he still held firmly in his grasp. While in the air over this timber the house "went to pieces," the larger portion of it falling sixteen rods directly north of its starting point. One portion of the roof was twenty-five rods distant in a direction north 30° west, and another portion sixty rods distant north 25° east. The stove was mainly found seven rods directly north of the principal ruins of the house ; some parts, however, were carried several rods farther in the same direction.

Mr. Osmonson and the two children fell about 20 ft. north of the main ruins of the house. Mr. Osmonson had his face scratched and one rib broken in falling through the top of a tree. The children were entirely unhurt, the youngest one did not even cry. Large hail was falling at the time, and the children were laid under the ruins of the house while the father hastened to find the other members of the family. The children in the cellar were not hurt. Mrs. Osmanson was injured in the back, probably by something striking her as the house moved off. The stable in which the horses had been put was eight rods south-west of the house. One of the horses was blown into the cellar, and lay there upon his back when found, while the other was in the standing timber, twenty-two rods distant, north 38° east from the stable, with his hind feet resting upon the ground, while his fore feet were hanging upon a bent over sapling. The position of the horse, and the thick growth of timber rendered it impossible for him to get there only by being carried above the tops of the trees and dropped down. He was uninjured. An iron pump with 46 feet of zinc pipe was taken from a well, and carried north 15° west, a distance of fifteen rods. A lumber waggon was broken entirely to pieces. One wheel and an axle was carried north 65° east, seventy-five rods, while the larger proportion of the remainder went north-

east sixteen rods. One wheel was entirely broken to pieces and the tire left hanging on a tree 10 feet from the ground. This tire, $\frac{1}{2}$ in. thick, 1 and $1\frac{1}{2}$ ins. wide, and very slightly worn, was broken twice in two and bent in such a manner as to show that it had been acted upon by a force of great power.

Eighty rods north-east of Osmonson's stood a house belonging to Mrs. Ketchum. This house was on the south side of a hill. It was taken bodily from the foundation, up the hill north, and left in a little niche in the woods north-west from its starting point fifteen rods. The family escaped by going to the cellar.

The storm bent to the north at this point, destroying everything in its course, and scattering the débris east, west, north, and south. The deed of a farm, the house and two barns of which were destroyed, was found ten miles distant. A portion of an organ in the house was found $4\frac{1}{2}$ miles directly north, while the boiler and some cooking utensils were carried east one mile. On the bank of Sugar river a granary and log house were destroyed. The stove, a part of the furniture, and some of the logs of the house were blown into the river. A lady school teacher boarding at the house was saved from the same fate by a log falling upon her and holding her down. And this work of destruction proceeded until in Jefferson county the storm entered an open marsh and was dissipated.

It has often been noticed that the severity and destructive violence of tornadoes is much greater in some portions of their path than in others. In the present case there are instances of buildings remaining uninjured while strong ones near by were destroyed. In many places where there was continuous timber there would be strips from ten to thirty rods long in a direction parallel to the axis of the storm, where nearly every tree was prostrated, then an interval where little damage was done, and again another piece where all were down. Tracts of interrupted violence frequently reached entirely across the track of the tornado, but they usually extended only partly across. In Prof. Daniells's opinion they were more frequent north of the axis than south.

VII. THE ELECTRIC LIGHT FOR INDIA.

IN the report on the results obtained by the electric light experiments, instituted on behalf of the Board of Directors of the East Indian Railway Company, Mr. Louis Schwendler states that in endeavouring to determine the quantity of light per unit of power, unit of speed, and unit of money, he tried four different dynamo-electric machines producing the electric current in *one* direction, viz., a medium and a small-sized Siemens (Hefner von Alteneck) dynamo-electric machines; a dynamo-electric machine, workshop pattern, as supplied by Messrs. Soutter and Lemonnier, of Paris; and a dynamo-electric machine with two sets of brushes, as supplied by the British Telegraph Manufactory. The trials have established that these four machines are all sufficiently practical for the production of the electric light, and that the unit of light as produced in the electric arc by any of the four dynamo-electric machines is at least fifty times cheaper than the unit of light as produced by combustion, considering the expenditure of power only. This represents an enormous engineering margin in favour of the electric light.

The Siemens medium dynamo-electric machine, however, gives about double the quantity of light of any of the other three machines, and only about half as much power is expended to produce the unit of light. This favourable result is principally due to the comparatively small internal resistance of the machine, and to its low speed.

For use in India Mr. Schwendler proposes several alterations in the machine.

With regard to lamps, Mr. Schwendler for practical use prefers the Serrin lamp, with the following alterations, which his own experiments have suggested :—

1. All parts of the lamp, including clockwork, &c., to be made of gun-metal. No steel or iron is to be used except in the electro-magnet and its armature.

2. The carbons may be of any shape—round, triangular, or square. The carbon-holders, constructed as in the Siemens lamp, should be sufficiently large to hold a round carbon of 18 m.m. diameter. With 18 m.m. carbons the lamp should burn eight hours.

3. The screw for regulating the tension of the spring or springs which act in opposition to the magnetism of the

electro-magnet should move 1 m.m. by one whole turn. In order to know the adjustment at any one time, and to be able to make the *same* adjustment again, a millimetre scale is to be attached, by which the movements of the screw can be read. The circumference of the head of the screw is to be divided into ten equal parts. Hence by this arrangement the comparative tensions of the spring or springs can be read up to 0·1 m.m. This micrometer screw, after the lamp has been regulated for any given current varying within two known limits, is to be fixed by a clamp and screw to keep that adjustment *constant.*

4. The distance between the electro-magnet and its armature is also to be made adjustable by a micrometer screw, provided again with millimetre scale like the above, and with a clamp and screw, for making any best adjustment *constant.*

5. The clockwork and all regulating parts of the lamp to be entirely covered by a strong metal case, which is to be constructed in such a manner that it can be taken off or put on without interfering in any way with the adjustment of the lamp. The case may be cylindrical, opening like a door in two halves on hinges, and with a key for closing it. None of the adjustable parts of the lamp are to protrude, as it is intended not to touch the lamp after its proper adjustment, which is done in the laboratory only. Each lamp has only *one* best adjustment for any given current varying between two known limits, and the best adjustment is made constant by fixing everything. The case or cover is then closed, and the lamp put up for use.

6. The electro-magnet which pulls the arc should offer no more resistance than 0·02 S.U. It is to be *shunted* by another electro-magnet which offers exactly the same resistance. Hence one-half of the current passes through the electro-magnet of the lamp, and is made use of *for pulling the arc.* The amount of iron used in the electro-magnet, and the number of convolutions, should be such that at the mean distance of the electro-magnet from its armature the magnetic force is strong enough to produce an arc of 2·5 m.m. against the mean tension of the spring or springs when employing a current of about 25 Webers. The magnetic action of the shunt for the same current should be about double that of the electro-magnet of the lamp, in order to leave a margin for a finer adjustment, *i.e.*, equalisation of the magnetic action of the two. The two electro-magnets, each forming a shunt to the other, are adjusted in such a manner that the extra currents they produce, when the

primary current varies, are equal, and therefore, as they are invariably opposite to each other, they neutralise one another entirely, which will have the desired effect of a quicker regulation of the lamp for any variation of current. The iron used in the shunt should have double the weight of the iron in the electro-magnet. The section of the wire for filling the shunt should be double the section of the wire filling the electro-magnet. Coil on so many convolutions on to the shunt until its resistance becomes equal to the resistance of the electro-magnet.

For adjusting the quality of the extra currents the following method should be adopted :—

Form a Wheatstone bridge two sides of which are formed by a mercury rheostat, each side offering about 0·02 S.U. resistance. The third side of the bridge is formed by the electro-magnet of the lamp, the fourth side by the shunt. In one diagonal place a dynamo-electric machine and about *one unit* resistance, together with a convenient make-and-break contact, best done by a mercury cup. In the other diagonal place a Bell telephone of lowest possible resistance. One end of this diagonal can be moved along the mercury rheostat. Start the dynamo-electric machine, listen to the telephone, and alter the ratio of the mercury branches of the bridge, by shifting along the contact until the telephone is perfectly silent. Then if, at commencing and stopping the current, a strong click is heard, we know it is due to the two extra currents not being equal, and as we further know that the shunt produces the greatest extra current we make this extra current smaller, by shifting along the two poles of the shunt an iron wedge, until the telephone is quiet when starting and stopping the current. The iron wedge is then fixed in its position. This shunt is also to be *inside* the metal cover of the lamp.

7. The two terminals of the lamp are to be of exactly the same pattern and size as those used in the dynamo-electric machine. They must *not* be terminals with hand-screws, but strong hexagonal-headed screws with lock-nuts.

The *division of the electric light—i.e.*, the production by the *same electro-motor* of a number of lights at different points of a given space—Mr. Schwendler considers to be impracticable from an engineering point of view. Such divisions of the electric light can, he argues, only be effected by a large sacrifice of total and external light, and moreover this loss increases rapidly with the number of lights burned in the same circuit. He succeeded in working three Serrin

lamps connected up successively in the circuit of a dynamo-electric machine, but found the loss of light very great. It appears that the electric light can alone compete with light produced by combustion when produced of great intensity in *one* point by *one* dynamo-electric machine.

Having satisfied himself of the difficulty and impracticability of the division of the electric light, he tried *diffusion*—*i.e.*, a few large lights (each light produced by one machine) are placed at different points of the space, and by optical means the light is diffused over a large area. This method is found to be perfectly practicable. There is naturally also a large amount of light lost (by absorption), but this loss will bear a constant ratio to the total light produced, and probably may decrease with the intensity. The light is diffused by means of a *silvered glass* reflector in which a powerful electric light burns, throwing *direct* and *reflected* rays up to a white ceiling, or any other convenient white surface. The form and size of each reflector must of course depend on the locality where it is to be used.

Mr. Schwendler recommends the employment of the electric light at railway-stations in India. The station is to be lighted up with four powerful electric lights, placed judiciously, so as to have the greatest and most uniform effect, each light being produced by *one* lamp and *one* dynamo-electric machine. Each light, by an opaque silvered glass reflector, is thrown against a white ceiling or any other white surface. No *direct* rays are allowed to enter the retina. This, Mr. Schwendler says, produces a light effect, resembling daylight, with no shade whatsoever. The intensity of the light will be such that the smallest print can be read by a normal eye at any place on the platform.

VIII. PAINLESS DEATH.

IN one of his lectures Prof Tyndall spoke of the probabilities in favour of the entire absence of pain accompanying death by lightning. It is popularly supposed that an impression made upon the nerves—a blow or puncture—is felt at the precise instant it is inflicted, but such is not the fact. The seat of sensation is the brain,

and intelligence of the injury must be transmitted to this organ through a certain set of nerves, acting as telegraph wires, before we become conscious of pain. This transmission or telegraphing from the seat of injury to the brain requires *time*, longer or shorter, according to the distance of the injured part from the brain, and according to the susceptibility of the particular nervous system operated upon.

Helmholtz, by experiments, determined the velocity of this nervous transmission in the frog to be a little over 85 feet per second; in the whale, about 100 feet per second; and in man, at an average of 200 feet per second. If, for instance, a whale 50 feet long were wounded in the tail, it would not be conscious of the injury till half a second after the wound had been inflicted. But this is not the only ingredient in the delay. It is believed that to every act of consciousness belongs a determinate molecular arrangement of the brain, so that, besides the interval of transmission, a still further time is necessary for the brain to put itself in order for its molecules to take up the motions or positions necessary to the completion of consciousness. Helmholtz considers that one-tenth of a second is required for this purpose. Thus, in the case of the whale, there is, first, half a second consumed in the transmission of the intelligence through the sensor nerves to the brain, about one-tenth of a second consumed by the brain in completing the arrangement necessary to consciousness, and, if the velocity of transmission from the brain to the motor nerves be the same as that through the sensor, about half a second more is consumed in sending the message to the tail to defend itself. Therefore one second and one-tenth would elapse before an impression made upon its caudal nerves could be responded to by a whale 50 feet long.

If we regard as correct the calculations representing the average velocity of transmission in the human nerves, and if we estimate the distance from the origin of the filaments in the brain to their termination in the foot as 5 feet, the time required, in case some one steps on your favourite corn, for the news to be telegraphed to the brain, for the brain to prepare a message, and to telegraph the same to the muscles of the leg to draw the foot away, would be about one-twentieth of a second. Now, it is quite conceivable that an injury might be inflicted which would render the nerves unfit to be conductors of sensation, and if this occurred, no matter how severe the injury might be, there would be no consciousness of it. Or it might happen that the power of the

brain to complete the molecular arrangement necessary to consciousness would be wholly suspended before there would be time for the transmission of the intelligence of the injury. In such a case, also, although the injury might be of a nature to cause death, this would occur without feeling of any kind. Death in this case would be simply the sudden negation of life, without any intervention of consciousness whatever.

Doubtless there are many kinds of death of this character: the passage of a rifle-bullet through the brain is a case in point. The time required for the bullet in full velocity to pass clean through a man's head may be roughly estimated at a thousandth part of a second. Here, therefore, would be no room for sensation, and death would be painless. But there is another action which far transcends in rapidity that of the rifle-ball. A flash of lightning cleaves a cloud, appearing and disappearing in less than a hundred-thousandth part of a second, and the velocity of electricity is such as would carry it in a single second of time over a distance almost equal to that which separates the earth and moon.

A luminous impression once made upon the retina endures for about one-sixth of a second, and this is why we see a ribbon of light when a glowing coal is caused to pass rapidly through the air. A body illuminated by an instantaneous flash continues to be seen for the sixth of a second after the flash has become extinct; and if the body thus illuminated be in motion, it appears at rest at the place where the flash falls upon it.

The colour-top is familiar to most of us. By this instrument a disk with differently coloured sectors is caused to rotate rapidly; the colours blend together, and, if they are chosen in the proper proportions, the disk will appear white when the motion is sufficiently rapid. Such a top rotating in a dark room and illuminated by an electric spark appears motionless, each distinct colour being clearly seen. Prof. Dove has found that an illumination by a flash of lightning produces the same effect. During a thunderstorm he put a colour-top in exceedingly rapid motion, and found that every flash revealed the top as a motionless object with its colours distinct. If illuminated solely by a flash of lightning, the motion of all bodies on the earth's surface would, according to Prof. Dove, appear suspended. A cannon-ball, for example, would appear to have its flight arrested, and would seem to hang motionless in space as long as the luminous

impression which revealed the ball remained upon the eye. If, then, a rifle-bullet passing through the brain move with sufficient rapidity to destroy life without the interposition of sensation, much more is a flash of lightning competent to produce this effect. We have well-authenticated cases of people being struck by lightning who, on recovery, had no recollection of pain.

The Rev. Dr. Bartol, who was lately nearly killed by lightning, expressed the belief that if the stroke proved fatal it must produce the most agreeable mode of death; but to be stunned, as he was, is very unpleasant. As soon as consciousness returned he experienced a terrible sense of oppression, and an irresistible weight seemed passing through him, while his mind was dazed so that for a while it seemed he had suddenly been precipitated into wonderland. His recovery was attended by headache, continued for a week.

The following case is described by Hemmer;—On June 30, 1788, a soldier in the neighbourhood of Mannheim, being overtaken by rain, stationed himself under a tree, beneath which a woman had previously taken shelter. He looked upward to see whether the branches were thick enough to shed the rain, and in doing so was struck by lightning, and fell senseless to the earth. The woman at his side experienced the shock in her foot, but was not struck down. Some hours afterward the man recovered, but remembered nothing about what had occurred, save the fact of his looking up at the branches. This was his last act of consciousness, and he passed into the unconscious condition without pain. The visible marks of a lightning stroke are usually insignificant, the hair being sometimes burnt, slight wounds occasioned, or a red streak marking the track of the electric discharge over the skin.

Prof. Tyndall relates—standing in the presence of an audience, about to lecture—that he accidentally touched a wire leading from a charged battery of fifteen large Leyden jars, and the current passed through his body. He says life was absolutely blotted out for a very sensible interval, without a trace of pain. In another second or so consciousness returned. He saw himself in the presence of the audience and in contact with the apparatus, and immediately realised that he had received the battery discharge. The *intellectual* consciousness of his position was restored with exceeding rapidity, but not so the *optical* consciousness. To prevent the audience being alarmed he stated that it had

often been his desire to receive, accidentally, such a shock, and that his wish had at length been gratified. But while making this explanation the appearance which his body presented to himself was that of being in separate pieces. His arms, for example, seemed to be detached from his body and suspended in the air. Memory and the power of reasoning and speech were complete long before the optic nerve recovered from the electric shock. The Professor dwelt upon the absolute painlessness of the shock, and believes there cannot be a doubt that to a person struck dead by lightning the passage from life to death occurs without consciousness. It is an abrupt stoppage of sensation, unaccompanied by a pang.—*National Medical Review.*

NOTICES OF BOOKS.

Notes by a Naturalist on the Challenger, *being an account of various Observations made during the Voyage of H.M.S.* Challenger *round the World, in the Years* 1872-76. By H. N. Moseley, F.R.S. London : Macmillan and Co.

We have here a general summary of the zoological and botanical results of the *Challenger's* vogage. It is remarked in the Preface that " very much of it was intended for family reading," and in fact no small portion of the book might—as far as internal evidence goes—have been written by any observant man of culture having no special acquaintance with any department of Natural History. We find even the music of the Tahitian National Air, a modern production which can scarcely be regarded as an ethnological document. It must further be remembered that, except as far as strictly Oceanic life is concerned, the circumstances of the Expedition were not too favourable to extended biological observation. The proportion of time spent on shore was not large, and, as the author declares, the zoological results of the deep-sea dredgings were disappointing. Hopes had been entertained, by the late Prof. Agassiz and others, that at great depths many important forms of old geological epochs would be found still existing. Such expectations have not been fulfilled; and though many new species, and even new genera, were obtained by the dredge, they belonged as a rule to well-known families, and very few displayed any important structural difference from forms already recognised. " We picked up no missing links to fill up the gaps in the great zoological family tree."

In another respect the results may be pronounced negative. Upon land wide areas have been found specially favourable to the production of variations and the development of new forms. Hence the faunæ and floræ of continents are relatively richer than those of islands. It might therefore have been expected that the ocean-floor, wider far than any continent and enjoying throughout conditions of temperature essentially alike, would have disclosed a corresponding exuberance of forms. The very contrary is the fact: deep-sea life is monotonous. Nor do the voyagers appear to have been favoured with a sight of any of the still unclassified monsters which the sea, according to some witnesses, may be supposed to contain. In *the* sea-serpent Mr. Moseley appears to be a decided unbeliever, and accepts the usual hypotheses of floating sea-weed, flocks of birds, &c., remarking further that " Sea-serpent stories are often utterly

without foundation in fact, and sometimes apparently ships from which they emanate are laden with rum."

On the subject of the visual powers and the colouration of deep-sea species we find some interesting remarks. The author very justly pronounces it quite conceivable that animals might exist to which obscure heat-rays might be visible, and to which men and other mammals would appear constantly luminous. On examining with the spectroscope the phosphorescent light emitted by three species of deep-sea Alcyonarians, he recognised the red, yellow, and green rays only. In connection with this fact it must be noted that "almost all the deep-sea shrimps and Schizopods are of an intense bright scarlet, differing markedly in their intensity of colouring from shallow-water forms, and having, apparently for some purpose, developed an unusually large quality (quantity ?) of the same red pigment which colours small surface Crustacea." Many deep-sea Holothurians are, however, dark purple, and in their case Mr. Moseley considers the colouring useless, and due merely to the persistence of a pigment developed originally in shallow-water ancestors.

Among the more important observations on fishes we may refer to the notice of *Periophthalmus Kolreuteri*, which travels on mud and moist earth, using its pectoral fins as saltatory legs. Concerning flying fish the author agrees with Dr. Möbius, that the species of *Exocœtus* do not flap their fins at all during their flight. As to the *Dactylopteri* (the flying gurnets) he has distinctly seen the fins or wings in rapid movement.

Among terrestrial animals Mr. Moseley appears to have paid the greatest attention to birds. He mentions that the Apteryx of New Zealand "considers it necessary to put as much of its head as it can under its rudiment of a wing, when it goes to sleep"—a very curious instance of the persistency of habits. On the other hand, we find a converse case where part of the penguins on the small island of Inaccessible have learnt to nest in holes under rocks, where the swine cannot get at them. On the north side of the island, where the pigs cannot get, the penguins retain their original mode of nesting on the open ground. The account of the voyagers making their way through a penguin colony is not pleasant reading.

Concerning insects we find fewer observations than might have been anticipated. Referring to a flock of a dozen males of *Ornithoptera poseidon* who were paying their court to a single female, the author proposes some experiments to test hypothesis of sexual selection, both in case of butterflies and birds. "A hen might be kept in a cage between two males, noting to which she gave the preference, and then whether any alteration in the colours of the plumage caused a change in her inclinations."

A starling (*Calornis metallica*) is mentioned as having been found caught in the web of a yellow spider at Little Ke Island, and at St. Thomas a large ground spider (*Lycosa*) is said to

habitually prey on lizards. At Juan Fernandez the author was struck with a showy flora, co-existing with a paucity of insects—an observation similar to that said to have been made in the mountains of Guiana by Schomburgk.

Whilst watching the movements of seals in the water, Mr. Moseley was struck by their close connection with the whales, and saw how easily a whale might be developed out of a seal. The fur-seals when on land still bend their hinder limbs forward, as do land mammals. The sea-elephants carry their hind legs always stretched backwards, when little modification is needed to turn these otherwise useless members into the broad tail-fin of the whale.

With reference to the interval between man and the anthropoid apes, we find an utterance with which we heartily agree :—"The wide, but unscientific, distinction commonly drawn between man and the higher monkeys is an error of high civilisation, and comparatively recent. To the Dyack, the great ape of Borneo is simply the Man of the Woods. The author, when at Tonga, was struck with the manner in which the natives, when conversing, incessantly contract and relax the muscles of the forehead and twitch up the eyebrows, just as do the monkeys.

Passing over abundance of other observations well worthy of notice, we come to a passage on the possible faunæ and floræ of other worlds. " On the theory of Evolution it is impossible that plants or animals, of any advanced complexity, at all resembling those existing on the earth, should exist on other planets or other solar systems. It is conceivable that very low forms of vegetable life may exist on other planets, and may have been by some means transported to the earth : the idea is conceivable, though highly improbable. But it is quite impossible that that infinitely complex series of circumstances which on the earth has conspired to produce from the lowest living forms a Crustacean should have occurred elsewhere ; still less is it possible that a bird or a mammal should exist elsewhere ; still more impossible that there should be elsewhere a monkey or a man. It is even probable that protoplasm itself, the basis of all life, is a production entirely confined to our small planet." To all this we must of course assent if—but only if—species are the result of an infinite number of fortuitous variations.

We cannot better convey our opinion of this work than by expressing the hope that the author may soon be engaged on some exploring expedition which may give him greater scope, and that we may have the pleasure of reading the account of his results.

A Monograph of the Silurian Fossils of the Girvan District, in Ayrshire, with Special Reference to those contained in the "Gray Collection." By H. ALLEYNE NICHOLSON, M.D., &c., and R. ETHERIDGE, Jun., F.G.S. (Fasc. I.—Rhizopoda, Actinozoa, Trilobita.) Edinburgh and London: W. Blackwood and Sons.

THE authors have been led to undertake a detailed and systematic description of the fossil remains found in the Silurian area of the Girvan district, which they consider will be an important contribution towards the solution of certain problems in both palæontology and geology. Judging from this first fasciculus they are carrying out their task with scrupulous care, accuracy, and thoroughness.

Studies in Comparative Anatomy. No. II. "Anatomy of the Indian Elephant. By L. C. MIALL, Professor of Biology in the Yorkshire College, and Curator of the Leeds Museum; and F. GREENWOOD, Curator to the Leeds School of Medicine. London: Macmillan and Co.

THE authors have dissected a young female elephant, and have been able to make not a few additions to and rectifications of the descriptions furnished by earlier observers. They have confined themselves almost exclusively to myology. The osteology and dentition they omit altogether, as ordinary text-books already contain descriptions sufficient for the purposes of the naturalist or the palæontologist.

Concerning the alimentary canal and its appendages, and the circulatory, respiratory, and reproductive systems, they give merely a summary of what is already known, supplemented by their own observations.

As regards the nervous system they are unable to give any novel information, the rather as the brain of their subject was not removed till eight months after death, when it was found too much disintegrated for examination.

They recommend those who may have the opportunity to make a microscopical examination of the large intestine of the elephant immediately while fresh, and also to investigate the brain minutely immediately atter death. The value of this monograph is increased by the bibliography of the subject.

CORRESPONDENCE.

THE SEA-SERPENT.

To the Editor of the Monthly Journal of Science.

SIR,—With reference to the letter of "Serpent-Hunter," in your February No., I would beg to ask whether anyone has taken the taken the trouble to ascertain whether any such narrative as that quoted in Dr. Wilson's work was really deposed to at Liverpool, or whether the whole is not the invention of some audacious *canard*-monger? A friend tells me that this point has been raised by one of your contemporaries.—I am, &c.,

SCEPTIC.

SPIDER'S WEB FOR MICROMETERS.

To the Editor of the Monthly Journal of Science.

SIR,—Mr. W. Mattieu Williams, in his article on "Spider's Web for Micrometers," contained in your last issue, speaks of spiders as "his six-legged friends." This is doubtless a clerical error, as spiders have eight legs.—I am, &c.,

AN ENTOMOLOGIST.

ADHÉMAR'S THEORY OF EVOLUTION.

To the Editor of the Monthly Journal of Science.

SIR,—Has the possible bearing of Adhémar's theory upon the question of Evolution ever been thoroughly discussed? If every ten thousand years the waters of the globe are translated from the Northern to the Southern Hemisphere, deluging the continents on their way, must not the great majority of animal and vegetable species, terrestrial at least, be extirpated? If prior to the last of these great cataclysms the bulk of the water occupied the Northern Hemisphere, where were the wide continents in which the majority of organic species seem to have taken their rise?

I am well aware that Adhémar's theory is far from being demonstrated; but is not its doubtful compatibility with the doctrine of Descent at least worthy of consideration? If the Antarctic ice-cap possesses the dimensions lately ascribed to it in the "Quarterly Journal of Science,"—say a thickness rising from about half a mile at its margin to 24 miles at its centre, and extending over a circle of 3000 miles diameter,—it seems to me that we have here a mass of water which if rapidly set at liberty would go far to render Adhémar's deluges (with the last of which that of Noah is probably identical) at least physically possible.—I am, &c.,

A. P. P.

THE SENSES OF THE LOWER ANIMALS.

To the Editor of the Monthly Journal of Science.

SIR,—In an article on the "Senses of the Lower Animals," inserted in your last year's volume, the alleged fondness of serpents for milk and their power of destroying it by smell are incidentally referred to. In "Science Gossip" a case is mentioned, on the authority of a country clergyman, where a viper followed for some distance a woman who was suckling her infant. Popular tradition in many parts of Europe accuses snakes of fastening on the teats of cows. Now I do not deny that serpents may have an acute scent, as the manner in which the common viper congregates wherever the marsh-rosemary (*Ledum palustre*) flourishes seems to be sufficient affirmative evidence; but in my own somewhat extensive personal experience I never met with any instance of a snake of any kind being attracted by milk, and I should be happy to hear if any of your naturalist readers have in this respect been more successful.—I am, &c.,

SERPENT-HUNTER.

OPTICAL ILLUSIONS—A CORRECTION.

To the Editor of the Monthly Journal of Science.

SIR,—On page 236 of the March number of your Journal a footnote to my article on "Optical Illusions" states that "An account of a very similar observation was communicated by Mr. J. Aitken to the Royal Society of Edinburgh, in November, 1878, apparently without any knowledge of the observations of Addams, Brewster, or of the author of this article." I have received word from my friend Mr. Aitken that, while the observation was

made without any such previous knowledge, references to the older observations were given in the account communicated to the Royal Society of Edinburgh, and Mr. Aitken has courteously permitted me to peruse his paper in confirmation of the fact. I will only add that no mention of previous workers appeared in the abstracts given by the scientific press which came under my notice; and the inference that Mr. Aitken was not aware of the existence of these when his paper was presented to the Society appeared to be confirmed by the title he gave it, describing the phenomenon as "A *New* Variety of Ocular Spectra."

I am happy, however, to make the correction which Mr. Aitken has pointed out to me.—I am, &c.,

S. P. Thompson.

University College, Bristol,
March 13, 1879.

PROCEEDINGS OF SOCIETIES.

Royal Society, *February* 13.—A paper by Mr. J. E. H. Gordon, "On an Extension of the Phenomena discovered by Dr. Kerr, and described by him under the title of 'A New Relation between Electricity and Light,'" was read. In November, 1875, Dr. Kerr announced, in the "Philosophical Magazine," that he had discovered a new relation between electricity and light. He showed that when glass is subjected to an intense electrostatic stress that a strain is produced which causes the glass to act like a crystal upon polarised light. While the author was endeavouring, by means of the electric light, to project the effect of Dr. Kerr's experiment on a screen, the electrostatic stress was accidentally allowed to become strong enough to perforate the glass. Immediately before perforation there occurred the following effects:—First appeared a patch of orange-brown light, about 6 or 7 inches diameter. This at once resolved itself into a series of four or five irregular concentric rings, dark and orange-brown, the outer one perhaps being 14 inches diameter. In about two seconds more these vanished, and were succeeded by a huge black cross about 3 feet across, seen on a faintly luminous ground. The arms of the cross were along the planes of polarisation, and therefore were at 45° to the line of stress. The glass then gave way, and all the phenomena disappeared except the extreme ends of the cross, and the discharge through the hole, where the glass had been perforated, was alone seen.

February 27.—"Studies in Acoustics. I. On the Synthetic Examination of Vowel Sounds." By William Henry Preece and Augustus Stroh. In their investigation the authors assumed that vowels are compounded of a prime sound and certain upper partials. Since each partial can be considered as a simple harmonic curve, if we assume the pitch of a prime to be constant, then it would be possible, by means of a machine, to represent and vary each partial in phase and in amplitude. For this purpose an instrument was constructed, which the authors call "the synthetic curve machine," in which a number of toothed wheels are mounted on steel pins or axes rigidly fixed on a board, so that they will revolve together, and the numbers of their teeth are so calculated that during one revolution of the wheel A, B will make two, C three, D four, E five, F six, G seven, H eight revolutions, and so on. Besides assuming the pitch to be constant, the authors assumed that each octave of the partial, to

maintain equal loudness of sound, must diminish one-half in amplitude as it rises. This instrument enabled the authors to form synthetically all the curves produced by vowel tones, and to show how these tones are compounded of primes and harmonic upper partials. It shows how *simple tones* can be produced by simple harmonic curves, and *compound tones* by the simultaneous action of several simple tones. The authors then determined to try to reproduce the vowels by sounding a prime and one of its partials alone. This was done by means of an electro-magnet vibrating an armature with a movable spring attached to it in such a way that the vibrations of the armature could produce a given prime, while the vibrations of the spring, by varying its length, could also be adjusted to any particular partial. The effect not being by any means perfect, a machine was made on the principle of the synthetic curve machine, which would, instead of drawing curves on paper, reproduce eight partials by transferring the vibrations of the intermediate wheels to a vibrating diaphragm. Here, again, though the vowels were fairly reproduced, something was wanting in their clearness. Another machine was now made upon which disks were fitted, whose peripheries were cut in exact copy of the curve produced by the synthetic curve machine. These curves were transmitted by vibration to the receiving diaphragm of a phonograph, and really formed an "automatic phonograph." With this instrument the sounds most resembling the vowel sounds of the human voice were easily recognised. But although the reproduction of vowels was good, it was imperfect, and it follows from this investigation, as far as it has gone, that vowels cannot be reproduced exactly by mechanical means. Something is always missing—probably the noises due to the rush of air through the teeth, and against the tongue and lips. One very curious result arising from the experiments with the automatic phonograph was to show that, by varying the pitch, the vowel sounds could be shifted,—*i.e.*, the curve which produced *oo* at a low velocity becomes approximately at *O* a higher velocity. *O* similarly becomes *ah*, *ah* becomes *a*, and *a*, *ee*. The curves arrived at synthetically do not differ very materially from those arrived at analytically by Helmholtz; they principally differ in the prominence of the prime. But curves produced by the synthetic machine, compounded of the different partials without their prime, show that there exist *beats* or resultant sounds. A vowel sound of the pitch of the prime may be produced by certain partials alone, without sounding the prime at all. The beat in fact becomes the prime. This point is clearly illustrated, orally, by the automatic phonograph, and graphically by the sketch drawn by the synthetic curve machine. The authors are investigating the true theory of the loudness of sound. They think that loudness does not depend upon amplitude of vibration only, but also upon the quantity of air put into vibration; and, therefore, there exists an absolutely physical unit

in acoustics analogous to the unit of quantity of electricity or quantity of heat, and which may be called the quantity of sound. In commencing their research the authors endeavoured to find a disk for the phonograph and telephone which would vibrate to the finest shades of sonorous vibration. They obtained the best results from a stretched membrane of thin india-rubber, rendered rigid by a cone of paper.

March 6.—" Preliminary Report upon the Comatulæ of the *Challenger* Expedition," by P. Herbert Carpenter, M.A., Assistant Master at Eton College. The collection of *Comatulæ* made by the staff of the *Challenger* includes specimens from forty-five different localities, but few of which are deep-water stations. *Comatulæ* were only obtained seven times from depths exceeding 1000 fathoms. At lesser depths, 200 to 1000 fathoms, *Comatulæ* were met with at thirteen stations; but by far the greatest number, both of species and of individuals, were dredged at depths much less than 200 fathoms, and often less than 20 fathoms, at twenty-six widely-distant stations. At the present time the author regards the collection as containing 111 species, mostly new; but as the work of examination and description progresses it is not unlikely that forms which he now considers different may turn out to be merely local varieties of one and the same species, so that the number given above may be subject to alteration. Of these 111 species, 59 belong to the genus *Antedon*, 48 to *Actinometra*, 1 to *Ophiocrinus*, and 3, which are peculiar in having ten rays to the calyx instead of only five, to a new genus, for which he proposes the name *Promachocrinus* (Greek *Promachos*, " Challenger"). In two of the species the rays are undivided, as in *Ophiocrinus;* but in the third they divide, as in our common *Antedon rosacea*, so that there are twenty arms. The voyage of the *Challenger* has, according to the author, settled two curious questions in connexion with the Crinoids, the origin of which is due to Lovèn. They refer to *Hyponome Sarsii*, a so-called recent Cystid, and to *Phanogenia*, a supposed new genus of the Comatulidæ. *Hyponome* turns out to be nothing more than the disk of a *Comatula*, minus its skeleton. The stellate condition of the centrodorsal in *Phanogenia* has long been a puzzle to the author, but the material brought home throws a considerable light upon it. This condition appears to be one of the concluding stages of a long series of changes in the shape and relations of the centrodorsal, which do not commence until some time after the loss of the stem, and the entry upon the free state of existence. The dredgings in Torres Straits brought up a considerable number of specimens of a hitherto undescribed *Comatula*. The author proposes to name it *Actinometra Jukesii*. The examination of the *Challenger* Comatulæ has entirely confirmed the opinions held by Dr. Lütken and the author respecting the distinguishing characters of *Antedon* and

Actinometra. They both agree in referring forms with a (sub) central mouth, five equal ambulacra, and no terminal comb on the oral pinnules, to *Antedon.* On the other hand, species with an eccentric mouth, a variable number of unequal ambulacra, and a terminal comb to the oral pinnules, belong to *Actinometra.*

Chemical Society, *February* 6. — Dr. J. H. Gladstone, F.R.S., President, in the chair.

Dr. Frankland opened a discussion on a paper read by Dr. C. M. Tidy on December 6th, 1878, "On the Processes for Determining the Organic Purity of Potable Waters." In the course of a long address Dr. Frankland said that he entirely dissented from Dr. Tidy's suggestion that non-volatile matter may be removed *mechanically* during evaporation. He had proved by actual experiments that mechanical removal only took place during violent agitation or the breaking of gas bubbles at the surface of the liquid. He was still more astonished, however, to find Dr. Tidy, who is a physician as well as a chemist, suggesting that the poisonous constituents of sewage may be volatile. All the knowledge we have hitherto acquired about the infectious matter of epidemic disease pointed, he said, to the inevitable conclusion that the propagating material is not merely organic, but organised; and that its virulence, unlike that of arsenic or other similar poisons, resides in its vitality and its power of multiplying itself almost indefinitely in the human body. An organised liquid, gas, or vapour, was a physical impossibility, and hence there is no foundation whatever for the opinion that the poison of sewage is volatile, or that it is not contained in the residue left on evaporation. With regard to the albuminoid ammonia process, Dr. Frankland's experience coincided with that of Dr. Tidy. It had been put forward as capable of answering the question, "Is the water wholesome or is it not?" It did not, however, answer this question: It condemned water containing only peaty matter, and it acquitted water containing urea and uric acid? Could anybody believe that in 1876 the Chelsea Company's water, drawn from the Thames, was "safe" in January, "dirty" in March and August, "safe" again in November, and "of extraordinary purity" in December, when it contained no less than 0·423 part of organic elements in 100,000 parts of water; and when three other samples drawn from the same source were denounced as "dirty" by the same process. Again in 1871, whilst the West Middlesex Company's water was "of extraordinary purity" in January, April, and May, the Chelsea Company's water was "dirty" in those months, although quite "safe" in December. Floods undoubtedly make Thames water dirty, but the albuminoid ammonia curve pursued the even tenor of its way quite regardless of them. Dr. Frankland cordially agreed with Dr. Tidy's strictures on superficial and rapid analyses of waters, in which

the determinations were chosen with far greater regard to the analyst's time than to the needs of the inquiry. The so-called partial analyses were often very partial, and often brought disgrace upon the profession of chemistry. In conclusion, he congratulated the author on his valuable addition to the literature of water analysis, and on the clearness with which he had brought a great array of facts before the Society. His comparative tables were of great value, and he trusted they would be carefully studied by all chemists who practise water analysis. They formed a chart upon which the rocks so fatal to many a water analysis were so clearly shown that he who runs might read.

Mr. Wanklyn said that as long ago as 1867 Chapman, Smith, and himself directed attention to the cardinal defect of the combustion process, and this defect had never been overcome. It was this, that the organic matter in the water does not survive the evaporation to dryness. Dr. Frankland said that he would be content if he had a process to burn up the organic matter in the water itself. Messrs. Cooper and Wanklyn had invented such a process, and had burned up substances in solution.

Dr. Voelcker said that an impartial observer would come to the conclusion that all methods were more or less defective, and some gave very erroneous results. He would most earnestly urge the importance of determining all the constituents of a water, organic and inorganic, and not founding an opinion on one factor.

In his reply Prof. Tidy said he had concluded that the poison in water was volatile from analogy, because when it did act it was very virulent, and virulency and volatility usually go together.

Edinburgh University Chemical Society, *January* 29.—Alexander Macfarlane, D.Sc., F.R.S.E., in the chair.

A paper was read by Mr. J. S. Thomson, "On Paraffin and what is got from it, as illustrated by 'Exhibits at the Paris Exhibition of 1878.'" A full account was given of the modes of preparation of these exhibits, which consisted of specimens of all the commercial products of the Addiewell Chemical Works. These included naphtha, burning oils, lubricating oils, solid paraffin in blocks (weighing not less than 7 cwts. each), sulphate of ammonia, and candles of various kinds made from the paraffin wax. The methods of preparation of other products of paraffin were also explained.

Physical Society, *February* 22, 1879.—Prof. W. G. Adams, President, in the chair.

Dr. Schuster gave the results of some observations made by a spectroscope with two prisms, one for the red and the other for the blue end of the spectrum, on the Spectrum of Lightning. Three observations were made, one at Las Ammas, one at Maniton, and one at Salt Lake City, last year. These showed the

three nitrogen lines, with three well-defined bands and one doubtful band. The nitrogen lines correspond to the spectrum of air, and the bands appear to Dr. Schuster to agree with the spectrum of the light round the negative pole of the spark in a tube containing oxygen with adulteration of carbonic oxide.

Prof. Ayrton exhibited an Exisothermal Model of a Cooling Globe. If we imagine a globe initially heated throughout to a uniform temperature (as was probably the earth), and then kept in a space having a constant temperature, but much lower than that to which the globe was heated, then the temperature at every point of the ball will fall, but at very different rates; the parts, for example, near the surface cooling comparatively rapidly, while those near the centre will cool very slowly. A surface could, therefore, be constructed, such that the x of any point on this surface represented its distance from the centre of the globe, the y the time t from the commencement of the cooling, and the z the temperature of that point at that moment. The nature of this surface would depend on the size of the globe, on the specific heat, conductivity, and surface emissivity of the material. The "Experiments on the Heat Conductivity in Stone" of Profs. Ayrton and Perry, described in the "Philosophical Magazine" for April, 1878, enables them to determine these constants accurately for a trachyte sphere, and by using these data they have been enabled subsequently to construct such a surface, called by them an "exisothermal" one for a trachyte globe of 8000 miles in diameter, and which gives graphically the temperature of every single point of the earth from the moment when it was at the temperature of molten trachyte down to eight hundred thousand million years afterwards,—that is, until long after the present era.

March 8.—Prof. Ayrton brought forward a new theory of terrestrial magnetism, originated by himself and Prof. Perry, of the Imperial Engineering College, Japan. (See p. 287.)

Mr. F. D. Brown described his apparatus for maintaining constant temperatures and pressures. A constant temperature can be obtained if the pressure can be kept constant. The vessel in which the constant pressure is desired communicates with an air-pump by a pipe in which a movable tap or valve is placed. By opening or closing this tap the pressure is regulated. This is effected by an electric clutch arrangement. A mercury anemometer sends a positive or negative current from a battery through the clutch according as the pressure is too high or low, and this current actuates the clutch to close or open the valve. The clutch consists of an axle driven by a turbine to get power to work the valve, and the current, by means of electro-magnetism, connects the tap to the axle, which then opens or closes it as the case may be. In this way a pressure varying no more than one-fifth millimetre each way can be obtained.

Manchester Literary and Philosophical Society, *February* 18.—J. P. Joule, D.C.L., LL.D., F.R.S., &c., President, in the chair.

Professor Roscoe, LL.D., F.R.S., communicated a paper by Sadamu Ishimatsu, "On a Chemical Investigation of Japanese Lacquer, or 'Urushi.'" During a few months last year the author had the opportunity of examining roughly into the nature of "Urushi," in the Laboratory of Tokio University. The specimen of lacquer which he had under his examination was obtained from Kuyemon Nakamuraya, in Tokio, a large lacquer merchant. It is a milky juice of pale grey colour, and gives out a certain kind of poisonous volatile gas. Some persons are terribly attacked by this poison, producing a great swelling where the acid comes in contact. During the author's examination in the laboratory one of the apparatus keepers was terribly attacked by this gas, producing ugly swellings all over the face. By using the solution of chloride of sodium, carbonate of soda, acetate of lead, &c., he was said to have recovered within a week. This poison, however, acts only on certain persons, for the author while working with it never felt any uneasiness from it. The blackening of lacquer in air is by many supposed to be due to the combined action of light and air, but from the following experiments this seems to be erroneous:—1. A square box was made which had a well-fitting sliding door, and the inside of which was made perfectly black, so that practically no light was permitted to enter. In it was placed a small quantity of lacquer at dark, and the door closely shut. On looking at it the next morning it was observed that the lacquer had turned perfectly black, proving that it is not light that blackens the lacquer. 2. The bottle in which the specimen of lacquer was kept for more than three months was exposed to the incident light of the laboratory. The surface of the lacquer was turned perfectly black, forming a wall as it were; while those portions which were in contact with the sides of the bottle, which receive as much light as if there were not any glass sides before it, were not at all blackened. This phenomenon proves that the blackening in the atmosphere is in all probability due to the oxygen of the air, but not to light alone, nor to the combined action of light and air. The examination showed that the "Urushi" consists of three principal constituents: (1) a resinous part soluble in alcohol, (2) gum, and (3) residue. The resinous part soluble in alcohol seems to be the principal portion: it has a smell like ordinary lacquer, but it never dries as the original does. It is brownish black, and slightly sticky to the touch. When treated with potash solution it forms a bluish black precipitate, but nothing is obtained on addition of sulphuric acid to the filtrate. When boiled with hydrochloric acid the resin is transformed into a substance elastic while hot, something like the mass obtained when heated sulphur is dropped into cold water. When boiled with

nitric acid nitrous fumes are given out, and the mass gradually becomes yellow, and finally a beautiful orange-coloured mass was obtained. The mass was to a great extent soluble in absolute alcohol, leaving behind a small quantity of a yellowish body. A quantity of alcoholic extract was precipitated with acetate of lead, the precipitate was thoroughly washed with absolute alcohol, aad then decomposed by means of dilute sulphuric acid. The mass was dissolved again in absolute alcohol, then separated from sulphate of lead. This separated alcoholic extract was again precipitated by sugar of lead, and, after filtering and washing, the precipitate was dried partially in an air-bath, and carried under the receiver of an air-pump and dried over sulphuric acid. This lead salt exploded when heated. The amount of lead was estimated as oxide by igniting it with nitric acid, and the salt was subjected to organic combustion. Nitrogen was determined by Dumas's method. The following numbers were obtained as the mean results :—

Carbon	26·93
Hydrogen	4·11
NO_2	18·44
PbO	47·42
Oxygen	3·10
	100·00

The author then took alcoholic extract of the original lacquer and precipitated it with acetate of lead. After requisite purification and drying the precipitate, it was analysed, the lead being determined as before. The following is the mean of two experiments which were tried :—

Carbon	50·450
Hydrogen	5·705
PbO	3·775
Oxygen	40·070
	100·000

The gum is soluble in cold as well as in warm water. It has no smell, almost no taste; it has a yellowish, or rather brownish, colour, and is of a non-crystalline body. It is quite insoluble in alcohol. On subjecting this substance to organic analysis the following percentage of hydrogen, carbon, and oxygen was obtained :—

Carbon	41·20	41·45
Hydrogen	6·51	6·58
Oxygen	52·29	51·97
	100·00	100·00

These analyses yield a formula approximating to the composition of common gum. The residue is, the author thinks, nothing more than the mixture of cellulose, bark, dust, &c.

NOTES.

Agriculture.

From a paper by A. Johnen, in "Biedermann's Central-blatt" for January, 1879, "On Comparative Observations of Rainfall according to Fautrat's Method," we learn that the quantity of rain in a beech-wood is 13 per cent greater than in the open country. In a pine-wood the excess was only 2 per cent.

From manure experiments on the Agricultural Trial-field of the University of Giessen, Prof. A. Thaer finds that the quantity of nitrogen given to a selected "morgen" of land in the form of manures during seven years amounted to 89·5 lbs. The crops reared during the same years contained together 198·69 lbs. of nitrogen. Consequently the soil had given 109·19 lbs. nitrogen more than it had received. Hence, while the manures furnished 45 per cent of the nitrogen in the crops, the atmosphere had yielded 55 per cent.

According to Dr. E. Wein, Prof. J. Nessler, Prof. A. Mayer, and Prof. R. Heinrichs, the composition of an English cattle food is—

	Wein.	Nessler.	Mayer.
Moisture	13·72	—	12·9
Albuminoids	11·75	13·21	9·4
Oily matter	4·28	4·24	3·6
Non-azotised extractive	67·24	—	66·9
Vegetable fibre ...		—	3·6
Ash	3·01	—	3·6

Wein and Mayer, from chemical and microscopical evidence, conclude that the food is composed of equal parts of bruised maize and locust-beans, and that the essential oil of a plant of the umbelliferous order has been added. Another English cattle food in the form of cake contained also a large proportion of locust-beans.

Biology.

Mr. H. W. Bates, in his Presidential Address to the Entomological Society, refers to some unexpected facts in the geographical distribution of insects. Thus the insect fauna of New Zealand is quite distinct from that of Australia, and its origin will therefore have to be accounted for by a totally distinct set of causes. The Carabidæ of Australia have often a near but very peculiar relationship to forms of the same family in the Palæarctic region. Next to that great region, indeed, Australia is the richest part of the world in species and genera of the

Feronia group—a circumstance which ten years ago would have been deemed most unlikely.

Mr. J. W. Slater, writing in the "Entomologist," shows that the larvæ of gnats, midges, &c., instead of being sanitary agents, intensify the putrefaction of organic refuse suspended in water.

At a recent meeting of the Biological Society of Paris, M. Bert described the results of his experiments on the temperature of the brain. In a state of absolute inaction, bodily and mental, the temperature of both sides of the head is equal; but as soon as any brain-work is undertaken, if the equality does not continue, the left side always shows the higher temperature. According to M. Raymond, in cases of paralysis the temperature is higher in the parietal region than over the rest of the cranium; and here, again, the left side is found hotter than the right by 0·3 to 0·4 of a degree C.

At another meeting of this Society (March 1st) Dr. P. Bert described experiments made to elucidate the influence of the cerebral hemispheres upon the capillary circulation. One hemisphere of the brain of two axolots, a black and an albino respectively, and also of a bat, was removed. No modification of the capillary circulation resulted. There was produced, however, a bilateral paralysis of the chromotaphores of the skin, and the animal lost the power of changing its colour under the influence of emotions.

At a meeting of the Bordeaux Société des Sciences Physiques et Naturelles, Dr. Azam gave an account of an interesting case of double consciousness and scission of personality which has been under his observation for many years, and which has been described in the "Comptes Rendus de l'Academie des Sciences Morales." The patient, Felida X——, has, as it were, two distinct lives, two conditions separated from each other by a short sleep or a torpor of a few seconds. After her return to the normal state she is entirely unconscious of what has taken place during the abnormal condition, whatever its duration. She is more cheerful during this abnormal state than in the normal condition. When in the abnormal state she remembers, however, anything which has happened in her normal intervals. The author, in accordance with M. de Quatrefages, considers that the mnemonic cellules are disseminated throughout the brain. He formerly thought that they were seated in the right hemisphere, because the faculty of articulate speech, which resides in the left hemisphere, is never affected in Felida. This opinion is difficult to maintain, since the faculty of articulate speech cannot act without memory.

From a paper read before the Shanghai branch of the Royal Asiatic Society, by M. A. A. Fauvel, it appears that a true alligator—as distinct from the crocodile—has been found in China. Hitherto the alligator has been supposed peculiar to the Western

Hemisphere. The name proposed for the species is *Alligator Sinensis.*

Dr. A. Voisin finds that the brains of idiots are structurally little developed, remaining in a fœtal condition, or at most in a state like that of early childhood. The convolutions are abnormally simple, and present only a few nucleoli.

Mr. W. H. Edwards, in the "Canadian Entomologist," records the singular fact that ants cherish the caterpillar of *Lycœna pseudargiolus* (a small butterfly closely resembling the "blues" of English collectors), for the sake of a clear, green fluid, doubtless of a saccharine nature, which exudes from a peculiar mamilloid organ on the back of the eleventh segment, The most perfect understanding seems to prevail between the ants and the caterpillars, which must thus rank with the *Aphides* among the domestic cattle kept by our six-footed rivals.

Chemistry and Technology.

In a note on large crystal growing Mr. C. W. Quin remarks that a great difficulty is to guard against sudden rises in temperature, which generally have the effect of causing the growing crystal to be partially and unevenly re-dissolved. To receive a continually even temperature, after many experiments he hit upon the plan of plunging the beaker containing the growing crystal into the house-cistern, the temperature of the water in which never differed by more than 0·5° F. either way day or night. To prevent the solution from becoming exhausted too rapidly he immersed in it to the depth of half an inch or so a crystal drainer containing a filter-paper full of the salt to be crystallised, so that a constant stream of strong solution was continually descending on the growing crystal.

The double staining of vegetable tissues, various processes for which have been noted under "Microscopy" from time to time in the "Journal of Science," has proved eminently successful in rendering structural details more evident. Those portions consisting of tissues hardened by much secondary deposit absorb the dye with avidity, and in the washing process part with it but slowly, so that the simpler tissues are deprived entirely of their colour before the hard parts yield up theirs; careful staining with another dye follows, and the effects of differential colouring are very apparent. Some operators have used three and four colours with great success.

M. G. de Plessis* recommends bichromate of potash for the preservation of delicate marine organisms, such as polypes, *Medusæ*, *Salpæ*, &c. The solution can be made in either fresh or salt water, and a weak solution answers well, exact proportions

* Bulletin de la Société Vaudoise des Sciences Naturelles, sér 2, vol. xv., pp. 278—280, April, 1878.

being of but little moment. A specimen of *Medusa aurita* has kept well a whole year, and its beauty and transparency leave nothing to be desired. The solution has only one inconvenience, it permits the development of mould; but this can be prevented by the addition of a few drops of phenic acid or phenic alcohol. Another convenient salt is permanganate of potash, acting very much like osmic acid, clearing away the protoplasm and bringing out the minutiæ and showing nuclei, outlines of cells, &c. It can be dissolved in sea-water, and should be used as a saturated solution either in sea or fresh water. This solution kills small organisms at once. They are left in it from thirty minutes to an hour, then withdrawn and placed in alcohol, after which they can be made transparent with turpentine and mounted in Canada balsam. According to the author it effectually replaces the costly and somewhat dangerous osmic acid.

We learn from M. Reimann's "Färber Zeitung" that two new green dyes have been introduced into practical use; the *Vert acide* of A. Poirrier, of Paris, and the Victoria green of the Baden Aniline Company. The price of the latter is only 16s. per kilo.

The red azo-colouring matters are fast superseding cochineal in the flannel dye-works of Saxony. They have the advantage of not being injured by washing.

M. Aimé Girard has reported to the Société d'Encouragement pour l'Industrie Nationale on M. Kuhlmann's method for recognising the composition and measuring the volume of the gases and acid vapours given off by the chimneys of chemical works. By means of this arrangement a manufacturer can at any hour inform himself concerning the nature of the gases traversing his chimney. To establish the composition of the chimney gases and to detect, *e.g.*, the presence of hydrochloric acid therein, M. Kuhlmann introduces into the shaft a glass tube which communicates with a series of test-tubes, at the end of which is a cistern of water which acts as a respirator. The flow of water compels the gas to traverse the series of test-tubes, the first of which contains caustic soda coloured with litmus. Then follow solutions of barium chloride and silver nitrate. The dimensions of the cistern are such that five or six hours are required for the total efflux of the water which it contains, and the strength of the alkaline liquid coloured blue is such that in the regular course of work five or six hours are required to turn them red. An earlier colouration gives immediate warning. M. Kuhlmann's method of measuring the total volume of gases escaping is as follows:—When he wishes to know the speed of the gases he introduces at the bottom of the chimney a certain volume of a strongly coloured gas, such as hyponitric acid, which accompanies the colourless gases up the chimney and presents itself along with them at the top. Knowing then the volume of the chimney and the mean temperature it is possible to calculate from the time which has elapsed the quantity of gas emitted.

Artificial benzoic acid is at present manufactured in large quantities from the naphthalin that is abundantly found as a by-product in the gas works.

The efficiency of creosote as a preservative of timber against the ravages of the teredo has, remarks the "Engineering and Mining Journal," been signally established by the recently-published report of a commission appointed by the Royal Academy of Sciences of Amsterdam to make a thorough investigation of the subject. This commission selected several of the ports of Holland in which to conduct their experimental trials, and subjected to their tests oak, red-fir, common fir, and pine, in pieces about 3 feet long and 12 inches square. They found that all external applications, such as coal-tar, paraffin varnish, mixtures of tar, resin, sulphur, &c., were absolutely worthless. Mechanical protection, by the use of nails, was found to be only partially efficacious, and, with the use of copper or iron sheathing, too expensive for ordinary use. Impregnation with various materials was also fully experimented upon, and, as against this element of destruction, with the single exception of creosote oil, all proved to be either completely or almost worthless. In this category are named sulphate of copper, copperas, acetate of lead, salts of mercury and arsenic, soluble glass, chloride of calcium, and paraffin oil. Only the blocks treated with creosote oil were found at the end of two years to have remained uninjured. The commission thereupon draw the conclusion that the only effective preservative agent for timber against the depredations of the marine boring-worms is creosote properly applied and of good quality.

According to C. Fürstenau in the "Chemiker Zeitung" the Austrian ultramarine manufacturers sophisticate their colours with three parts of gypsum, and lest the colour should appear too pale it is further mixed with glycerin or glucose or a mixture of both. This keeps the powder damp and renders the colour apparently deeper.

Great industrial activity is being displayed by the German Distiller's Association. At a general meeting it was resolved to establish and maintain a chemical laboratory, an experimental distillery, a school of distilling, a trade journal, a glass-blowing establishment for the manufacture of normal hydrometers, and an office for general intelligence.

It is stated in "La Lancette Belge" that Mr. Edison has invented an ink which gives raised characters upon paper capable of being read by the blind by touch.

The formation of mannite in beer is always a mark of the want of cleanliness and is particularly promoted by decaying wood. In such cases the sugar present enters not into the alcoholic but the mucic fermentation.

Dr. Upmann, in "Chemiker Zeitung," in consequence of some experiments pronounces oxalic acid to have a harmless effect on dogs. Dr. Pfeiffer, in reply, points out that the stomach and intestines of dogs generally contain considerable quantities of calcium phosphate by which the oxalic acid would be converted into calcium oxalate, an inoffensive salt.

Geology.

M. W. H. Hermite, in a paper read before the Academy of Sciences, concludes that the form of the earth is due not to a previous condition of igneous fluidity, but mainly to the configuration of its seas. The remarkable polar flattening of Mars is due, according to his hypothesis, to the different distribution of land and water upon the surface of that planet. Volcanic phenomena he finds also incapable of being referred to the existence of a central fire, or even to the presence of a sea of lava of small extent.

The First Part of Vol. vii. of the "Proceedings of the Yorkshire Geological and Polytechnic Society" contains a number of useful papers. Mr. Sorby gives an account of his new method of studying the optical characters of crystals by looking through them with a microscope at a circular hole or rectangular grating. The Rev. J. F. Blake communicates a paper on the geological history of East Yorkshire. The Rev. E. M. Cole describes the red chalk of the East Riding, Lincolnshire, and Norfolk, which lies at the base of the white chalk and is free from flints. Mr. J. W. Davis, the indefatigable Secretary of the Society and Editor of the "Proceedings," contributes an account of the fish-remains found in the coal measures and the evidence which they afford of fresh-water origin. He shows that the coal was deposited in a series of pools, into which the vegetable matter was carried whose decayed remains form the coal, and when mixed with the mud simultaneously brought down the "hubb," or black bituminous shale. In this, as well as in the coal, large numbers of Cœlacanths have been discovered, as well as Ganoids and Elasmobranchs, though much more sparingly.

We learn from the "Engineering and Mining Journal" that a resolution of thanks to Prof. Hayden for his "accurate and comprehensive survey of the State of Colorado," passed the Legislature of Colorado on January 14th, and upon which Senator Gaussion remarked: "These reports, coming from a scientific and authoritative source, do more to answer and satisfy the inquiries of capitalists than every thing else. They tell the world what the great Western country is made of. The Western domain of the United States is, to-day, the glory of the nation."

Metallurgy.

In a paper "On the Relations between the Chemical Composition and the Mechanical Properties of Steels" (Bull. de la Soc

Chem. de Paris), M. J. Deshayes says that carbon renders steels rigid and elastic, increasing their elastic tension, but their resistance to rupture diminishes if 0·500 is exceeded. Manganese renders steels rigid and elastic, and increases their elastic tension, but the elongation and contraction remain considerable, which gives a good resistance to a shock. Silicon plays the same part as carbon, rendering steels hard, and slightly diminishing elongation. Sulphur decreases the breaking strain and the resistance to a shock. Phosphorus renders steels deficient in body, and, if its proportion exceeds 0·250 per cent, fragile on receiving a shock. Chrome acts like manganese, but more energetically.

According to the "Revue Industrielle" a metal has been extensively produced in France which is a combination of iron and steel. The two metals are run separately into a mould divided into two parts by a thin plate of sheet iron, when the whole becomes welded together. The new metal is particularly adapted for armour-plating, anchors, and safes.

Physics.

The Committee appointed to receive subscriptions to present a bust of Mr. William Spottiswoode, Pres. R.S., to the Royal Institution as a testimonial of his valuable services as its Treasurer and Secretary successively, have engaged Mr. Richard Belt as the sculptor.

On behalf of the Committee of Economical Arts, M. le Compte du Moncel has presented to the Société d'Encouragement pour l'Industrie Nationale a report on M. E. Regnier's electric light regulator. The Committee decide that M. Regnier has solved the problem in question. His arrangement has been successfully tried at the station of the Northern Railway, and in M. Breguet's workshop, where it has acted for some hours with regularity under the influence of the current of a small Gramme machine.

A remarkable case of cohesion, or the welding of two metals at a temperature far below the melting-point of either of them, has lately been noticed by Mr. Charles A. Fawsett, of Glasgow, and by him reported to Sir William Thomson. If a piece of silver, 1 centimetre square, is heated on the inverted lid of a porcelain crucible, to about the temperature of 500° C. (932° F.), and the end of a thin platinum wire is brought into contact with it, the two metals will be found to have welded to that extent that the silver may be raised from the lid, and will remain attached to the platinum wire when cooled off, Mr. Fawsett reports that other metals—copper and aluminium, for example—will likewise adhere to silver, though the experiment is less striking than in the case of platinum.

THE MONTHLY

JOURNAL OF SCIENCE.

MAY, 1879.

I. THE SPIRITUAL IN ANIMALS.

CERTAIN writers, who have given up as untenable every other alleged ground of distinction between man and his "poor relations," take their final stand on the religious sentiment—on the recognition of a God and the belief in a future state: this, in their opinion, forms the final and insurmountable barrier. Before we can be expected to admit this assumed distinction it must be shown to our satisfaction, first, that all normal men are spontaneously worshippers; and, secondly, that all other animals are incapable of religious emotions. All other animals, we repeat; for if in any one species there can be found a trace of the "God-idea," then the boundary-line—whatever it may be worth—will run in an unwelcome direction, including that one species together with man, whilst excluding all the rest of the animal creation. Now where is the evidence that "brutes" are destitute of the religious sentiment? Simply nowhere. Who will dare to say that he has analysed all the emotions that pass through the mind of every animal? that he has looked upon the world with its eyes? Who can even guarantee that none of the actions of brutes have a religious significance? If we reflect how strangely varied are the devotional rites even of our own species, should we not pause before authoritatively deciding what must be the worship of other beings? The whole matter rests upon assumption. Man has complacently taken for granted that a "mere brute" must of necessity be without any trace of religion, and then, arguing in a circle, has elevated this imagined incapability of religion into a boundary-line between man and "mere brutes." We cannot recognise the validity of such reasoning. Lord Herbert of Cherbury,

John Wesley, Meinhold, Quatrefages, and their followers, will possibly claim the right to assume the absence of the religious feeling in any animal till its presence shall have been formally demonstrated. We deny their claim. Before any attribute can be selected as distinguishing one body from another, its presence in the former and its absence in the latter must both be equally proved, not merely guessed. That the lower animals build churches and chapels, maintain a clergy, get up revival meetings, or distribute tracts, no one contends. But have they no dim but overshadowing consciousness of an Unseen Power behind, and above the visible and the tangible? Does not anything to them unwonted and unintelligible inspire them at times with awe, as contradistinguished from fear? Have they not—especially such as have been brought into human society—some vague idea of the last great change? Do we not find among them, in short, many indications of that lowest stage of religion—whether incipient or degraded it is not our business to inquire—which it is now the fashion to call fetichism? To us the cases lately observed and recorded by Mr. J. G. Romanes and others seem strongly to favour this view.

But it may again be doubted whether man gives everywhere more decided proofs of religious emotions. Unless this also is shown to be a rule without exception, the proposed boundary-line must again be declared at fault. To take only one instance: the Veddahs of Ceylon, as we are informed on good authority,* "have no knowledge of a God or of a future state; no temples, idols, prayers, or charms; no instincts of worship." Are they, therefore, men or brutes?

A kindred question here presents itself. Personal immortality—a second and prolonged, or rather never-ending, life—is claimed as a characteristic attribute of their own race by all the more cultivated portion of mankind, with the exception of a small minority of so-called materialists,† and is almost with one accord denied to brutes.

Many estimable persons will therefore be shocked at the declaration of the late Prof. Agassiz—himself certainly no materialist—that "most of the arguments for human immortality apply to other animals quite as forcibly." Take,

* TENNENT, Ceylon, vol. ii., p. 442.

† An unhappily-chosen term. A materialist, correctly speaking, is one who in opposition to the idealist ascribes to matter substantive existence independent of the perceiving mind. In common usage it is applied to one who denies the existence of spiritual beings, and who ought rather to be styled an "apneumatist."

for example, the doctrine of compensation, so much insisted on in the case of man. If a "land of the leal" hereafter be needed to atone for the inequalities of the present human life, the same plea may be urged also on behalf of the lower animals. How widely different, for example, may be the respective lots of horses. One may first see the light in a ducal stable, more comfortable than many a human dwelling; he is reared and trained with the utmost care and consideration, and fed on the choicest food suitable to his tastes; he becomes the pet of some lady of rank, and knows no harder task than to carry his mistress along Rotten Row. Another, from no fault of his own, is foaled in some rude shed, grows up amidst hunger, cruelty, and neglect, and before his bones and muscles are matured is doomed to drag a costermonger's cart—a similar fate to that which modern industrialism has prepared for the limbs and competitive examination for the brains of the young of our own species. Are not the respective destinies of these two horses almost as unequal as those of the financier and the farm-labourer?

It may perhaps be contended that such inequalities are due not to Nature, but to man's interference. Let us, therefore, take an animal species not under special or direct human influence. Suppose any common female butterfly—say a *Vanessa Atalanta*—lays one hundred eggs. A portion of these never come to life at all, or are destroyed by a variety of causes before hatching. The remainder become caterpillars in due course, but very few ever reach maturity. Some are devoured by birds; others are pierced by ichneumons, and slowly consumed by parasitical larvæ. A few pass safely through their early stages of being, and come forth as butterflies. But here again one in the very outset of his career may perish in a spider's web; another may encounter its fate in the net of a collector, whilst the birds will not fail to secure an ample share. A very few only of the original hundred will live out their allotted span, find mates, and die finally a natural death. Is not here, also, a fair case made out for a hereafter to balance the capricious inequalities of the present?

In like manner we may deal with the other arguments for human immortality, and show their applicability to the lower animals. Thus orators and essayists love to enlarge on the unsatisfactory nature of human life. It is, they tell us, vain, empty, and incapable of filling our desires—a discovery which by the way is never made until health or strength, or at any rate elasticity and animal spirits, are

declining. This, rightly or wrongly, is taken as a proof that we were intended for a higher destiny than this world can offer. But the lower animals likewise become life-weary; the gaiety and buoyancy of their earlier days vanishes; they, too, feel inclined to exclaim with the poet—

> "Life is had,
> And then we sigh and say, can this be all?"

The melancholy which all animals experience in the evening of their day is fundamentally the same. From the *vanitas vanitatum* of Solomon to the sadness of "a gib-cat, or a lugged bear, or an old lion," there is one continued series of phenomena, differing in degree, but alike in kind and in cause.

Have the lower animals, then, no right to say that life has palled on their taste—that this world does not satisfy their cravings? It is possible, indeed, that if a draught of some magic elixir could restore to them all they have left behind in the flight of years,—their youthful vigour and their flow of spirits,—this melancholy would disappear. But would not a similar change come over man?

Again we proclaim that our being is in part spiritual, on the very satisfactory ground that mere matter cannot be supposed to think, remember, reason, or even see or hear, and that spirit, being of its own essence indestructible, our immortality necessarily follows.* Be it so: the lower animals also see and hear, think, and remember. On our own showing, then, their nature, like our own, is partly spiritual, and like ourselves—if spirit is essentially indestructible—they may look forward to a future life. What that life may be for them we have not the faintest knowledge. It may have relations with that which awaits ourselves, or it may be totally distinct. We question if the more highly organised among them would find the prospect of annihilation satisfactory. To a transmigration of souls they would also have good right to object. To be perhaps born again as men of the nineteenth century—heirs to "competitive examination, sewage irrigation, and international arbitration," with the woman's rights movement

* A strange writer in a somewhat heterodox journal, whilst assigning souls to animals, plants, and, if we understand him rightly, to minerals, and whilst fully admitting the immortality of the spiritual part of man, does not consider such part as indivisible. On the contrary, he maintains that the soul of every animal is an offset from that of its male parent. Every spermatozooid—or, as he holds, spermatozoon—contains a portion of the soul of the animal by which it is secreted. The female parent contributes merely the material part of the young animal. How all these points were ascertained it does not appear.

looming in the future—they might, perhaps, feel it preferable to sleep on for ever beneath the shade of their good green woods.

Will the future existence, either of man or brute, be more than merely linear, as at present? Who has not felt, at times, a craving for multipresence—a wish to see and enjoy at once what in this life we can only behold successively? As with sight, so also with all the other faculties of our being. Shall we be always restricted to the exercise of one only function at a time? Must the intellectual, the esthetic, the sensuous, and the moral phases of our being always merely relieve each other on duty? Of all the many limitations of our earth-life none is more irksome than this. Many a time when on a journey we have repined that we could not stay at some beautiful spot, and at the same time go on to other scenes not less lovely. The burden of a strange old song rings continually in our ears:—

> "For say, O why must the whitethorn fade
> Before the rose can bloom."

A bye-question here presents itself. The belief in ghosts—very widely spread among the human species—is frequently brought forward as a corroboration of the existence of unseen, probably immaterial, beings, and hence of the being of God, with whom man, in virtue of his spiritual nature, has an affinity. But in numerous accounts of the alleged appearances of ghosts, and indeed of phenomena not readily to be explained by known physical laws, we find it stated that dogs and other animals were seized with the utmost terror.* Hence it is but fair to argue that if these narratives contain an element of truth, then dogs, as well as men, have an affinity with the spiritual world, and are able to recognise the supernatural. If these stories are mere inventions, exaggerations, or distortions, or if the phenomena though faithfully reported admit of "natural" explanation, then they prove nothing as regards man.

* ENNEMOSER, History of Magic (HOWITT's version), pp. 355, 362, and 395.

II. ANCIENT GLACIER ACTION IN THE PUNJAB:

WITH SPECIAL REFERENCE TO MR. MATTIEU WILLIAMS'S THEORY.

By Major H. A. Tracey, Royal Artillery,
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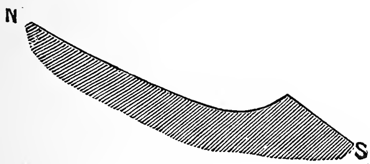
To enable my readers to trace on the map the country alluded to in the following lines, I would remind those who have not hitherto had their attention called to it that from Calcutta, on India's eastern border, to Peshawur, on its N. W. frontier, runs a road whose solid construction, bridges, and embankments rival Rome's magnificent works of the same kind. This Imperial monument of civilisation is about 1700 miles in length, and I, who have travelled more than 1000 miles along it, bear witness that it is but one of many of the grand Imperial highways of India. About 170 miles from the Peshawur end of this road lies the city of Rawul Pindi, and there branches off a road due north towards Cashmir, which in about 50 miles reaches the Cashmir frontier, just beyond the town of Murree.

I was quartered to the N.W. of Murree on a ridge whose geographical position is remarkable, and so narrow that at the part occupied by my hut I could throw out a tumblerful of water that would drain into the Indus from my N.W. windows, while from my opposite window the ground fell away and drained into the Jhelum. I was, in fact, on part of the actual water-shed that the Murree hills form between these rivers. It was on shaly rock we were situated, with occasional bold limestone bluffs. Our height above the sea was 8400 feet. In the autumn of 1876 I had marched from my eyrie beyond Murree to Rawul Pindi, and in the spring of 1877 was in camp at a spot 12 miles due south of it. I was then about 60 miles due south of Murree, but no longer in the "hills," being only about 1200 feet above the sea. While in camp there I received the number for April, 1877, of the "Quarterly Journal of Science," and read the article by Mr. Mattieu Williams, on the "Great Ice Age and Origin of the Till." I only wished I could have offered the author an Arab hospitality, and seated him at my tent-door, when I would have recounted to him some particulars of

my route from Kyra Gully to the spot I was on that I think would have interested him. For instance, first day's march of only 8 miles:—Leave Kyra Gully 8400 feet; descend 1500 feet; ascend to Murree camping-ground, 7600 feet. Shale nearly the whole way, but limestone cropping out here and there. At Murree we got on to sandstone. Next day to Trete, 4000 feet, where the road and ground in section N. and S., goes thus:—



making a little basin: on the outer edges are built a few huts and houses: the whole basin very fertile. We were caught there by heavy rain. I, my men, my horses and mules, spent a miserable night there; every step in the soft red unctuous mud sunk deep; my tents bore its stains after repeated scrubbings, and the rough canvass cover of a valued folding-chair to this day bears witness to the tenacity of the clay with which that rock basin at Trete is filled, and which is unmistakably different to, and more tenacious than, any of the many fine specimens of mud and dirt passed on our way down.

Third day's march to Barakow, 1200 feet, and the plains of the Punjab are entered. The rapid change most oppressive for men and animals. The ground is strewn with great sandstone boulders. The stream that from Kyra Gully has run far below us on our right we cross, and find it swelling into a river, and at Barakow there are deep pools large enough to bathe with comfort in. Good shooting round Barakow, but country all sandstone boulders and scrub.

Fourth day, 17 miles to Rawul Pindi, 1700 feet. A long, level, monotonous tramp, stiflingly hot to us just fresh from higher levels, but delightfully cool to those who rode out to meet us and had spent their summer on the plains. Half-way we halted, where a bend of the river neared the road and a low ridge of sandstone rock ran from east to west, and on the northern side had been scooped out into a deep rock-pool, where lie plenty of fish, which, with the northerly aspect of the sloping rock, make this a favourite picnic spot from Pindi. The range of low sandstone hills run from E. to W., across the rapidly widening mouth of the valley we

have descended, and repeat and lose themselves in the plain, 7 or 8 miles more of which bring us to Rawul Pindi, situated, you will remark, on ground that, rising imperceptibly, is still 500 feet *above* Barakow, and on whose northern side are extensive and dangerous bogs.

Fifth day's march. Pindi to Dargool. Still going due south and ground steadily rising; all hard baked clay, but within a mile of Pindi the ground dips again over another series of low undulating hill, this time not bare rock, but covered with rounded stones of every size from that of an ostrich's egg to a nutmeg. The occasional cuttings for the road show that many feet from the surface these ground and *striated* egg-shaped stones, cemented firmly by tough clay form the face of the country we are travelling over—officers dismount and walk by common consent, drivers take their beasts short by the head, and every one abuses India, the Punjab, and that particular bit between Pindi and Dargool heartily. But after a few miles the stones get smaller and smaller, the great cracks in the dry clay get deeper, the clay banks get higher and look more and more like fortifications, and then we almost suddenly descend into the valley of the stream that has followed us so long and see a level fertile plain in front of us, and the stream now swollen into a river, able to make itself respected, sweeps round under great jagged perpendicular clay walls, between 200 and 300 feet high on its left bank, and of unmistakably the same mixture as stained our tents so badly at Trete; while the right bank is for miles, level smiling young corn fields, among which stand our tents in bright relief near the river's edge.

Reading in that camp Mr. M. Williams's article, and recalling the character of the ground passed over in my recent marches down from the hills immediately north of me, I confess myself a complete convert of his views, and here record facts that seem to me to confirm them.

III. A PLAN FOR ESTABLISHING LIFE-SAVING AND SIGNAL STATIONS IN MID-OCEAN.

By Isaac P. Noyes, Washington, D.C.

AS navigation increases between the Old and New World, the necessity for life-saving stations in mid-ocean is more and more felt every year.

This has undoubtedly been a subject that in some way or other has presented itself to many minds; to some as only a passing thought, as quickly away as it came; to others a thought chiefly in connection with its seeming impracticability; while to a few it has been much dwelt upon, in hopes of solving it in some way or other that would be of practical value to mankind, either for the saving of life or for the interest of science. Had this problem, in all its various details, been easy to solve, so as to have become a practical reality, something ere this would have becn done toward accomplishing it.

Of course the great impediment in the way is the enormous depth of water, making it, under any present known system, simply absurd to entertain thoughts of locating anything in the shape of a vessel or buoy that must necessarily require such an extensive cable to secure it in its place. There would be no trouble about the anchorage, or the vessel or buoy, in what was at the bottom or at the top, but simply in the connecting link between the two. "Simply" in this case though, like in many others, is the chief and almost impossible thing—that is, seemingly impossible up to this date. As we read the history of the world we see that safety-guards and institutions for the history of science are more and more established as the world progresses, and what at one age would not have been demanded, or even hardly thought of, in another is looked upon as an indispensable necessity. What is considered as impracticable in one age, in another or future age is smiled at and done apparently without the least possible effort, as though it was the most easy and natural thing imaginable.

Such a subject may prompt the question, Is it desirable even if practical? and perhaps many may scout the practicability of such an idea. When we read of some fearful accident or wreck at sea, where some noble ship containing

hundreds of precious lives has gone down with all on board, or only a few left to tell the mournful tale, this question of life-saving stations far out away from land has presented itself in a very forcible manner, and the mind seems impatient and would leap at once to the rescue to prevent or lessen the fearfulness of such catastrophes. But no sooner is this thought through the brain than "How?" presents itself. How overcome this huge difficulty in the way? how make this *connecting link*, the cable, that shall secure the vessel or buoy to the anchorage at the bottom? After a number of years' thought on this subject a suggestion finally occurred to me whereby, I think, this connecting link may be made a practical thing, and no longer be in the way of the consummation of this auxiliary to the safety of ocean travel, and the means of taking regular mid-ocean observations the same as on land. For the cable proper I would not have anything unlike cables in general, yet would suggest a galvanised iron-wire cable similar to those used on large derricks, in place of the regular chain as employed by vessels for anchoring, as on the whole I think it would be stronger. But then the matter of the kind of cable in this connection just now is not of much importance, the principal point being now how to construct and lay it so that it will be a practical thing.

Some parties who have thought on this question have suggested the anchoring of an immense buoy, and making of this a huge store-ship, &c. In this place I would remark that I think that a buoy properly shaped and constructed would be better than a vessel, for the reason that the upper portion of the cable could be so arranged as not to interfere with passing vessels. Experts say that the trouble with the buoy and cable would be twofold: first, the great length of chain would require it to increase very fast in size as its distance increased from the bottom in order simply to have sufficient strength to hold itself up. Secondly, this great weight would necessitate a buoy or vessel of such huge proportions as to make the thing as a whole simply impracticable in the way of cost, &c.

Sometimes it may be best to have the strength of a thing concentrated in one support or bearing, but in general the more we divide it up the better; and this would seem to be the idea to follow out in this cable or connecting link.

There would be no trouble in constructing a number of small buoys. So in the place of one huge buoy, which experts say would have to be so large, I would have a number of smaller ones, and these I would have at intervals of

about 250 feet, or thereabout. But the exact measurement of these intervals is of very little consequence at present; they may be more or less, and could easily be determined upon whenever it was proposed to carry the idea into effect, and would depend upon the size of the buoy, weight of cable adopted, &c.

This length, 250 feet, is simply taken in order to illustrate the plan with a little more facility, as most people can understand such things by illustration better than by abstract or technical terms. The depth of the Atlantic Ocean where such anchorages would be required is from 10,000 to 15,000 feet. At 250 feet for a section, this would make from twenty to thirty sections, requiring twenty to thirty buoys. If each buoy had sufficient bearing capacity to support its section of cable, it will readily be seen that the cable will stand as to strength on the same relative proportion and basis as any ordinary cable of 250 feet; so that if the anchor was in water 3 miles deep the strength of chain or cable would be intact for the purpose of holding the vessel or buoy at the surface, as though at any ordinary anchorage.

Many may ask, how are we to get these buoys—all strung, as it were, on this cable—into position? This, at first, seemed as insurmountable as the cable itself, but now it seems the easiest thing imaginable, and, in fact, not more difficult than it would be to effect any deep anchorage. Let the cable be constructed with the buoys all attached at their regular intervals, and in this manner towed to their respective grounds, soundings of which should be taken in advance, in order to determine the necessary length of cable, allowance to be made for the angle at which the cable would lie in the water. When this has been accomplished, secure the anchor and let go, and like any other anchor there would be no trouble in its finding its way to the bottom and taking hold.

When located, these buoys or stations should be manned much after the manner of lightships and life-saving stations, combined with lights, signals, stores, and life-saving implements; and, in addition to this, have regular signal officers stationed upon them, with established communications with the head-quarters on shore *via* the ocean telegraph. Then we could begin to trace storms upon the ocean as well as upon the land, and if for no other purpose it would seem that these stations would pay for themselves.

After having constructed the stations and suggested what could be accomplished by them, the practical mind will

certainly ask how will we pay for them? Even if good, there must be a great many of them in order to have them of any practical value. I propose to follow up the same idea as in regard to the construction—divide up the cost between the nations. All the civilised people of the world are interested in the safety of the travel between the Old World and the New, and it would seem should likewise be interested in the attempt to trace the line of a storm on the ocean as well as on the land, for only thereby can we become as familiar as we should be with the weather of the whole globe.

The great powers, like the United States, England, France, Germany, Russia, &c., should each construct and maintain a certain proportion; and the smaller and more distant powers, like Holland, Italy, Turkey, &c., a smaller proportion each. In order to stretch across the ocean between America and Europe, a contribution of two from each of the larger powers and one from each of the smaller powers would locate them within the comparatively short distance of 150 to 200 miles of each other. We cannot locate ocean accidents or tell just where storms may happen, and then place our buoys or vessels; neither is there an attempt to do this in our life-saving and weather stations on land: that would be simply impossible. It is not proposed either at present to be able to get them close enough to be in the neighbourhood of every accident or on the line of every storm; yet if placed at these intervals I think that they would pay for themselves in the good they would secure to humanity. We read of mighty works done in the past, but when we come to look at them carefully we find them executed by unwilling hands,—a serf class,—all for the mere vanity of those in power, and not for any real good to mankind. Somehow or other the world has always spoken of the present age in a mere hard mechanical way. We have the Stone, Brass, Iron Age, &c., though occasionally we have the more intellectual designation of "Age of Reason," yet I think there is another name by which this age should be known, "Age of Humanity." No age in the world has done so much for humanity at large; sure there are some minor exceptions, but then all ages have had these more or less. I think that we have the least bad and the most good; so, in respect to the numerous things that are done purely out of brotherly love, I think it full time that we call this the "Age of Humanity," and I hope that the benefactors of mankind may go on and never stop, but continue to extend their genial influence from age to age. Here, in this ocean

life-saving and signal service, I think that there is an opportunity for some of our well-disposed millionares like Peabody, Lick, Cornell, and others of their kind, to establish one of these stations in the ocean. I doubt if their money could be put to any better purpose than in helping carry out some such plan for the benefit of Science, and for the additional safety of those who go down to the sea in ships."—*Kansas City Review of Science and Industry.*

IV. AN HONEST CASHIER.

IT is one of the curious things in nature that the animals nearest to man in the order of development are of little or no use to him industrially. There has never been a time when strong races of men have not compelled their weaker brothers to work for them. But, barring the showman and the organ grinder, the meanest of men have not been able to subjugate or enslave their simian relatives. An ancient Arabic proverb accounts for the freedom of apes by the fact that they shrewdly refuse to talk: "well they know that were they to speak they would be made to work; so they wisely hold their tongues."

The proverbial prudence of the monkey appears to fail in a measure, however, in the land of the white elephant. An Austrian resident at the Court of Siam reports that in that country the monkey is trained to fish for crabs with his tail, as comical a pursuit as can well be imagined, except, perhaps, for the worthy and intelligent ape engaged in it, who sometimes gets a "bite" from a monster crab that he is totally unable to land, and falls a victim to the superior weight of his Cancer Ferox, who drags him into the water, drowns, and finally devours him. The Siamese ape is also stated to be in great request among native merchants as a cashier in their counting-houses. Vast quantities of base coin obtain circulation in Siam, and the faculty of discrimination between good money and bad would appear to be possessed by these gifted monkeys in such an extraordinary degree of development that no mere human being, however carefully trained, can compete with them. The cashier ape meditatively puts into his mouth each coin presented to him

in business payments, and tests it with grave deliberation. If it be genuine he hands it over to his master. If it be counterfeit, he sets it down on the counter before him with a solemn grimace of displeasure. His method of testing is regarded in commercial circles as infallible; and, as a matter of fact, his decision is uniformly accepted by all parties interested in the transaction. But, though a true and invaluable servant to his own particular master, it seems that his moral character is not altogether irreproachable. His deplorable passion for fruit renders him the terror of Siamese market gardeners, who find brute force inadequate to restrain him from visiting their orchards, and therefore have recourse to divers and sundry stratagems, one of which is reported to be as successful as it is certainly ingenious. A specially active and enterprising ape is captured and carefully sewed up in the skin of a tiger cat. He is then turned loose in the orchard of his predilection, and straightway clambers, as well as he may, incumbered by an unfamiliar garment, into the branches of a fruit tree among his unclothed fellows. Scarcely do these latter set eyes upon him with all his feline terrors thick upon him, when a dreadful panic strikes them, and they scramble away with piercing screeches and agonised chatterings. Never more do they return to an orchard which they believe to be infested by the deadliest enemy of their race. The startling intelligence is rapidly disseminated throughout the monkey society of the neighbourhood, and the wily gardener enjoys an absolute immunity from depredation for ever afterwards, for the very thought of a tiger cat appals the simian soul, and doubtless the tale of "the awful apparition in Ting-tse's orchard" is handed down in quadrumanous families from generation to generation.—*Scientific American.*

V. OPIUM SMOKING AMONG THE CELESTIALS.

By Rich V. Mattison, Ph.G., M.D.

As one passes through the Chinese quarter of San Francisco he cannot help being sharply impressed with the immense traffic in an article which is seemingly part of the very life necessities of this curious people. We seem scarcely to pass a shop, whether devoted to the sale of

clothing or drugs or groceries, but what we find a notable proportion of the business to consist in the sale of opium. We pass the shop of the merchant, and while one assistant is counting out the gold for a bill of exchange on the Flowery Kingdom, we see another weighing carefully a small portion of the much coveted drug. The jeweller, surrounded by the precious bracelets of nephrite and phrenite, lays aside for the moment the curious golden circlet he is filing, to catch up the balance and poise upon the pan the little horn cup a moment, and again return to his employment. The grocer, surrounded by the many dainties of Mongolian gastronomy, stands under the rows of varnished fowls, balance in hand, dispensing the drug with the most imperturbable gravity and solemnity. As we stand by the half-open doorway on one of those beautiful summer evenings so common to Pacific climes, a young celestial enters the shop to return in a moment laden with his store of dreamy forgetfulness, the absorption of which transports him, in imagination, to his native land, where riding in a gorgeous palanquin, with maidens to fan him and coolies to fly at his slightest wish, he passes into his dwelling by Kin-Sha-Kiang, or the river of the golden sands, where his wife, with the feet of a mouse, brings his tea in golden cups, and so he passes into the arms of Morpheus (or Morphia's meconic embrace), his couch covered with scarlet and silken curtains with fringes of golden strands, only to awake finding himself lying coiled up on a hard board shelf covered with matting, his head upon a block; for now transported by the magic lamp of a private detective we are in the classic precincts of an opium den. To reach it we have passed through many dark, subterranean alleys, through courts of filth and squalor and wretchedness to any other than Mongolian eyes. On either side of the room, which is about sixteen feet square, are accommodations for twenty or more smokers—shelves rising in tiers like the bunks of a steamer's cabin. In the centre is a small table covered with the shells, bowls, cups, lamps and other paraphernalia of a first-class opium den, sustained by liberal patronage. It was early evening, scarcely midnight, and at our right inclined a strong, sleek, almond-eyed native of a foreign land, well known to our guide as one of the most inveterate smokers of the city. Immediately in front of him was a small saucer filled with lamp oil, and inverted over it was a tumbler in the bottom of which (or the apex as it was placed) a small hole was drilled, through which protruded a piece of wick—this being lighted constituted the lamp. By its side lay an oyster-shell containing a

quantity of a dark coloured extract, and on either side a long wire exactly like the knitting needles of our grandmothers, excepting that one extremity ends in a small spoon. The pipe is naturally of interest. The most usual style is that having a shaft of bamboo, resembling somewhat a flute pierced laterally at each extremity, at one of which is fitted a small metal cup in which to receive the bowl of the pipe proper. This bowl is of earthenware or metal, and is about 3½ inches in diameter, convex on both its upper and lower surfaces, the latter ending in a tube of half an inch in diameter and of similar length; this fits into the metal cup of the bamboo shaft. The upper convex surface is pierced in the centre with a metal tube, having a funnel-shaped aperture about one thirty-second of an inch in diameter. The cavity of the bowl, as here exhibited, is of a capacity of nearly a hundred cubic centimetres.

The pipe is filled by taking up on the apex of one of the knitting needles a small portion of the extract, usually from 2 to 5 grains, and holding it momentarily in the flame of the lamp, rotating the needle dexterously meanwhile, then withdrawing it only to repeat the same operation until the extract is dried to light brown colour and of just such consistency that it sticks to the pipe, when, with a dexterous twist, the point of the needle is inserted into the apex opening of the upper convex surface of the bowl, and the needle instantly withdrawn by a rotary motion. This manœuvre places the extract in the shape of an inverted pyramid, with a central opening communicating with the orifice leading into the cavity of the bowl. It is during this evaporation of the extract over the lamp that the cultivated smoker judges of the quality of the opium. If it bubbles up to that delicate shade of light brown, and at the same time gives off the peculiar odour so characteristic to the trained olfactory nerve bulbs of the Mongolian smoker, then is he satisfied of the quality of the extract purchased.

The bowl being filled, it is inverted over the flame of the lamp at an angle of about 45°, and the volatilising narcotic rapidly drawn by a few strong inspirations into the body of the pipe, and so on into the pulmonary cavities. The inspiration thus made is peculiar; it is not only buccal, but more strongly pulmonary. The inspiration is slow and deep and prolonged, until the chest is filled with the narcotic vapour, and expiration then occurs with the mouth closed, and the inspired smoke issuing slowly through the nostrils.

The quantity smoked varies greatly with different smokers, it varying from 30 grains to upwards of 300 or 400 grains at

one sitting, this being from 10 to 100 pipefuls of the extract. The whole process is done in the most methodical manner. The veriest coolie or wealthiest merchant proceeds to arrange his lamp and extract and pipe with a solemn gravity ludicrous to behold. In this grave affair of state, perhaps five, ten, or even fifteen minutes are consumed; then follows an interval of placidity in those unaccustomed to smoking, to be followed by another and another smoke. But we begin to feel almost like opium smokers ourselves breathing so long the atmosphere De Quincy called "the mephitic regions of carbonic acid gas," so we ascend to the upper regions of the street, and realise, as we never have before, the fresh, invigorating influence of the brisk midnight air of the ocean coast.

The above is taken from the "American Journal of Pharmacy." The effects of opium-smoking have been recently tested by N. von Miklucho Maclay during his stay in Hong Kong. The experiment was made at the Chinese Club, where every convenience for smoking opium is to be found. Dr. Clouth, of Hong Kong, took the necessary observations, and his notes are summarised as follows in the "Chemist and Druggist":—Herr Maclay was in normal health, and had fasted 18 hours before commencing the experiment. He had never smoked tobacco. Twenty-seven pipes, equivalent to 107 grs. of the opium used by the Chinese, were smoked in two and three-quarter hours, at tolerably regular intervals. The third removed the feeling of hunger caused by his long fast, and his pulse rose from 72 to 80. The fourth and fifth caused slight heaviness and desire for sleep, but there was no hesitation in giving correct answers, though he could not guide himself about the room. After the seventh pipe the pulse fell to 70. The twelfth pipe was followed by singing in the ears, and after the thirteenth he laughed heartily, though without any cause that he can remember. Questions asked at this time were answered only after a pause, and not always correctly. He had for some time ceased to be conscious of his actions. After the twenty-fifth pipe, questions asked in a loud tone were not answered. After the last pipe had been smoked he remarked, "I do not hear well." Forty minutes later there was a slight return of consciousness, and he said, "I am quite bewildered. May I smoke some more? Is the man with the pipe gone already?" Fifteen minutes later (4.55 P.M.) he was able to go home, and then retired to bed. He woke the next morning at 3 A.M. and made a hearty meal, after his fast of 33 hours. During the

next day he felt as if he had bees in a great hollow in his head, as well as a slight headache. The organs of locomotion were first affected, next came sight and hearing, but Herr Maclay is very positive that there were no dreams, hallucinations, or visions of any sort whatever.

VI. IS ORGANIC VARIATION FORTUITOUS?*

ADMITTING the broad fact that the organic world, like the organic individual, has been produced not by any sudden and arbitrary intervention of supernatural power, but by a gradual course of Evolution, the question remains—What is the efficient cause of such Evolution? Are species, as we find them, due to the mere accumulation of fortuitous changes through an almost infinite succession of ages? Or can we trace "in the inner domain of life" other and more powerful factors than Natural Selection? A profound thinker suggests as such agencies "Habit and Intelligence." By Habit he understands "that law in virtue of which all the actions and the characters of living beings tend to repeat and to perpetuate themselves not only in the individual, but in its offspring. Mr. Murphy's "Habit," therefore, includes what is commonly known as heredity. He considers intelligence "an attribute of all living beings, and coextensive with life." But by intelligence he means "not only the conscious intelligence of the mind, but also the organising intelligence which adapts the eye for seeing, the ear for hearing, and every other part of the organism for its work." It will easily be seen that the inquiry thus opened must be to no small extent a critique of the hypothesis commonly known as Darwinism. But from the majority of writers who have undertaken such a task Mr. Murphy differs most favourably. He is a decided Evolutionist, agreeing with Darwin "in the belief that all species have been derived by descent with modification, probably from one, certainly from a few original germs." He admits Natural Selection to be "a really operative agent—a *vera causa*," but he differs from Darwin and from his direct disciples by contending—and in our opinion with great cogency—that "the process of

* Habit and Intelligence: a Series of Essays on the Laws of Life and Mind. By J. J. MURPHY. London: Macmillan and Co.

Evolution proves the agency of an Intelligent Power, acting through and controlling the unintelligent forces of Habit and Variation, just as all the vital forces act through and control the inorganic ones."

But though thus differing from Mr. Darwin on a fundamental point, our author never attempts to undervalue his high authority and the services he has rendered to Natural Science. Such a passage as the following stands by no means singly in the present work:—"Much is to be said for this theory, as for everything that Darwin has ever proposed." Nor have we any imputation of motives. In this respect Mr. Murphy stands out in contrast, not merely to a number of lesser luminaries, but to a most distinguished biologist whose views on the Darwinian question in many points approximate to his own. In further contradistinction to certain writers on Evolution the author is not a mere man of culture—an outsider who has "read up" the subject, and who then assumes to teach when scarcely qualified to be a learner. On the contrary, he tells us that for ten years he has been thinking, reading, and conversing on the subjects discussed in this volume, and after a careful examination we see every reason to accept his avowal. Of one of the non-biological critics of Darwinism Mr. Murphy indeed speaks, in our opinion, too favourably. Referring to a somewhat over-rated article in the "North British Review" (June, 1867) he writes—"This is a good instance of the service that an able man may do to a science which is not his own, and of which he does not know the details." Yet in an earlier passage of his work he quotes from the very same article an objection against Evolution almost childish in its character, and based on utter ignorance of the subject under discussion. Concerning this argument Mr. Murphy very justly remarks that it is "one which could not have occurred to a naturalist."

The grounds advanced against the sufficiency of Natural Selection, to account for the origin and characteristics of species, vary greatly in their value. One of the most weighty, recently insisted upon in the "Quarterly Journal of Science" (vol. viii., p. 453), must be sought in the fortuitous destruction which takes place among young animals, quite irrespective of their greater or less "fitness" for survival. Such destruction is fully admitted by Mr. Darwin himself. This difficulty, together with that pointed out by Prof. Tait, may be summarised as follows:—"The final establishment of the superior type is dependent at each step upon three accidents. First, the accident of an individual sort or

variety better adapted to the surrounding conditions than the then prevailing type; secondly, the accident that this superior animal escapes destruction before it has had time to transmit its qualities ; and thirdly, the accident that it breeds with another specimen good enough not to neutralise the superior quailties of its mate."

A still more important argument is drawn from structure in advance of function. It is obvious that Natural Selection can only preserve such varieties as are immediately and directly useful to the organism in which they occur. Yet, as Mr. Murphy shows by a number of well-selected examples, there are cases "where structure has been laid down as a preparation for function before the function could be brought into action, as truly as the shipwright when he lays the keel on the land intends the future ship to float on the water." Thus in the metamorphosis of Crustaceans one particular stage—the so-called *Zoea* phase—is characterised by a prolonged abdomen calculated for future utility, though for the time being rather an incumbrance. Elsewhere the author asks, of what use can a dorsal groove and an incipient cartilaginous band be to Ascidian larvæ? Yet, if we regard these creatures as representing the common ancestors of the Vertebrates and Ascidians, we have here the first outlines of the vertebral column essential to the former. The transition from the swim-bladder to the lungs, from the fins to legs with toes, and from the reptile's fore leg to the bird's wing, are also, on the hypothesis of Natural Selection, beset with difficulties. We can scarcely imagine any intermediate stages between the two which would not be useless, even if not positively injurious, to the animal in which they might occur. Mr. Murphy expresses the opinion that this argument is new, and that, if taken up by thorough anatomists and embryologists, "instances of structure in anticipation of function might be found everywhere in the organic world." *

The phenomena of mimetism, or organic mimicry, have generally been considered as affording strong evidence in favour of the Darwinian view. But it is difficult to understand how the first slight variations in the direction of approximating a defenceless animal to some formidable species could be of such essential utility to the former as to ensure its preservation by Natural Selection. A further very formidable difficulty is that the mimicking form is—and if

* A somewhat similar argument, at least as far as concerns the dangers of intermediate stages, may be found in the "Quarterly Journal of Science" (vol. v., p. 329). The instance there given is, however, much less happy and conclusive than the cases adduced by Mr. Murphy.

the mimetism is to be useful must be—rare. Now, according to hypothesis, it is only very numerous species which are likely to yield individuals possessing the required variations. Another difficulty is to be found in the metamorphosis of certain Dipterous insects. Instead of the tissues of the larva being transformed into those of the perfect insect, they seem to be melted down, except at certain spots, into a semi-liquid substance, from which the tissues of the perfect insect are afterwards re-developed. Mr. Mivart bases on this exceptional fact an argument against the all-sufficiency of Natural Selection.

To the contention that geological time is too short for Darwin's theory, few geologists and biologists will attach much importance. The very discrepancies in the calculations of the mathematicians and physicists who have undertaken to fix a maximum limit—discrepancies which are in the proportion of 1 to 40—might provoke a smile.

From all these considerations, and from many others which we cannot here particularise, Mr. Murphy concludes that organic species and their peculiarities are due not alone to the accumulation of fortuitous variations, but to Intelligence. Still he does not hold that the intelligence which directs the formation of organic structures is necessarily conscious, nor that such structures and their adaptations are the direct work of Creative Wisdom: he considers it "more reasonable to believe that organic progress has been effected not by a fresh exertion of Creative Power at every one of the innumerable stages, but by a principle of Intelligence which guides all organic formation and all motor instincts, and finally attains to consciousness in the brains of the higher animals, and to self-consciousness in the brain of Man." He thus, equally with Darwin, avoids making God responsible for the imperfections of the organic world, for the existence of parasitic worms, &c., which on the hypothesis of the Old Natural History "can only be regarded as instruments of torture devised by the Creator, and whose existence no writer of Bridgewater Treatises has yet even attempted to reconcile with His infinite wisdom and benevolence."

An important consideration, which would well repay special notice did space allow, is that the constructive instincts of social insects—bees, wasps, and ants—cannot be inherited in the direct line, because these instincts are possessed by the workers alone, which are non-reproductive. This, however, is only partially correct. Female wasps, humble-bees, &c., especially at the first establishment of a new colony, enjoy none of the exemption from labour which falls to the lot of the queen hive-bee and of female ants.

Mr. Murphy remarks that "neither association nor the principle of imitation will account for the child's use of the word I," and quotes Ferrier's opinion that the use of this pronoun proves not only intelligence, but self-consciousness, which appears to belong to man alone. We have certainly met with no instance of a parrot or other talking bird speaking of itself as "I." But it is interesting to note that young children, before learning to use the personal pronoun, speak of themselves by the same name which is applied to them by others. Just as the parrot which is addressed as Polly connects that word with itself, and if desirous of anything says, *e.g.*, "Give Polly a bit," so the young child addressed as "Baby" speaks of itself as "Baby." This is additional evidence that a child at a certain early age is mentally on a level with the mature parrot, and shows that the difference between man and beast is one of degree rather than of kind.

Incidentally we note that the author considers it possible, "though to our faculties not conceivable, that the number of dimensions in Space may be infinite, though we exist and move in only three." The discussion of this interesting speculation, which seems gradually dawning on the human mind, must be left to mathematicians.

On p. 571 of Mr. Murphy's work we find an assertion with which we are unable, without certain reservations, to agree. We read that "the more highly organised among organisms grow to the largest size and live the longest." We fully admit that the Vertebrata are as a class more highly organised, longer-lived, and larger than the Evertebrates; but the birds, and probably reptiles and fishes, though less highly organised than the mammals, appear to live longer; and even within the mammalian circle, man and the anthropoid apes, though unquestionably the most highly organised, take but an intermediate rank in stature.

Dr. Beale's argument—not against Darwinism, but against Evolution in general—that the microscopic and chemical characters of species are less variable than their external features, requires an amount of attention which it has not yet received.* Mr. Murphy very properly declines to pass any definitive judgment on the question.

It will not be irrelevent for us to pron ounce our conviction that "Habit and Intelligence" will amply repay a close and critical study, and that the eighteenth chapter in particular points the way to a field of research not to be neglected.

* Beale's edition of Todd and Bowman's Physiology, p. 41.

VII. THE SCIENCE OF AGRICULTURE.

IT is now, we suppose, universally admitted that agriculture, in order to be progressive and profitable, must be conducted on scientific principles. To attain this end and to meet the demands of an increasing population the United States Government has for a long time included a Department of Agriculture, the main object of which is "the introduction of all the productions of the earth that can be grown in any part of the country, and to encourage by every means that diversity of production which is at once the safety and the wealth of the nation." From the Prefatory Report of the Commissioner of Agriculture for 1878 we learn that experiments were made during last season on the growth of different varieties of sugar cane. As far as the experiments go a variety of cane from Jamaica, called the "Salangore," is shown to be worthy of extensive introduction and trial. The attention of the Commissioner has, however, of late been more especially given to the question of producing large supplies of sugar from sorghum and maize. He procured as much as possible of the pure well-cured seed of a variety of sorghum called the "Minnesota Early Amber," and distributed the same in every Congressional district in the United States. The results of this distribution have been most favourable, and the variety has yielded everywhere a large amount of rich saccharine juice, which under proper treatment gives excellent sugar and syrup, the yield being from 120 to 250 gallons of heavy syrup to the acre. It is proposed another season to make experiments with the different varieties of maize and sorghum, and to ascertain the different modes of cultivation and the stage of growth at which the production of sugar is at its maximum, in order that with as little delay as possible the country may be prepared with all necessary data to enter intelligently upon this new industry.

The great drawback to the work of the Department seems to be the want of a larger chemical laboratory with a sufficient appropriation to meet the expenses of the additional force that will be necessary to carry forward investigations on a larger scale than the present laboratory and appliances will permit. With the facilities of the existing laboratory much information which skilful chemical analysis can only

determine is, remarks the Commissioner, necessarily withheld from the farmer and the manufacturer. The report contains extracts from letters from prominent agriculturists in the United States, all of whom testify to the fact that the agricultural interests of the country would be greatly advanced by a more thorough analysis than has yet been made of the grains, grasses, and edible roots, in order to determine the exact value of each in the production of milk, beef, and fibre, or muscular power.

The English Government would do well to follow the lead of the American Government by establishing experimental agricultural stations in various parts of the kingdom with properly furnished laboratories and experienced chemists. It is impossible to over estimate the benefits to agriculture in England resulting from the scientific experiments made on a large practical scale by Messrs. Lawes and Gilbert at Rothamsted, but we contend that such work ought not be left entirely to private enterprise.

In the preface to the English translation of his Lectures on Chemical Manures* at the Experimental Field at Vincennes, M. Georges Ville remarks that it is important that both England and France should be alive to the fact that the agricultural crisis from which both countries are now suffering, as well as the more serious troubles which threaten civilised nations, are only the prelude to the economic struggle between the Old World, bound in the trammels of tradition, and the New World, pressing onward free and unrestrained in the path of progress.

At a period when the means of communication had not reached the development which they have since acquired, the home markets provided certain and easy outlets for agricultural produce. But at the present time, with free trade and the facilities of transport, farmers are compelled to compete in our own markets with all the world. In order that the struggle may be possible and remunerative, it is absolutely necessary that crops of every kind should be increased to their utmost possible limit. The traditions of the past are not sufficient for the necessities of the present. We want more rapid, more economical, and more powerful processes. The agriculturist used to divide the land into two nearly equal parts, setting one aside for grazing pur-

* On Artificial Manures, their Chemical Selection and Scientific Application to Agriculture. A Series of Lectures given at the Experimental Farm at Vincennes during 1867 and 1874-5. By M. GEORGES VILLE. Translated and Edited by WILLIAM CROOKES, F.R.S. Illustrated with Thirty-one Engravings. London: Longmans, Green, and Co. 1879.

poses, or for growing forage plants, and reserving the other for cereal crops, which was equal to asserting that in order to grow cereals there must be meadow land, cattle, and manure. Instead, however, of growing meat in order to have corn, he must grow corn for profit's sake in the first place, and afterwards for straw, cattle-feeding, and manure.

The object of the farmer, then, should be not to produce manure, but to manure his land more abundantly than formerly. No matter what may be the material he employs, whether it be farmyard or chemical manures, used either together or separately, he must somehow or other give back to the soil a larger amount of fertilising material than that lost by the growth of the crops. In the cultivation of the soil increase of production depends less on the worker and on the quality of the tools which he employs than upon the quantity of fertilising materials which he has at his disposal. According to M. Ville the only way to do this is to employ chemical manures, and to prove his assertion, to show that with chemical manures large crops may be quickly obtained from the most barren lands, he refers among others to an experiment carried out by M. Ponsard, President of the Agricultural Committee of Omey in Champagne, on a piece of waste land in one of the most barren districts of a proverbially barren portion of that province. M. Ponsard manured one half of the ground with about 32 tons of farm-yard manure per acre, and the other with about half a ton of chemical manure per acre. With the farm manure he obtained 14 bushels of wheat, whereas with chemical manure the land yielded about 30 bushels, there being a loss of £19 in the former case and a gain of £17 in the latter.

Similar experiments have been made with beet-root, potatoes, sugar-cane, &c., and in each case the results have been in favour of the chemical manure. In fact, by varying the quantity of the ingredients entering into the composition of chemical manure, so as to suit the requirements of each class of plants, the work of vegetation may be regulated almost like a machine, the usefulness of which is in proportion to the fuel it consumes. The first point is to discover the degree of richness of the natural soil, and then to ascertain the dominant constituent of each plant. Plants are divided into three categories—first, those in which nitrogenous matter is the dominant constituent, such as cereals, hemp, colza, beet-root, and general garden stuff. The second group, in which calcic phosphate preponderates, comprises maize, sugar-cane, Jerusalem artichokes, turnips, and sorghum. The third group includes leguminous plants,

such as clover, sainfoin, lucerne, potatoes, and vines, and in these potash is the dominant ingredient.

Now what M. Ville terms "normal manures" contain calcic phosphate, potash, lime, and nitrogenous matter, differing only in the respective proportions of these four substances. By varying, therefore, their relative proportion according to the necessities of the particular plants for which they are required, the principle of dominant constituents can be applied to every possible condition which may arise, thereby meeting the requirements and advancing the interests of every description of farming.

It is not necessary to restore to the soil, weight for weight, constituent for constituent, all that is taken from it, but the four constituents named above are essential, and must always be added. Analyses of farmyard manure show that it contains the four constituents which it is essential to restore to the soil, but it also contains carbon, hydrogen, and oxygen; also sodic chloride, magnesia, soda, silica, ferric oxide, &c., all of which are abundantly contained in the poorest soils, and which do not therefore increase the value of manure. Farmyard manure therefore owes all its efficacy to the four essential constituents mentioned above. But we have just shown that each of these constituents with regard to the three others fulfils functions that are in turn subordinate or predominant, according to the nature of the plants to be grown; with farmyard manure, however, there is no possible division; its composition cannot be varied. The only alternative, then, is to use it in conjunction with chemical manures. In practice the quantity of farmyard manure usually applied to an acre of land is, we believe, from 16 to 20 tons, in which quantity the four essential constituents form only about a fortieth of the whole mass. Their proportions are as follow:—

Nitrogen	181	lbs. per acre.
Potash	164	,,
Phosphoric acid ...	98	,,
Lime	352	,,

To place the land under the proper conditions for high cultivation the amount of the fertilising substance in the farmyard manure must be at least doubled by means of chemical manures, and in the case of each particular plant it is necessary to concentrate that chemical agent which is especially favourable to its growth. It must also be remembered that one-third of the nitrogen is lost to the soil on

account of the decomposition which the manure must first undergo before it can exercise its action.

M. Ville strongly advocates the foundation of experimental fields. They are, he affirms, the only reliable method of ascertaining with certainty the composition of the soil with respect to the requirements of agriculture. A piece of land should if possible be selected which in its physical nature and degree of fertility represents the average quality of the land that is to be cultivated. For a newly-worked farm the field should consist of twenty plots, each containing about four poles, arranged in two parallel rows of ten plots each. The first row should be devoted to the cultivation of wheat, and the second to that of beet-root or potatoes, according to the climate and the wants of the district. The wheat furnishes indications of the richness of the superficial layers of the soil, and the beet-root of the deeper layers. Full instructions are given respecting the manuring of the several plots. M. Ville also gives directions for establishing experimental fields for agricultural colleges, societies, and for elementary schools. For the latter the plots should be about eleven yards square. By carrying out the advocated system of manuring it will be conclusively established that it is possible to farm without using farmyard manure; that a manure can be and is composed which more than takes its place, and that the action of animal manure is intensified by the addition of chemical manure.

VIII. MEASUREMENT OF POWDER PRESSURES IN CANNON BY MEANS OF THE REGISTERED COMPRESSION OF OIL.

THE determination of the pressure exerted by the elastic gases produced by the combustion of gunpowder is an element of the first importance in the theory and practice of gunnery, and a measure of this pressure is indispensable in all mathematical computations of the effects of gunpowder on the projectile or on the gun.

In 1855, Dr. W. E. Woodbridge conducted some experiments at Washington Arsenal with the view of measuring the powder pressures of fired gunpowder. The results of these experiments were not published by the U.S. Ordnance Department until November, 1878, when some additional experiments were recorded by the author.

It was proposed in these experiments to ascertain the pressure of the gases evolved by the combustion of gunpowder, by including in the cavity within which the pressure should be restrained a piezometer, which, by registering the compression of a liquid contained within it, should afford an indication of the pressure to which it had been exposed.

This instrument, as employed in these experiments, is a small cylindrical vessel of steel, inclosing a quantity of oil destined to receive the pressure of the fluid by which it may be surrounded, through the medium of a piston, which is carried inward a distance proportional to the amount of compression. To the piston is attached a stem of wire, extending inward, on one side of which a fine point is made to press, inscribing, when the piston is moved, a line on the stem equal in length to the extent of its motion.

In preparing the piezometer for an experiment, two items are to be specially observed: it must contain no air, and the "setting," or adjustment of the quantity of oil contained, must be done at the precise temperature the instrument is to have at the moment of firing.

The procedure is as follows:—All the parts are first oiled over their whole surfaces. The adjusting screw is inserted into the body of the instrument, which is then set upright in a socket attached to the middle of a small pan intended to catch any overflow, and is nearly or quite filled with oil, which should be made to flow down the side of the cavity rather than in a stream. The support of the marking-point, quite clean but covered with oil, is now screwed into its place, with the aid of a special implement, described. When this is withdrawn, it will be necessary to replace the oil caused to overflow by its insertion. The barrel is now slowly put in its place and screwed firmly down. The hole in the piston for receiving the stem is filled with oil, the stem screwed in, and the piston inserted in the barrel. The adjusting-screw is loosened a little, permitting the piston to be pressed just below the top of the barrel, and again tightened. The next step is to bring the instrument and its contents to the setting temperature. For this purpose a water-bath (a common wooden pail) was provided; a narrow tin cup, deeper than the bath, and weighted at the bottom

so as to stand upright within it; and a pair of wooden pinchers for handling the piezometer, which instrument could be inserted in them in such a way as to be nearly enveloped and yet to leave the adjusting-screw and piston readily accessible. The piezometer, seized in the pinchers, is placed at the bottom of the cup in company with the tools to be used in setting it, and is covered with a loose wad of cotton. The cup is set in the middle of the bath and surrounded with water, kept as nearly as possible at the desired temperature, for a sufficient time to impart, as nearly as appreciable, the same to the instrument. It is then withdrawn, the screw loosened, the piston depressed a little to a regulated depth with a special tool, the screw tightened, and the piston rotated a few degrees, which completes the setting. The object of this last movement is to inscribe a transverse line on the stem, affording a starting point in measuring the length of the stroke.

Small changes of temperature after the instrument is set are of no consequence, as the oil will of course return to the same volume, and the piston stand at the same place, on returning to the same temperature.

Before placing the piezometer in the hollow plug, a thin leather envelope, kept saturated with oil, is drawn upon it (with the intention of affording protection against the shock of firing), and when inserted the remaining space within the plug is filled with oil, which is retained by stopping the opening through the retaining-ring (which forms the communication with the bore of the gun), with a loosely-fitted disc of cork or leather.

One particular to be noted is the position of the eye of the piston with reference to the line in which the gun will recoil on firing. The metal surrounding the eye occupies a position at one side of the piston's axis of rotation in the barrel, thereby throwing the centre of gyration out of that line; and if that centre be so situated as to fall outside of a plane coincident with the line of recoil, it is evident that the piston will have a tendency to rotation when the gun is fired. It was apprehended that this rotation might interfere with the proper marking of the stem, or accurate measurement of the mark, and for that reason care was usually taken to place the piston in such position that its centre of gyration should be nearly in the plane just mentioned and forward of its axis of rotation.

After firing, the length of the stroke was measured under a compound microscope by the application of a scale divided into thousandths of an inch and capable of being read to ten-thousandths.

The volume of oil subjected to pressure, the area of the piston, and the length of stroke being known, we derive from them the degree of compression. To complete the data for ascertaining the pressure to which the piezometer has been exposed in any given case, it becomes necessary to determine the relation of compression to the pressure required to effect it.

The experiments made for this purpose were conducted with a good deal of care to secure accuracy, and were somewhat elaborate.

The barrel of the compressing pump is of cast-steel, placed horizontally, its exterior diameter being 2·2 ins., and that of its bore 0·7 ins. The piston, which is also of cast-steel, tempered, is forced in and retracted by means of a square-threaded steel screw 1·5 ins. in diameter.

These parts are placed in a strong frame of iron. The screw is turned by means of a ratchet-wheel, which forms its head, and a lever and pawl connected with it. To facilitate the retraction of the piston a crank is attached. Screw-valves prevent, during the alternate movements of the piston, the escape of the liquid through the aperture by which it enters the barrel, and its return from the receiver into which it is forced. A safety-valve of tempered steel, ground to a seat, also in hardened steel, with its graduated lever and weight, is attached to the pump, and serves for measuring the pressures it is made to exert.

A comparison of the pressure of a column of mercury of known height, with the weight on the valve of the gauge required to balance it, was adopted as the most accurate method of determining the relation between the weights to be placed on the scales and the desired pressures per square inch.

The mercurial pressure-gauge consists of a cistern of glass containing mercury, and a series of perpendicular glass tubes attached to a graduated staff and joined continuously together, extending to the height of fifty-two feet. The tubes are so connected with the cistern that by pressure upon the surface of the mercury it may be forced to ascend the tubes.

In computing the pressure of the column, its height from the surface of the mercury in the cistern was taken, and the counteracting pressure of the weight of the oil by which the pressure was applied was allowed for in proportion to its height and specific gravity.

To ascertain the compressibility of oil in steel piezometers, it was filled and set in the same manner as for use in the

gun. The receiver was immersed in a water-bath kept at the proper temperature. The pressures, temperatures, and lengths of stroke are given below ;—

Compression of Oil in Steel Piezometer.

Pressure. Per square inch. Lbs.	Length of stroke at— 60° In.	50° In.
10,000	0·2148	0·2401
15,000	0·2780	0·3266
20,000	0·3656	0·4108
25,000	—	0·5670

We now arrive at the experiments in which the piezometer was used to record the compressions produced by the pressure of fired gunpowder under various conditions, and from which the pressures were to be derived. They were made at Washington Arsenal, D.C., commanded by Major Alfred Mordecai, who heartily co-operated in their conduct.

Two six-pounder guns, one of iron the other of bronze, were used in these experiments. The diameter of the bore of each, at the seat of the shot, was 3·69 ins., very nearly. The iron gun was used in the first three experiments, in which the piezometer was attached to the bottom of the bore. It was afterwards pierced through its side to receive the piezometer inclosed, as described in the Report, in a hollow steel plug, the centre of the opening being 1·5 ins. forward of the bottom of the bore. It was used in this form in several experiments. The bronze gun was, however, used in the greater number. It was pierced with nine holes at different distances from the bottom, beginning at 1 inch and ending with 47·8. They were arranged alternately to the right and left of the central vertical plane, in the upper half of the gun, and inclined 45° to that plane.

A solid plug was fitted to each hole, and was withdrawn only to permit the insertion of the piezometer.

Small holes to receive a thermometer with an elongated bulb were drilled near the openings, mentioned above, extending to within a short distance from the bore.

Several experiments were made to ascertain pressures in a musket barrel. A portion of the bore at the breech-end was enlarged enough to receive the piezometer, and was separated from the forward portion by a ring screwed to place and a leather disc closing its opening, as described in connection with the hollow plug. The piezometer was introduced from the rear, the surrounding space filled with

oil, and the breech-plug inserted afterward. A vent of the normal size was drilled just forward of the partition formed by the ring and disc just mentioned.

It has already been intimated that the temperature at which the piezometer was set was that at which the gun was to be fired. In the earlier experiments the method by which it was intended to effect this was by inclosing the instrument, *ready* to be set, in the gun for a sufficient time to equalise their temperature—which was left to merely atmospheric influences. The instrument was then withdrawn, quickly set, and returned, and the gun fired without delay.

It was afterwards found that the gun changed its temperature sensibly, from changes of wind and sky, in shorter intervals than was supposed. To avoid the errors liable to arise from this cause the practice was changed. The instrument was set at a determinate temperature, higher than that of the outside air, but lower than that of the arsenal workshops. The gun was run into the shops (near by) for a short time in the interval between the experiments, and made slightly warmer than the firing temperature. The piezometer was then properly inserted, the gun taken outside, and when cooled to the proper point the thermometer was removed and the gun fired by a primer already inserted.

The average pressure indicated at different distances from the bottom of the bore, the charge being in each case $1\frac{1}{2}$ lbs. of powder, in a cartridge-bag, and one 6-pounder shot, is given below:—

Distance from the bottom of bore.	Pressure per sq. in.
1-inch (bronze gun)	20,210 lbs.
$1\frac{1}{2}$,, (iron gun)...	18,150 ,,
4 ,, (bronze gun	16,510 ,,

The capability of the piezometer to measure pressures of much more brief duration than the time of the passage of the ball through the barrel will appear from a comparison of the inertia to be overcome in each case. The weight of the piston and stem is 20 grs., and the resistance of the inertia of the oil is equivalent to that of about 36 additional grs., while the weight of the pistol ball is 218 grs. After taking into account the differences of area and the mode of action in the two cases, I think it may be safely concluded that the piston will have reached its extreme position as soon at least as the ball has passed through the same distance.

The superior practical importance of determining the *greatest* instead of the *average* pressure upon the sides of the

bore, with reference to the construction of guns, is evident from the fact that the strength of the gun must be adapted to the former and not to the latter. The inadequacy of the mode of determining, even relatively, the amount of pressures of very brief duration by the initial velocities of balls projected by them is shown by the effects of detonating powders used in fire-arms: the barrel may be shattered while the ball is thrown with comparatively little force.

The effect of increasing the charge of powder, compared with that of increasing the weight of the projectile, in augmenting the pressure was unexpected, and has to some seemed almost incredible; although, as is not surprising in regard to a subject of such a nature, opposite views are held by men thoroughly conversant with artillery.

	Lbs.
The mean of the pressures indicated at 1 inch from the bottom of the bore, with a charge of 1½ lbs. of powder and a ball weighing, with its sabot, 6·3 lbs. nearly, was	20,210
With the same charge, at 1½ ins. from the bottom	18,150
With 1½ lbs. powder and a shot weighing 12·15 lbs., at 1½ ins. from bottom...	20,743
With 2 lbs. powder and a ball of 6·36 lbs., at 1 in. from bottom	20,640
With 3 lbs. powder and a ball of 6·43 lbs., at 1 in. from bottom	22,220

The pressure in the cases in which shot of 12·15 lbs. weight were used, reduced to that which would be indicated at 1 inch from the bottom of the bore, on the supposition that the same difference would exist as with the smaller charges, would be 22,700 lbs., indicating that the effect of doubling the weight of the ball (but with diminished windage) is not very different from doubling the weight of the powder —instead of increasing the pressure in a much greater ratio, as some have supposed.

The results of the experiments with the musket barrel accord, so far as they admit of comparison, with those just stated. The manner in which an increased charge of powder may be supposed to affect the pressure has already been referred to. The resistance of the forming gases in that part of the charge which is least confined, to the expansion of those in other parts of the charge, is perhaps most strikingly illustrated by the action of the fulminates, which, in quantities of a few grains, will, as is well known,

tear in pieces a brass or copper plate upon which they are gently heated.

The variations of pressure sustained by the gun when fired with charges very nearly the same are greater, as might be expected, than the variations of initial velocity imparted to the ball under similar circumstances.

When the combustion of the powder takes place with more than average rapidity, the pressure in the first instants of the explosion is augmented, but its action on the ball is not so well sustained as in the case in which the combustion is more slow and consequently longer continued.

In an article "On the Pressure of Fired Gunpowder in its Practical Applications," in the "American Journal of Science and Arts," September, 1856, Dr. Woodbridge, after reference to the experiments of Count Rumford, who estimated the pressure of gunpowder fired in a space which it filled at not less than 54,750 atmospheres—from an erroneous estimate of the strength of his eprouvette, which was burst by the charge—gives the following experiment, which seems to show that the extreme force of gunpowder fired in small quantities does not exceed 6,200 atmospheres:—"I inclosed in a hollow cylinder of cast-steel, $1\frac{1}{8}$ ins. in exterior diameter and one-fourth of an inch in diameter interiorily, 20 grs. of Hazzard's Kentucky rifle powder, which filled loosely the cavity. This was fired by a flash of powder penetrating through the aperture of a valve (of steel) opening inward, but designed to prevent the escape of gas outward. The cylinder was not ruptured, and, being put under water, no gas was found to escape; (the weight of the instrument was too great to test the loss of gas by my scales.) On pressing in the valve by means of a screw, an abundance of gas escaped, carrying with it the odour of sulphuretted hydrogen. The seat of the valve was found to remain perfect; a fact which, when compared with a former trial in which the gases escaped in consequence of a slight defect of the valve, is presumptive proof of its immediate action. The residuum was found to weigh 10·45 grs. The calculated strength of the cylinder would be equal to an internal pressure of about 93,000 lbs. per square inch, or 6,200 atmospheres of 15 lbs." Dr. Woodbridge regards the method of ascertaining powder pressures by the registered compression of a liquid as capable of greater accuracy than those which are based upon indentation or other change of form in pieces of metal acted on by a piston receiving the pressure to be measured.

NOTICES OF BOOKS.

Rhymes of Science: Wise and Otherwise. New York: Industrial Publication Company.

We have here a collection of poems referring to Science chiefly from a sarcastic point of view. There is "Sir Thomas the Good," extracted from the "Ingoldsby Legends;" there is the well-known "Society upon the Stanislow," and various other effusions of a kindred stamp. There is not, however, Peter Pindar's account of the insect-hunting adventures of Sir Joseph Banks, which we should strongly recommend to the compilers if they feel disposed to produce a companion-volume. The weak side of such verses is that their point, and even their very intelligibility, depend on allusions which are soon forgotten. Thus we can understand that the "De Sauty" of Dr. O. W. Holmes treats of the first laying of the Atlantic telegraph-cable. But what "Cyano-Rhinal" or "Ceruleo-Nasal" can mean is a mystery. We should take them to be "pet names" for that most disreputable of Primates known as *Cynocephalus mormon*. Again, we have the translation of a song given at the concluding dinner of the convention of German physicians and naturalists, headed "Science on a Bender." The allusion here escapes us. "Jim Green" is a rhyme of finance rather than of science.

Notes on Building Construction, arranged to meet the Requirements of the Syllabus of the Science and Art Department of the Committtee of Council on Education, South Kensington. Part III. Materials.—Advanced Course and Course for Honours. London, Oxford, and Cambridge: Rivingtons.

We have here a wonderful collection of information,—geological, chemical, physical, and even to some extent botanical and zoological,—together with much lore not falling within the boundary-lines of any of the sciences, the whole having a bond of unity in the fact of its being necessary for builders and architects. The author, whoever he may be, and the publishers must be congratulated, at least as far as we are able to judge, on the general accuracy of the work. The only questionable statement we have found is in the chapter on Pigments, where Persian red is stated to be a "chromate of lead, produced by boiling white

lead with a solution of bichromate of potash." The colour we have met with under this name is a compound of stannic acid and chrome, much more stable than any lead chromate, and approaching a madder-lake in its shade. Trade names, however, are used with such latitude that we should hesitate to pronounce the passage quoted to be erroneous.

A peculiarity of this book, if we consider the especial purpose for which it is written, is the retention of the "old" chemical nomenclature, for the very satisfactory reason that it is more generally intelligible. We should have thought, however, that South Kensington would have been prepared to fulminate the greater excommunication against the luckless wight who in these days should speak of carbonic acid as a constituent of the atmosphere.

We should consider the work an admirable manual of reference for builders, but the demands made upon the verbal memory of the unfortunates who have to be examined in it are simply fearful to contemplate.

An Introduction to the Systematic Zoology and Morphology of Vertebrate Animals. By A. MACALISTER, M.D. Dublin: Hodges, Foster, and Figgis. London: Longmans and Co.

BORROWING Earl Beaconsfield's critique on the poetry of Mr. R. Browning, we might couple with our appreciation of the merits of this work the wish that some good soul would translate it into English. Let us suppose it placed in the hands of a man well versed in the science of zoology as taught in the middle of the present century, but who had spent the last twenty years on some desert island. He would naturally expect to find that many new discoveries had been made, and that many notions accepted in his day had been given up. But to what extent such changes in the matter had taken place he would be unable to detect, on account of the still greater and more obtrusive revolution in the manner. He would find himself hopelessly lost in a fire-new terminology. Biology has not, indeed, like chemistry, given names of ten and twelve syllables in length to the individuals she describes. No one, fortunately, has yet conceived the idea of making the generic and specific names of an animal or plant embody its supposed line of descent. All this will come by-and-bye unless a vigorous stand be made. But for groups and classes of higher rank than genera, as well as for the parts of animals and for their structural attributes, the names selected are needlessly long and gratuitously un-English. We always presume that in every branch of natural science the great object of the teacher should be to concentrate the attention of the

student upon the facts, the phenomena described, and for that very reason to burden his memory as little as possible with words. Too many professors seem to be of the very opposite opinion. The slightest excuse is enough to justify the introduction of a new term. This tendency is particularly to be regretted in the present day. The division of labour, which has rendered such invaluable services in the industrial arts, assuredly must and will extend to the realms of science and learning. We have already numbers of eager students in biology who know "little Latin and less Greek," and their number will assuredly increase. Why, then, should treatises be written which such men cannot understand—if at all—without great loss of time? Prof. Macalister—who is neither the only, nor perhaps the greatest, sinner—seems conscious that his terminology is open to criticism. He remarks, in his Preface, that it is "impossible to write a strictly scientific treatise in popular language without increasing its bulk proportionally." Had we time we think we could undertake to render his book, not indeed into "popular language," but still into language such as a mere English scholar, guiltless of Greek, would clearly understand, and that without increase of bulk. Take, for instance, two ugly neologisms, arctiodactyl and perissodactyl—why not say simply even-toed and odd-toed? Here, by using plain English, we economise space without sacrificing intelligibility.

Birds of the Colorado Valley. A Repository of Scientific and Popular Information concerning North American Ornithology. By Elliott Coues. Part I.—Passeres to Laniidæ. Washington: Government Printing-Office.

We have here the first part of a most valuable work—one of the many collateral publications issued in connection with that wonderful undertaking, the United States Geological and Geographical Survey of the Territories. The author describes fully the structural characteristics, the habits, the geographical distribution, and the synonymy of every bird-species known to occur in the Colorado Valley. The region in question corresponds approximately with the "middle faunal province" of some authorities. It is bounded on the east by the main water-shed of the Continent, the Rocky Mountains, and of the Sierra Nevada on the east. Its northern limit is the Salt Lake Valley, and to the south it fades gradually into the great Neotropical region, many of its characteristic forms being still traceable on the table-lands of Mexico. The district offers wonderful variations of climate and surface, and altitude—as the author remarks—does the work of latitude. The characteristics of the work are accuracy and

thoroughness; its only defect, in our opinion, is a disposition to indulge in irrelevancies, as may be seen in the essay on the Cat-bird, very pleasant reading though it be.

The chapter on Swallows is admirable. The question whether certain individuals of the various species of this group, and of the swifts, do not occasionally hybernate, is discussed at great length; and the author, whilst fully admitting that the vast majority of these birds migrate, does not feel able to refute the evidence in favour of a minority passing the winter in northern climates in a state of torpor. The literature of this subject, chronologically arranged, extends to more than twelve pages, but curiously enough no mention is here made of Gilbert White, who repeatedly refers to the subject in his "Selborne."

The architecture of the swallows is also considered at some length. The interesting fact is brought into due prominence that, with one possible exception, all the species have modified the structure of their nests in accordance with the novel facilities afforded by the settlement of the country. The case of the common European house martin is still more instructive, since within the last fifty years it has changed the shape of its dwellings, not in accordance with any modification in the character of the site selected, but simply with a view to increased convenience.

Dr. Coues considers that the original and typical colour of the eggs of swallows, like those of hole-diggers in general, was a pure white, but that they have gradually become speckled as the nesting-habits of the bird have undergone modification.

Swallow-shooting the author denounces with just severity, and he sympathises warmly with these amiable and useful birds in the defensive warfare they are now obliged to wage against those "wretched interlopers" the European sparrows, whom some wrong-headed acclimatiser has, it appears, rashly introduced into America. We hear with regret that the same blunder has been repeated in Australia. Whilst upon the subject of acclimatising it may not be deemed an unpardonable digression if we record our disgust at learning that an Expedition, which ere this has doubtless sailed from Australia to New Guinea, is about to introduce goats into that country—for the purpose, we presume, of destroying its magnificent flora.

Space will not permit us to extend any further our notice of this work, which we must pronounce a sterling contribution to zoological science.

Proceedings of the Literary and Philosophical Society of Liverpool. Sixty-seventh Session, 1877-78. No. XXXII. London: Longmans and Co. Liverpool: D. Marples and Co. (Limited).

We have on former occasions noticed with regret that in the "Proceedings of the Liverpool Literary and Philosophical Society" science plays a part somewhat similar to that of the "one halfpenny worth of bread" in Falstaff's tavern bill. It might, indeed, have been hoped that under the successive presidencies of the Rev. H. H. Higgins and Dr. Drysdale, both of whom have honourably won their spurs in biological research, a better day might have dawned. Such, however, is not the case. A little work has, indeed, been done, and is noticed in the reports of the successive meetings. Thus Mr. T. Higgin, F.L.S., communicates an interesting notice of a fresh-water sponge (*Spongilla coralloides*) from the rapids of the River Uruguay. Mr. E. D. Jones, a corresponding member, residing at Sao Paulo, in Brazil, is making good progress in recording the metamorphoses of the Lepidoptera of the district: he has observed a caterpillar, belonging to the Bombycidæ, which, if annoyed, utters a low but distinct musical sound. Of fifty species of caterpillars which he has examined, twenty at least possess venomous spines: one of these, a very hairy species, of a bright orange, when applied to the back of the hand produced a most intense pain, which lasted for more than twelve hours, and extended to the arm-pit.

Mr. F. P. Marrat briefly describes a collection of shells from Fuca Straits and Cape Flattery, presented to the Liverpool Museum by Dr. D. Walker.

Mr. T. Higgin communicates notes on the polypidom of the Hydractinidæ, which attach themselves to the calcareous shell of a mollusk, and by some unknown process convert it into a skeleton for themselves.

Mr. J. Roberts describes the geological results of the borings at East Hoyle Bank, and the Rev. H. H. Higgins enters into some speculation on the peculiar structure of the jaws of *Angosoma Neptunus*.

When we turn, however, to the *pieces de resistances*, the papers selected for publication in full and forming the body of the volume, we find not even one composed of the results of experimental research or original observation.

The Presidential Address on the question "Is Scientific Materialism compatible with Dogmatic Theology?" has already come under our notice.*

* See Quarterly Journal of Science, vol. viii. (N.S.) p. 133.

A paper—or shall we say a sermon—in reply, by Mr. J. A. Picton, entitled "On Scientific Materialism from a Non-scientific Point of View" contains a very remarkable utterance:—"If there is one point more than another insisted on by Evolutionists it is the denial of the possibility of miracles." Would Mr. Picton be surprised to learn that the ablest reply extant to Hume's attempted demonstration of the impossibility of miracles is from the pen of one of our most eminent English Evolutionists? Perhaps, however, the word "Evolutionist" is used in some special—perhaps local—acceptation. This is the more probable because in a paper by the Rev. H. H. Higgins, entitled "Developmentalists and Evolutionists, or the Use of Dogma in Science," we find the term "Evolutionist" used in a sense which we never met with before, and which we cannot for one moment accept. It has been used as the common generic name, including alike Messrs. Lamarck, Darwin, Wallace, Haeckel, Asa Grey, St. George Mivart, J. J. Murphy, Oscar Schmidt, Leconte, J. J. Allen, Huxley, Spencer, Belt, Bates, and their followers. If this term is taken away and applied to some particular though not very clearly characterised section who are to be distinguished from certain "developmentalists," we have no common name left for all those naturalists who recognise development as opposed to individual creation.

The essays on the Credibility of Venerable Bede, on Amy Robsart, on the Proverbs of European Nations, on Moses Mendelssohn, and on Trevelyan's Macaulay, do not come within our cognizance.

To one, indeed, of these literary papers we feel compelled to refer. Mr. E. R. Russell, in his essay on Trevelyan's Life of Macaulay, gives utterance to these sentiments:—"He never thought it worth while to quit more attractive studies for the blind and groping physicism which now almost monopolises the name of Science. Whatever good it may have done in other directions, physical science has of late discouraged and debilitated moral and historical inquiry, which is of much more value to the world!" It has always seemed to us, as well as to far abler men, that in England at least—on account of the preponderant attention paid to history, to scholarship, oratory, and polite literature—physical science is neglected, and that by reason of this very neglect we are drifting more and more into the background as compared with some of our neighbours.

The passage we have quoted shows how little sympathy and compatibility exist between literature and physical science, and how injudicious, to say the least, is the attempt to form societies for their joint cultivation. Whereas in England public interest centres in words rather than in things, in criticism and commentation rather than in observation and experiment, literature will always succeed in engrossing more than the lion's share of

time and resources, as the volume before us but too plainly proves.

Practical Hints on the Selection and Use of the Microscope. Intended for Beginners. By JOHN PHIN, Editor of the "American Journal of Microscopy." Second Edition. New York. 1877.

THIS little book will prove of great use to those for whom it is intended. The construction of the microscope is treated in a simple manner, so as not to confuse the beginner. The hints as to the selection of an instrument are very good, and especially adapted to a country like America, inundated with foreign microscopes, good, bad, and indifferent. These remarks will help the student to avoid purchasing what is useless to him.

The portion devoted to manipulative processes is well compiled, the directions plain and easy to follow.

The Chemistry of Common Life. By the late JAMES F. W. JOHNSTON, F.R.S., &c. A New Edition, revised and brought down to the present date, by ARTHUR H. CHURCH, M.A., &c. London and Edinburgh: W. Blackwood and Sons. 1879.

THE late Prof. Johnston had a peculiar talent which is possessed by very few scientific writers. In common with Faraday, Liebig, Tyndall, Huxley, and a few others, he enjoyed the power of being able to clothe the dry bones of scientific fact with the warm and living flesh of lucid exposition and apt illustration.

It is now nearly twenty years since the last edition of Prof. Johnston's work was published, under the editorship of the late Mr. G. H. Lewes. Prof. Church, however, has had a great advantage over his predecessor, inasmuch as he had the opportunity of consulting Prof. Johnston's private and corrected copy of the "Chemistry of Common Life." Little has been added to Prof. Johnston's original work, except in the way of correcting certain figures and statements. Prof. Church has performed his task of revision with great judgment. He tells us in the Preface that he has been compelled to add one entirely new chapter, which no doubt was suggested by the perusal of Prof. Johnston's own notes. It is entitled "The Colours we Admire." In this chapter Prof. Church tells how brilliant colours of all hues have been got out of the coal-scuttle; how chemists have entered into competition even with the great Cybele herself, and have produced alizarin in the test-tube with as great facility as

the faithful goddess herself has done in the madder-root; how copper exists in turacin, the colouring-matter of the beautiful crimson wing-feathers of the touraco, a West African bird, and iron in the hæmoglobin of the blood of mammals. We also have a description of chlorophyll and colein, the latter a colouring-matter discovered by Prof. Church in the leaves of the *Coleus Verschaffeltin*. This chapter might have been extended with great advantage. A copious index adds much to the value of the work.

CORRESPONDENCE.

MENTAL INANITION.

To the Editor of the Monthly Journal of Science.

Sir,—May I ask whether sufficient attention has been paid to the following curious analogy? It is well known that external agencies, such as malaria, make the most prompt and deep impression upon the human body when in a state of inanition. In like manner, objects seen, events witnessed, or words overheard when we are in a state of mental inanition, stamp themselves upon the memory with a permanence out of all proportion to their interest or importance. Thus, having once to walk for about 10 miles through a most uninviting country, on a very dull day, in order to escape the greater evil of waiting for two hours at a railway-station, I saw a butcher washing out an inkstand in the street just outside his shop. It is difficult to conceive a more insignificant occurrence, and yet it has remained as if burnt into my memory. I could give other matters equally trifling, remembered because at the time of their happening my mind was, so to speak, hungry for impressions.—I am, &c.,

S.

THE SEA-SERPENT.

To the Editor of the Monthly Journal of Science.

Sir,—Referring to "Sceptic's" question respecting the Sea-Serpent, in your April number, there is no doubt that Captain Drevar and his mate, of the barque *Pauline*, deposed to the truth of their story, of having seen a sea-serpent, before a magistrate at Liverpool. Capt. Drevar's story is related by the Rev. E. L. Penny, Chaplain to H.M.S. *London*, at Zanzibar, in the "Illustrated London News" for November 20, 1875.—I am, &c,,

M. H. Close.

40, Lower Baggot Street, Dublin,
April 23, 1879.

PROCEEDINGS OF SOCIETIES.

Royal Society, *April* 3.—"On the Thermal Conductivity of Water," by J. T. Bottomley. The experiments described in this paper were conducted by a method devised by Sir William Thomson. The liquid whose thermal conductivity was to be determined was heated from above, to avoid convection currents. Two methods of heating have been used. In one, a horizontal steam chamber was applied at the top of the water or other liquid, and, steam being continuously passed through the heating chamber, the surface of the liquid under experiment was kept at a very high temperature, and heat was conducted from above downwards. In the other method a large quantity of very hot water was deposited on the top of a mass of cold water, mixing being prevented by a simple contrivance, and the heat of this superincumbent layer was conducted downwards through the colder water below. The experiments were carried on in very large vessels, or tanks, in order to avoid disturbance by means of loss of heat at the sides. Three principal thermometers were employed; together with a fourth, whose object was merely to show when heat begins to be lost at the bottom of the layer of fluid experimented on. When this loss commences the experiment is at an end. The other three thermometers were used thus:—First there was a thermometer with a bulb 30 centims. long, which was placed vertically, its object being to show the average temperature from top to bottom of the layer of fluid bounded by horizontal planes passing through the top and bottom of its bulb. The rise of this thermometer in any time shows the quantity of heat that has passed into the stratum occupied by it in that time. The other two thermometers were placed with their bulbs horizontal, and one at a known distance vertically above the other. They indicate the temperatures of the layers in which they are placed. Knowing the difference of temperatures of two sides of a stratum of a liquid during any time, and the quantity of heat conducted across the stratum during that interval of time, the thermal conductivity of the liquid can be calculated. The result arrived at by the experiments described is that the thermal conductivity of water may be taken at from 0·0022 to 0·00245 in square centimetres per second. The author has made some experiments on the thermal conductivity of solution of sulphate of zinc. These experiments are now being carried on with the assistance of a grant from the Government Fund of £4000.

"Note on the unknown Chromospheric Substance of Young," by G. D. Liveing, M.A., Professor of Chemistry, and J. Dewar, Jacksonian Professor, University of Cambridge. In the preliminary catalogue of the bright lines in the spectrum of the chromosphere published by Young in 1861, he calls special attention to the lines numbered 1 and 82 in the catalogue, remarking that "they are very persistently present, though faint, and can be distinctly seen in the spectroscope to belong to the chromosphere, as such, not being due, like most of the other lines, to the exceptional elevation of matter to heights where it does not properly belong. It would seem very probable that both these lines are due to the same substance which causes the D_3 line." Again, in a letter to "Nature," June, 1872, Young says, "I confess I am sorry that the spectrum of iron shows a bright line coincident with 1474 (K); for, all things considered, I cannot think that iron vapour has anything to do with this line in the spectrum of the corona, and the coincidence has only served to mislead. But there are in the spectrum many cases of lines belonging to the spectra of different metals coinciding, if not absolutely, yet so closely that no existing spectroscope can separate them; and I am disposed to believe that the close coincidence is not accidental, but probably points to some physical relationship, some similarity of molecular constitution perhaps, between the metals concerned. . . . So, in the case of the green coronal matter, is it not likely that though not iron it may turn out to bear some important relation to that metal?" In 1876 he proves that the coronal line 1474 is not actually coincident with the line of iron. In the catalogue of bright lines observed by Young at Sherman, in the Rocky Mountains, it appears that the above-mentioned lines 1 and 82, along with D_3, were as persistently present as hydrogen, the only other line approaching them in frequency of occurrence being the green coronal line 1474 of Kirchhoff, which was present on ninety occasions out of a hundred. It occurred to the authors that these four lines may belong to the same substance. An analogy in the ratio of the wave-lengths of certain groups of lines occurring in different metals has been already pointed out by Stoney, Mascart, Salet, Boisbaudran, and Cornu; and the near coincidence observed by the authors in the ratios of certain lines of hydrogen, lithium, and magnesium, substances belonging to the same type, combined with a similar ratio in the wave-lengths of the nearly equally persistent lines of the chromosphere, greatly strengthens the probability of the assumption that these lines belong to one substance. The fact that the two less refrangible rays have no representative in the Frauenhofer lines is by no means opposed to their belonging to one substance, since aluminium behaves in a similar way in the atmosphere of the sun; and in the total eclipse of 1875 the hydrogen line *h* was not visible in the chromosphere, but was on the limit between

brightness and reversal; and during the late eclipse the two most refrangible rays of hydrogen were not detected from the same cause.

Chemical Society, *April* 3.—Mr. G. Attwood read a paper "On a Gold Nugget from South America." In the State of Guayana, Venezuela, a large area of alluvial soil has lately been found to contain gold, and nuggets up to 25 ounces have been discovered within 3 feet of the surface. Numerous gold-bearing quartz veins are found in the neighbouring hills. Quite one-half of these nuggets are covered with a dark brown substance, resembling a silicate of iron. Such a nugget was treated with hydrochloric acid (its weight diminished, after treatment with HCl and NaO, from 304·7 grains to 284·33 grains). The solution contained—Silica, 0·12 gr.; ferric oxide, 8·88; lime, 0·15; magnesia, 0·08. The nugget was then treated with caustic soda, and again with HCl. The solution contained—Silica, 4·66 grs.; ferric oxide, 4 60; lime, 0·21. During this process much gold in a finely divided state became detached, and after the treatment the nugget was partly covered with a coating of finely divided gold, of a dull colour. The nugget contained 94·54 per cent of gold. The gold from the quartz veins contains 87·9 per cent gold. From these experiments the author concludes that gold nuggets gradually increase in size, owing to the accumulation of fresh particles of finely precipitated gold. Specimens of these nuggets showing the dark glazed coating were exhibited, including one weighing over 14 ounces.

Institution of Civil Engineers, *March* 25.—Mr. Bateman, President, in the chair. The paper read was on "The Electric Light applied to Lighthouse Illumination," by Mr. J. N. Douglass, M. Inst. C.E. The author showed the progress of lighthouse luminaries from wood and coal fires to the introduction of tallow candles, fatty oils, mineral oils, coal gas, and electricity. In 1839 experiments were made by Faraday, for the Trinity House, at the Orford Low Lighthouse, with the Bude light, and, in 1862, at the South Foreland with the Drummond or limelight, but the results were not so satisfactory as to lead to their adoption. In 1857 experiments were tried at Blackwall with the electric light, produced by the first magneto-electric machine of Holmes; and on the 8th of December, 1858, the electric light, obtained by Holmes's second machine and a "Duboscq" lamp, was shown on the sea for the first time from the South Foreland High Lighthouse. On the 1st of February, 1862, the Trinity House exhibited the electric light permanently at Dungeness Lighthouse, by Holmes magneto-electric machines and lamps. The works for the production of the light were described, and the first cost and maintenance given. The intensity of the electric luminary was about 12½ times that of the oil

luminary. The cost per unit of light per hour was 0·1165d. for the oil, and 0·1294d. for the electric light. Frequent falling off of the latter light occurred, and the oil light had occasionally to be substituted. Shortly afterwards the French lighthouse authorities established the electric light at Cape La Hêve, with the magneto-electric machines of the Alliance Company of Paris. In 1867 Holmes further improved his machine and lamp. Two of these machines and lamps were exhibited by the Trinity House at the Paris Exhibition of 1867, with a dioptric apparatus of the third order. The Souter Point Lighthouse was lighted by electricity in January, 1871. The light was adapted to a dioptric apparatus of the third order. A lower light from the same luminary as the upper one was adopted here for the first time. The optical apparatus for both lights was designed by Mr. James T. Chance, M.A., Assoc. Inst. C.E. A "Holmes" fog trumpet apparatus was also worked from the same engines as the electric light apparatus. The total cost of the works was £18,000. The cost of the electric luminary per candle per hour was 0·056d., being rather less than half of the cost per unit at Dungeness. The maximum intensity of the beam from this apparatus was about 700,000 candles. The Trinity House next established the electric light at the South Foreland High and Low Lighthouses, in January, 1872. The apparatus for the production of the light consisted of two steam engines of 20 effective horse-power, four Holmes's improved magneto-electric machines and lamps, and two dioptric apparatuses of the third order for fixed white light. The cost of the additional works for these lights was £14,800. The intensity of the full power beam from the High Lighthouse was about 20 times that of the old first order dioptric oil light. The relative cost per unit of light was as 100 oil to 30·6 electric. In 1873 the Trinity House adopted, for the Lizard Lighthouses, Siemens's dynamo-electric machine and lamp, and a siren fog signal. These were driven by three of Brown's caloric engines, each of 10 effective horse-power. The cost of the additional works for these lights was £14,936, and the annual maintenance, including interest on first cost, amounted to £2,365 6s. 4d. against £1,016 7s. 11d. for the oil lights. The intensity of the full power beam of each light was about 330,000 candles, being about 211½ times the intensity of the oil light. The relative cost per unit of light was 100 oil to 14·04 electric. The successive improvements in the electric machines, and in the means of driving them, had reduced the cost of the electric light at the Lizard to one-ninth of that at Dungeness, and the quantity of light produced at the Lizard per pound of coke consumed was increased 20 times. The continued growth seaward of the shingle point at Dungeness led to the removal, in 1876, of the original electric light apparatus, and the substitution of a low flashing oil light and siren fog signal for both lighthouses. The author furnished information received

from M. Allard, Director-General of the French Lighthouses, relative to the electric lighthouses at Cape La Hêve and Cape Grisnez, where Alliance magneto-electric machines and Serrin lamps had been adopted. It was intended to exhibit the electric light in the present year in a new lighthouse on the Isle of Planier, opposite Marseilles, and it had been decided to substitute the electric light for the oil light in the Palmyre Lighthouse at the mouth of the Gironde. Some information was also given relative to the electric lighthouses at Odessa and Port Said, these making ten in which the electric light had already been established. The comparative cost and efficiency were shown of lighthouse luminaries produced by all the agents at present employed, viz., oil, coal gas, and electricity. Coal gas, on Wigham's system, was applied to the Howth Bailey Lighthouse, Dublin Bay, by the Commissioners of Irish Lights, in June, 1865, and it had since been extended to seven lighthouses on the coast of Ireland. In 1872 the Trinity House adopted it at the Haisborough High Lighthouse. The additional cost of the works necessary for the introduction of gas at this station was £1,996, and the annual maintenance of the gas establishment, including interest on first cost, &c., amounted to £832 4s. 3d. This light had a mean intensity of 1,173 candles, and a maximum intensity for thick weather of about 2,923 candles. In 1877 the necessary additions were made for lighting the Low Lighthouse (distant 776 yards), by gas from the same works, at a cost of £1,296. The system of Mr. Wigham had been further developed by introducing the flames of two, three, and four large burners over each other in the axis of the dioptric apparatus. In January of last year the Commissioners of Irish Lights adopted one of the latter lights in a new lighthouse at Galley Head, near Kinsale. The maximum intensity of the four burners combined for thick weather was about 5,012 candles. The author next gave statements showing the comparative focussing compactness of the lighthouse luminaries which had been referred to for utilising in optical apparatus, viz., the lights produced by oil, coal gas, and electricity. The focussing superiority of the electric luminary, compared with the best of these, was as 616 to 1. Statements were given of the comparative average cost and annual maintenance of a single lighthouse (shore station) in this country, with colza oil, mineral oil, coal gas, and electricity, as the illuminating agents, both with and without a first-class 20 horse-power siren fog signal. For a maximum degree of light equal to the single or combined intensity of the luminaries of the Lizard, the cost of the more perfect electric luminary per unit of light provided was about 13·22 and 6·22 respectively of that of coal gas, and about 13·65 and 6·65 respectively of that of mineral oil, at their maximum intensities. With higher intensities of the electric luminary the cost per unit would be more in its favour, no further addition to the working staff being

necessary. From experiments by Faraday for the Trinity House, in 1836, relative to the penetrative power of lights, through such obstructions as fog, mist, &c., and the more recent experiments by the French Lighthouse authorities and by the Trinity House, with oil and electric lights, it might be assumed that, with the atmosphere so impaired for the transmission of light, that the oil luminary at its maximum intensity would be fairly visible at the fog signal range of two miles, the electric luminary at its double Lizard intensity of 16,500 candles would be visible at about four miles. Further, that on more frequent occasions, when the oil luminary would be visible at about eight miles and a-half, the electric light would be visible at the full range of 17 miles.

April 22.—Mr. Bateman, President, in the chair.

The paper read was on "Dioptric Apparatus in Lighthouses for the Electric Light," by Mr. James T. Chance, Assoc. Inst. C.E. The author briefly premised that in the Fresnel or dioptric system the source of light occupied the central position within a structure of glass zones, or annular segments, by which the incident rays were condensed and directed on the sea; and that there were two principal kinds of dioptric apparatus, the fixed and the revolving. He then proceeded to make some observations concerning the different optical treatment which a small radiant like the electric arc required from that which suited an ordinary flame; in the latter case, as—for it concerned sea-lights—the object was not only to parallelise all the rays emanating from any point of the luminary, but also to reduce the vertical divergence due to the height of the flame by increasing the diameter of the optical instrument. On the other hand, the smallness of the electric arc afforded the opportunity of obtaining from the dioptric zones or other elements, by suitable generating sections, whatever divergence, whether horizontal or vertical, might be desired. It was also pointed out that the source of light, in the case of the electric arc, could not be entirely depended upon for maintaining the same position in relation to the focal horizontal plane, and that consequently—since the vertical divergence due to the luminary would move upwards or downwards with any vertical displacement of the radiant itself—the mariner could not be absolutely secured from failing to see the light, unless a special vertical divergence were given by the dioptric apparatus, independently of that caused by the size of the electric arc. This, however, involved the adoption for this illuminant of a dioptric instrument considerably larger than what was originally contemplated, so as to reduce materially the luminary divergence, and thereby be free to substitute for it, to some extent, a special vertical divergence. The author stated that in 1862 he had expressed himself in favour of a much larger apparatus than was then employed with the electric light at Dungeness. Also that,

in 1865, Messrs. D. and T. Stevenson had recommended a third order apparatus for the purpose, in their Report to the Commissioners of Northern Lighthouses. A similar result was arrived at by the Elder Brethren of the Trinity House, in 1869, in consequence of comparative trials instituted by Prof. Tyndall, for testing the relative merits of a sixth order light, and a third order one respectively, when used with the electric radiant. The Souter Point revolving light, which was first exhibited in January, 1871, was described. Reasons were assigned for adopting two optical agents—one to condense the light in the vertical plane, the other to produce the required horizontal compression—instead of attempting, even for the refracting part of the apparatus, to effect the two condensations respectively by a single agent. Reference was made to the proposal of Mr. Thomas Stephenson, M. Inst. C.E., for attaining this latter desideratum; as likewise to that of Mr. Brebner, M. Inst. C.E., with a similar object. The method actually adopted was similar to that which had been already employed in France for the revolving light with the electric arc. It consisted of a fixed third order light encircling 180°, and of a rotating octagonal drum of the same height surrounding it. Each side of this drum, comprising three panels in height, was composed of vertical refracting prisms, by which the light, radiating in azimuth from the inner fixed apparatus, was compressed horizontally into a beam of 7° 8′ divergence in addition to that due to the diameter of the electric arc. This was done in such a manner that every single prism had its own independent divergence of the same amplitude, whereby was obtained an extent of light-emitting surface of a height of 6½ feet and of 22½ inches in breadth. Stress was laid upon its being the characteristic feature of the beam issuing from any one of the sides of this glass drum, that, in passing before the eye of the observer at sea, its brilliancy would, from first to last, remain unchanged, as distinguished from the waxing and waning appearance of the ordinary revolving light; consequently, at whatever distance the flash might be visible, the interval of its duration would be the same. Attention was also directed to the valuable suggestion made by Mr. J. N. Douglass, M. Inst. C.E., the Engineer to the Trinity House, that advantage should be taken of the landward hemisphere of the radiant light of the electric arc, to provide a beam which should be made to issue through a window in the tower below the main light, in order to mark certain dangers in Sunderland Bay; and it was stated that 54·6 per cent of the rearward hemisphere of light had been thus utilised. The two fixed lights which were inaugurated at the South Foreland, in January, 1872, were described. It was explained how the whole of the catadioptric zones, both upper and lower, were in both lights made to parallelise the rays in the usual manner. The light, however, incident on the refracting portion of each light was distributed over the sea from the horizon to within a

short distance from each tower, by a succession of increasing angles of vertical divergence, so that the illumination of the sea became gradually diminished as the distance from land was lessened. In each light there was a rearward arc to spare, and this was turned to valuable account, from 67 to 71 per cent of this light being collected and acted upon by optical agents, which were particularly described, and thereby distributed uniformly over the front azimuthal arc, so as to intensify not only the illumination of the horizon and the distant sea, but also that of the nearer sea. It was mentioned that the two Lizard Lights, which were both fixed, and were first exhibited in March, 1878, had optical arrangements similar in every respect to those adopted at the South Foreland Lighthouses, with a slight variation in the refracting portions, arising from the circumstance that existing apparatus had to be turned to account in the construction of each apparatus. A table was appended, showing the condensing powers in the direction of the horizon of the lights described in the paper, distinguishing those optical portions which parallelised the incident light from those which gave to it special vertical divergence. According to this table, upon the assumption that the diameter of the electric arc was 12 millimetres, the condensing powers in the sea-horizon direction were as follow:—

Souter Point—Revolving	236·38
South Foreland—High fixed	50·17
South Foreland—Low fixed	43·40
Two Lizard Lights—Fixed	58·44

and data were added for adapting this table to particular cases. In a second table was given the respective condensing powers over the near sea, at different distances from the lighthouse towers.

Meteorological Society, *February* 19.—Mr. C. Greaves, President, in the chair.

Among the papers read were the "Diurnal Variations of Barometric Pressure in the British Isles," by Frederick Chambers. The object of this paper is to show that differences of types of the diurnal variations of pressure at inland or sea-coast stations are due to the superposition, on a common type of diurnal variation at all the stations, of a distinct diurnal variation of barometric pressure, such as is required to satisfy the convection-current theory which explains the well-known diurnal land and sea breezes. To show this, all that is necessary is to take the differences of the corresponding hourly inequalities of the barometric pressure at pairs of inland and coast stations, and to exhibit these differences in the form of curves, which are then found to closely resemble the curves of diurnal variation of air temperature.

"On the Relation existing between the Duration of Sunshine, the amount of Solar Radiation, and the Temperature indicated by the Black Bulb Thermometer *in vacuo*," by G. M. Whipple, B.Sc., F.R.A.S. The author has instituted a comparison between the duration of sunshine, as determined by Campbell's sunshine recorder, and the amount of solar radiation, as ascertained from the readings of the black bulb thermometer *in vacuo*, for the year 1877, at the Kew Observatory. It is evident that there is a close relation between these phenomena, but, owing to the great range of the black bulb thermometer, the exact nature of the connection is not immediately evident. The author says that it may be safely concluded that the measure of solar radiation, as given by the black bulb thermometer, is only to be considered at any place as an indication of the relative presence or absence of cloud from the sky at the locality, and so its use as a meteorological instrument may with advantage be set aside in favour of the sunshine record, which has not the elements of uncertainty attached to it, inseparable from the former instrument.

March 19.—Mr. C. Greaves, President, in the chair.

The papers read included one on "Dew, Mist, and Fog," by George Dines, F.M.S. The author has, during the last two years, made a number of experiments to determine the amount of dew that is deposited on the surface of the earth. The plan adopted was as follows:—Glasses similar to ordinary watch-glasses were procured; the surface area and the weight of each was ascertained. These glasses were exposed to the open air in the evening, being placed on different substances, viz., on grass, on slate, and on a deal board, the two latter being raised a few inches above the grass. A minimum thermometer was generally placed by the side of each glass. It is only on rare occasions that an amount of dew exceeding 0·010 inch in depth has been deposited upon the measuring glasses, and out of 198 observations in only three has that amount been exceeded: 58 observations give the amount from 0·010 to 0·005 inch, 107 from 0·005 to 0·001 inch, 22 less than 0·001 inch, and 8 observations no dew at all. The author thinks it may be fairly assumed that the average annual deposit of dew upon the surface of the earth falls short of 1·5 inch. There are two kinds of mist, the morning and evening. The morning mist is caused by the evaporation from the water and the moist ground taking place faster than the vapour is taken away; the air becomes saturated, but this does not stop the evaporation; the vapour continues to rise into the air, is there condensed, and forms mist, which gradually spreads over a wider surface. The evening mist is produced as follows:—The cold on the grass, caused by radiation, lowers the temperature of the air above it; the invisible vapour of water previously existing in the air is in excess of that which the air can retain when the temperature is lowered; the surplus is condensed, becomes a

mist cloud, and floats in the air just above the surface of the grass. Taken either separately or combined, the mists appear to the author totally and altogether inadequate to account for those dense fogs which at times overspread large tracts of country. Dense fogs near the earth are often accompanied by a clear sky above, when the sun may be seen reflected from the gilded vanes of our public buildings. After long consideration the author is inclined to attribute these fogs to some cause at present unknown to us, by which the whole body of the air to some distance above the surface of the earth is cooled down, and as a consequence part of the vapour of that air is condensed, and forms what has been called an "earth-cloud."

"On the Inclination of the Axes of Cyclones," by the Rev. W. Clement Ley, M.A., F.M.S. In this paper the author calls attention to the evidence recently afforded by the results of mountain observations to the theory that "the axis of a cyclone inclines backwards." The author first reviews the state of the question up to the present time, and details his own investigations, chiefly founded upon the movement of cirrus clouds; he then refers to Prof. Loomis's recent "Contributions to Meteorology," in which is discussed the observations at the summits and bases of several high mountains, the results of which fully confirm the theory that the axis of a cyclone inclines backwards.

Royal Institution of Great Britain, *February* 21.—"A New Chemical Industry, established by M. Camille Vincent," by Prof. Roscoe, LL.D., F.R.S. Dr. Roscoe, after referring to the experiment by which ammonia was first obtained by Joseph Priestley in the gaseous state, and to the various ammoniacal discoveries which have been made during the last hundred years, remarked that in the French beet-root sugar industry, as in the manufacture of cane sugar, large quantities of molasses or treacle remain behind after the whole of the crystallisable sugar has been withdrawn. These molasses are invariably employed to yield alcohol by fermentation. The juice of the beet, as well as that of cane-sugar, contains, in addition to the sugar, a large quantity of extractive and nitrogenous matters, together with considerable quantities of alkaline salts. In some sugar-producing districts the waste liquors or spent wash from the stills—called *vinasses* in French—are wastefully and ignorantly thrown away, instead of being returned to the land as a fertiliser, and thus the soil becomes impoverished. In France it has long been the custom of the distiller to evaporate these liquors (*vinasses*) to dryness, and to calcine the mass in a reverberatory furnace, thus destroying the whole of the organic matter, but recovering the alkaline salts of the beet-root. In this way 2000 tons of carbonate of potash are annually produced in the French distilleries. For more than thirty years the idea has been entertained of collecting the ammonia-water, tar, and oils which are given

off when this organic matter is calcined, but the practical realisation of the project has only quite recently been accomplished, through the persevering and sagacious labours of M. Camille Vincent, of Paris. The following is an outline of the process as carried out at the large distillery of Messrs Tilloy, Delaune, and Co., at Courrières. The spent wash having been evaporated until it has attained a specific gravity of 1·31, is allowed to run into cast-iron retorts, in which it is submitted to dry distillation. This process lasts four hours; the volatile products pass over, whilst a residue of porous charcoal and alkaline salts remains behind in the retort. The gaseous products given off during the distillation are passed through coolers, in order to condense all the portions which are liquid or solid at the ordinary temperature, and the combustible gases pass on uncondensed and serve as fuel for heating the retorts. The liquid distillate from the spent wash may be divided into—

1. The ammonia water.
2. The tar.

The ammonia water of the vinasse resembles that of the coal-gas manufacture in so far as it contains carbonate, sulphydrate, and hydrocyanide of ammonia; but it differs from this (and approximates to the products of the dry distillation of wood) by containing in addition methyl alcohol, methyl sulphide, methyl cyanide, many of the members of the fatty acid series, and, most remarkable of all, *large quantities of the salts of trimethylamin.* The tar, on re-distillation, yields more ammonia water, a large number of oils, the alkaloids of the pyriden series, solid hydrocarbons, carbolic acid, and, lastly, a pitch of fine quality. The crude alkaline aqueous distillate is first neutralised by sulphuric acid, and the saline solution evaporated, when crystals of sulphate of ammonia are deposited; and these, after separating and draining off, leave a mother-liquor, which contains the more soluble sulphate of trimethylamin. During the process of concentration, vapours of methyl alcohol, methyl cyanide, and other nitriles are given off, these being condensed, and the cyanide used for the preparation of ammonia and acetic acid by decomposing it with an alkali. Trimethylamin itself is at present of no commercial value, but M. Vincent finds that the hydrochlorate of trimethylamin, when heated to a temperature of 260°, decomposes into (1) ammonia, (2) free trimethylamin, and (3) chloride of methyl, and both ammonia and chloride of methyl are substances possessing a considerable commercial value. The latter compound can be employed as a means of producing artificial cold, and for preparing methylated dyes, which are at present costly, inasmuch as they have hitherto been obtained by the use of methyl iodide, an expensive substance. As a means of producing low temperatures chloride of methyl will prove of great service both in the laboratory and on a larger industrial scale.

When the liquid is allowed to escape from the receiver into an open vessel it begins to boil, and in a few moments the temperature of the liquid is lowered by the ebullition below -23°, the boiling-point of the chloride. The liquid then remains for a length of time in a quiescent state, and may be used as a freezing agent. By increasing the rapidity of the evaporation by means of a current of air blown through the liquid, or better by placing the liquid in connection with a good air-pump, the temperature of the liquid can in a few moments be reduced to -55°, and large masses of mercury easily solidified. M. Vincent has recently constructed a freezing machine which will probably compete favourably with the ether and sulphurous acid freezing machines now in use, as it can be simply constructed, and as the vapour and liquid do not attack metal and are non-poisonous, the frigorific effects which it is capable of producing being moreover most energetic. The second and perhaps more important application of methyl chloride is to the manufacture of methylated colours. The application of methyl chloride to the preparation of violets and greens is not, however, due to M. Vincent. His merit is in establishing a cheap method by which perfectly pure chloride of methyl can be obtained, and thus rendering the processes of the manufacture of colours much more certain than they have been hitherto. Dr. Roscoe concluded his discourse by referring to this as another instance of the utilisation of waste chemical products and of the preparation on a large scale of compounds hitherto known only as chemical rarities.

NOTES.

Biology.

In a paper on the development of the ovaries in female mammals after birth, read before the Academy of Sciences, M. Ch. Rouget states that his researches have led him to conclusions agreeing with those of Kölliker. His observations support the theory of primordial hermaphroditism.

M. Chatin describes, in the "Comptes Rendus," the special nutritive apparatus of phanerogamic parasites. He finds that the suckers of such parasites are morphologically and physiologically analogous to the roots of ordinary plants.

Sarracenia purpurea contains a proximate principle closely analogous to that of the Colchicaceæ, although belonging to a remote family bordering on the Papaveraceæ.

M. Maxime Cornu points out the existence in the Crassulaceæ of cortical woody bodies agglomerated together, and apparently serving to strengthen the fragile stems destined to support an efflorescence consisting of numerous flowrets.

M. Dareste has observed, in the yolk of egg, certain granules which have generally been supposed to be lecithine, but which in their physical and chemical characters agree rather with starch.

M. Couty has made known the results of observations on the non-excitability of the grey cortical substance of the brain, which, he finds, takes no part in the phenomena produced by exciting the surface of the brain. These phenomena remain the same whether the grey layer is intact or paralysed by an anæsthetic.

M. Arm. Moreau infers, from experiments performed on dogs, that the analogy supposed to exist between the phenomena induced by the presence of certain saline solutions in the intestinal canal and the phenomena which have been studied outside the animal system by the aid of the endosmometer cannot be maintained.

MM. Bergeret and Moreau propose water slightly acidulated with nitric acid as a remedy for the *Peronospora* infesting the lettuce.

M. A. Certes recommends the vapours of osmic acid for the preservation of Infusoria.

According to M. S. Jourdain, in *Arion rufus* the arterial blood issues into the visceral cavity from funnel-shaped orifices at the extremities of the minute arteries, and is afterwards taken up by

the veins, there being—as M. Milne-Edwards has shown to be the case in various regions of the body of the Mollusca—no system of capillaries establishing a continuity between the arterial and the venous systems. Similar orifices exist in many other Mollusca.

According to the "Revue Internationale des Science" a species of tortoise is found in China in which the head, neck, legs, &c., are covered not with scales, but with long and dense hair. The locality of this singular being is a lake in the province of Kiang-su. It is from 7 to 8 centimetres in length, with a prolonged snout, and a tail long and narrow.

Prof. Naegeli, of Munich, maintains that infectious and malarious diseases are due to *Schizomycetes*, which, however, are not all specifically distinct, but represent forms of one, or at most of a limited number of species.

M. P. Arnoux has brought to France a specimen of honey found in certain subterranean cavities in Ethiopia, and said to be collected by an insect "resembling a large mosquito." It is unaccompanied by wax, and differs from ordinary honey by being free from cane-sugar. We can scarcely believe that it is elaborated by a dipterous insect.

M. Ch. Musset has described to the Academy of Sciences a shower of sap which he observed, on the 22nd of August last, falling from two trees of *Abies excelsa*. Clouds of insects had collected to the feast.

M. Jousset de Bellesme has shown that the so-called liver of the Cephalopa presents no functional analogy with the liver of the Vertebrates. It transforms albuminoid matters, but is without action on fatty and starchy bodies. The author has already demonstrated the same fact concerning the liver of Crustaceans, and M. Plateau has reached a similar conclusion regarding the Arachnidæ and Myriapoda.

M. Merget, of Bordeaux, finding that mercurial vapour easily permeates disks of wood, recommends it as a means of studying the structure of vegetable matter. If wood, after exposure to the vapours of mercury, is brought in contact with a sensitive paper (obtained by saturating paper with an ammoniacal solution of nitrate of silver) a distinct design of the fibro-vascular bundles and of the medullary rays will be obtained. We may thus design the stomata of a ieaf, and show that in case of those possessing stomata on both surfaces the air circulates from one epidermis to the other.

Chemistry and Technology.

Mr. A. Percy Smith communicates to the "Chemical News" a case of poisoning by arsenical wall papers. A lady in Rugby

complained of a peculiar feeling of constriction in her nose, accompanied by a "nasty smell," nausea, and loss of appetite, and was unable to discover the cause. The symptoms had been felt since sleeping in a certain room, which she had done for two months. Mr. Smith analysed the paper, and found 0·35 grm. As_2O_3 per square foot; but that scarcely represents the total amount, because the paper was wetted in order to remove it, and the greater proportion of the bright green flowers washed off. There was very little copper present. The lady's husband was not affected in the slightest degree.

Writing to the "Chemical News" on the analysis of Australian *Eucalypti*, Mr. R. D. Adams, of Sydney, N.S.W., says that the celebrated *Eucalyptus globulus* is by no means the richest in the essential oil which gives the peculiar sanitary value to this class of trees, although it is the most "taking" in its earlier stages, from the rapidity of its growth and fulness of foliage. It is the *E. amygdalina* which yields more volatile oil than any other tested, and which therefore is largely chosen for distillation; thus it is also one of the best for subduing malarian effluvia in fever regions, although it does not grow with quite the same ease and celerity as *E. globulus*. The respective hygienic value of various Eucalypts may to some extent be judged from the percentage of oil in their foliage, as stated below, and as ascertained by Mr. Bosisto at the instance of Baron Ferdinand von Mueller, Director of the Victorian Botanical Department for the Exhibition of 1862:—

E. amygdalina	3·313	per cent volatile oil.
E. oleosa	1·250	,, ,,
E. leucoxylon	1·060	,, ,,
E. goniocalyx	0·914	,, ,,
E. globulus	0·719	,, ,,
E. obliqua	0·500	,, ,,

The lesser quantity of oil of *E. globulus* is, however, compensated for by the vigour of its growth and the early copiousness of its foliage. The proportion of oil varies also somewhat according to locality and season. *E. rostrata*, though one of the poorest in oils, is nevertheless important for malarian regions, as it will grow well on periodically inundated places, and even in stagnant water not saline. *E. oleosa* (F. v. M.), from the desert regions of extra-tropic Australia, might be reared on barren lands of other countries for the sake of its oil. According to Mr. Osborne's experiments, *Eucalyptus* oils dissolve the following, among other substances, for select varnishes and other preparations:—Camphor, pine-resins, mastich, elemi, sandarac, kauri, dammar, asphalte, xanthorrhæa-resin, dragon's blood, benzoe, copal, amber, anime, shellac, caoutchouc, also wax, but not gutta-percha. These substances are arranged here in the order of their great solubility. The potash obtainable from the ashes of various

Eucalypts varies from 5 to 27 per cent. One ton of the fresh foliage of *E. globulus* yields about 8½ lbs. of pearlash; a ton of the green wood, about 2¼ lbs.; of dry wood, about 4½ lbs.

In a petition to the Reichstag concerning the use of alleged poisonous colours, the Association for Promoting the Interests of German Chemical Industry point out that certain dyes, such as red corallin and aurantia, denounced as poisons, have subsequently been proved perfectly innocent. They pray that no colour may be prohibited till it has been formally pronounced dangerous to public health by a commission of specially qualified chemists and physiologists after a due scientific examination.

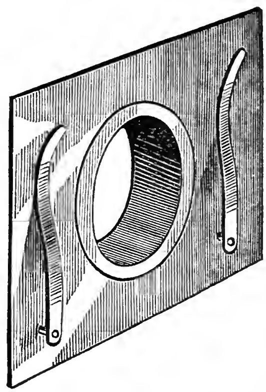
While engaged in preparing a Supplement to his "Handbook of Chemical Manipulation, Mr. Greville Williams, F.R.S., made inquiries with the view of ascertaining what improvements had recently been made in the volumetrical determination of acids and alkalies. Amongst the persons consulted was Dr. Otto N. Witt, who pointed out that the "orange 3" of Porrier formed an excellent substitute for litmus. This new coal-tar derivative was introduced into commerce about two years ago: its manufacture has, however, been almost entirely, if not quite, abandoned in favour of "orange 4" or tropæolin OO. This "orange 3" had, before its introduction into commerce, been studied independently by Dr. Witt and M. Griess; and M. Lunge, in the "Berichte der Deutschen Chemischen Gesellschaft," expressly mentions it as an indicator. Its chemical name is dimethyl-amido-azo-benzol-sulphonate of ammonium. Different specimens of litmus somewhat vary in their sensitiveness to acids and alkalies; that with which Mr. Williams experimented in comparing "orange 3" and litmus ceased to indicate with sulphuric acid when more dilute than 1 to 50,000. The new indicator, on the other hand, remained sensitive to 1 part of sulphuric acid in 100,000 of water. An immense advantage which this indicator possesses over litmus is that it is entirely unaffected by carbonic acid; the solution need not therefore be boiled, and the operation consequently takes much less time; in fact, four operations can be made in the same time as one with litmus. Mr. Williams also finds that the new indicator can be employed in the determination of ammonia instead of litmus, and has many advantages over it.

M. G. Luvini has preserved the eggs of the silkworm in hydrogen, carbonic acid, oxygen, and nitrogen for about three months. Those which had been kept in carbonic acid and in nitrogen after removal hatched well and gave an almost complete yield.

Physics.

A galvanometer, which measures the strength of the electric current directly, instead of indirectly, as in the instruments commonly employed, has, we learn from the "Engineering and Mining Journal," been constructed by Prof. Dolbear, of Tufts College. He utilises the force exerted by a common helix to draw the core within itself when a current is passed through it, which is directly proportional to the strength of the current. A simple mechanism, consisting of a spiral spring, a graduated scale, and a pointer, suffices to measure the strength of the current.

A simple contrivance for holding the object beneath the stage of the microscope, when extreme obliquity of illumination is required, has been devised by Mr. John Phin, of New York, and has the advantage of being easily adapted to any microscope.



The little sub-stage, with clips attached, is slid into the aperture in the stage. The mode of use will be obvious. Mr. Phin also states that the plan of holding the object beneath the stage is not new, having been invented by Mr. C. A. Spencer about twenty years ago.

Mr. E. J. Newton, of the Quekett Microscopical Club, has constructed an enlarged model of the brain of a cockroach (*Blatta orientalis*). The brain, properly hardened, was cut into sections of 1-1000th of an inch in thickness, and after the sections were mounted a careful outline of each was made by the camera lucida, on a piece of soft pine-wood of suitable thickness. Each slice was carefully cut out with a fine saw, and the whole piled together to form the magnified model. The surface of each section was carefully coloured, after the original preparations, to represent the appearance presented under the microscope.

The "Journal of the Royal Microscopical Society" has completed its first annual volume. The Fellows may be congratulated upon the improved form their official publication has taken. The volume contains 402 pages, comprising the whole of the papers read before the Society, seventeen plates besides woodcuts in the text, full reports of the meetings, translations of

microscopical and biological papers from foreign sources, and a list of similar articles in several British and foreign publications. Mr. Frank Crisp, LL.B., B.A., F.L.S., one of the Secretaries of the Society, has undertaken the honorary editorship.

The Council of the Royal Microscopical Society have devised a plan by which other societies founded for kindred objects should be brought into association with them. The presidents for the time being of such societies, at home or abroad, as the Council may from time to time recommend, shall be *ex officio* Fellows, and shall receive the publications of the Royal Microscopical on behalf of their respective societies, and exercise all other privileges of Fellows, voting excepted. At a recent meeting the Quekett Microscopical Club, the South London Microscopical Club, the Croydon Microscopical Club, and a large number of provincial and foreign societies, were, through their respective presidents, admitted to *ex officio* fellowship.

The microscopic phenomena of muscular contraction has recently been studied by Th. W. Engelmann : the results are given in "Archives Neerlandaises." During the contraction of the transversely striated muscular fibres there are produced, parallel with the change of form of the muscular elements, changes in the optical properties of the isotropic and anisotropic layers. These changes are of an opposite nature in the two layers, the isotropic stratum in its totality becoming more refrangent and the anisotropic less refrangent. In consequence, at a certain degree of shortening, the fibre seen by common light may appear homogeneous without appreciable transverse striæ—the stage of homogeneity or of transition. If the shortening is carried further the transverse striæ corresponding to the isotropic disks are seen to reappear. At any given phase of the process of contraction, consequently even in the transition stage, the isotropic and anisotropic substances may be recognised by means of the polarising microscope as well-defined and regularly alternating strata. They do not, at the time of contraction, exchange their respective places in the muscular compartment. The thickness of both strata decreases during contraction, that of the isotropic layer much more rapidly than that of the anisotropic. The total volume of each compartment does not vary sensibly during contraction. The anisotropic strata increase in volume at the expense of the isotropic, as during contraction liquid passes from the latter to the former.

Some interesting researches on electric fishes have been communicated to the French Academy of Sciences by E. J. Marey. Physiologists have been struck with the analogies presented by a muscle and the apparatus of electric fishes. These two kinds of organs, both subject to the will and provided with nerves for centrifugal action, have further a very analogous chemical composition, and even some points of structural resemblance. These

views, put forward before physicists had developed the theory of the correlation of forces, were necessarily very vague. It might even be conceived that in the living organism, as well as in our physical instruments, analogous conditions might produce either mechanical work or electricity. Having found that muscular acts are complex, *i.e.*, that a muscle in tetanus or in contraction executes a series of minute successive movements, which M. Marey calls *shocks*, which accumulate to produce muscular attraction, he has examined the discharge of the torpedo, and found there a similar complexity. Having passed this discharge through an electro-magnetic tracing apparatus, he found that it was made up of minute shocks, which recur at the rate of 150 in a second. Cold reduces the rate alike of the muscular and electric shocks, whilst heat acts inversely. Hence M. Marey concludes that these two functions are really homologues. The gymnotus gave similar results to those of the torpedo. The latter fish, when connected with a telephone and slightly excited, produced a very short croaking. If a prolonged discharge is occasioned by pricking the electric lobe of the brain, the sound produced lasts three or four seconds, and in tonality borders upon *mi* (165 vibrations).

On examining with the spectroscope, both by absorption and by means of the electric spark, the products of his operations on the mixture of earths from samarskite, M. Lecoq de Boisbaudran has observed rays or bands not to be referred to any element formerly known, and not corresponding to the descriptions of the spectra of the earths recently announced by MM. Delafontaine, L. Smith, Soret, and De Marignac. These new rays of absorption and emission seem to belong to one and the same body. The emission spectrum is composed of four bands shaded towards the left and formed of narrow rays, the strongest of which is the most refrangible and forms the right margin of the band. The absorption spectrum comprises two strong bands in the blue, and several rays of less importance in the green. The metal which yields these new spectra is precipitated as a double potassic sulphate along with didymium; its simple sulphate is rather less soluble than that of didymium; its oxalate is precipitated along with didymium, but ammonia separates the oxide of the new metal before that of didymium.

An improvement in Bunsen's battery is described in "Les Mondes" by M. Lefebure. The oxidation of the exterior surface of the zincs contributes nothing to the liberation of electricity, so it may be safely covered with varnish, thus reducing the consumption of zinc by one-half.

THE MONTHLY

JOURNAL OF SCIENCE.

JUNE, 1879.

I. COLOUR AND ITS RECOGNITION.*

IT is not without some reluctance that we venture to reopen a question which has been abundantly handled both by workers and dreamers, and which, though by no means decided or exhausted, must await further discoveries for its ultimate solution. We wish, however, to lay before our readers certain speculations calculated to prove valuable and suggestive not merely to professed men of science, but also to the cultivated lay-public. Although disclaiming all right to rank as a biological specialist, Mr. Grant Allen, the author of the work before us, has, in our opinion, thrown a welcome and seasonable light upon some of the most obscure features of the animal and vegetable world. Even though certain of the conclusions to which he has led may not be accepted as final, they must surely give a new stimulus and a fresh direction to research.

Mr. Allen's immediate object is to prove that the colour-sense—so far from being, as Dr. Magnus and Mr. Gladstone have on mere philological grounds sought to demonstrate, a recent development of the human mind—must have existed in pre-historical ages, and that it may be traced far down the animal series. In opposition, again, to a far more formidable authority, Mr. A. R. Wallace, he defends the theory of Sexual Selection. In the very outset of his work he suggests the law that "only those animals display beautiful colours, due to Sexual Selection, in whom a taste for colour has already been aroused by the influence of flowers, fruits, or brilliant insects, their habitual food,"—a principle which we shall have to examine in some detail. He shows

* The Colour-Sense, its Origin and Development: an Essay in Comparative Psychology. By GRANT ALLEN, B.A. London: Trübner and Co.

that, practically speaking, our idea of colour corresponds with that of the lower animals. He traces a connection between sight and the power of locomotion, contending that this sense is most developed among winged creatures.

We find it, then, asserted, that a perception and taste for colour is first aroused in animals by the influence of the objects on which they feed. On the other hand, the disciples of the new school of Natural History—and no one more emphatically than our author—contend that it is to the colour-sense, especially of insects and birds, that we owe all the varied colouration of blossoms and fruits as distinct from the monotonous green of earlier geological epochs. We can admit that, in virtue of the principle of Natural Selection, the smallest step on the part of a plant to produce a gay efflorescence, or the smallest step on the part of an insect towards the appreciation of a source of edible pollen or saccharine matter, would, *pro tanto*, give the advantage in the struggle for existence to the species in which it might occur. But where and how was the first step taken? "Did flowers show an original tendency to the production of coloured adjuncts prior to the selective action of insects? Did insects possess any tendency vaguely to discriminate colours apart from the reactive influence of entomophilous flowers?" Mr. Allen, in addressing himself to these questions, argues that the normal green colour of plants is connected with de-oxidation, and the consequent storing up of energy. But where oxidation and the expenditure of energy are known to be in progress,—as in decaying leaves, in buds, young shoots, and sprouts, especially those put forth in the dark, where the reductive action of the solar rays is of course wanting,—we find a rich and varied display of colour. The author gives as instances the sprouts of peonies, which are of a full dark red: the rosy stems and yellow early leaves of rhubarb, &c. Now all these colours are indeed merely adventitious. But in flowers, where the colouration is a "purposive adaptation," we find, even in anemophilous species, a rise of temperature, from which we know that oxidation is in progress. Hence we may naturally expect that all floral organs, whether anemophilous or entomophilous, would have a tendency to the production of bright colours. This is accordingly seen even in the mosses, lycopods, and ferns. We have thus a groundwork of differentiated colouring upon which Natural Selection or any other agency may operate. The author accordingly decides that "the bright pigments of entomophilous plants are due

originally to natural oxidation aided by the selective action of insects." To the second question, whether insects had any tendency to discriminate colours apart from the reaction of entomophilous flowers, he replies that colours differ not merely qualitatively, but quantitatively, according as they absorb a greater or less proportion of the luminous rays. Thus "white might be distinguished by the primitive eye from green, brown, or black." If a beginning is thus given the rest will easily follow.

In a chapter devoted to the colour-sense of insects, the reality of which he proves by an appeal to numerous and fully-established facts, the author reverts to the views of Mr. Gladstone, Dr. Geiger, and Dr. Magnus, and declares that they have been "partially adopted" even by Mr. Wallace in his "Tropical Nature." This we can scarcely admit, since in that very work he points out the cardinal error in Mr. Gladstone's reasoning, viz., that the absence of a precise nomenclature for colours is no proof of the lack of colour-perception. In disposing of the old theory of the colouration of flowers, our author happily remarks;—"Not even the watch-maker deity of Paley himself, one may suppose, would have invented flowers in the Secondary Age for the sole gratification of man in the Post-tertiary. To put it briefly, if insects have not a colour-sense, then the whole universe must be nothing more than a singularly happy concourse of fortuitous atoms."

Just as the varied hues of flowers are due to the selective action of insects, so the colouration of fruits must be sought in the reaction of birds and mammals. These, attracted by the colour no less than the odour of fruits, swallow the pulp, and void the hard, smooth, indigestible seeds with their excreta, placing them thus under highly favourable circumstances for germination. Hence the plant whose fruits assumed the most brilliant and striking colouration would gain a decided advantage in the struggle for existence.

On the fact that some poisonous, indigestible, or otherwise non-eatable fruits display a brilliant colouring much might be said did space permit. The author hazards the suggestion that some plants, such as the common arum and the manchineel, may derive a benefit from the poisonous property of their seeds, which if eaten by birds or small mammals might prove fatal, and "would thus have an opportunity of germinating in the midst of a rich manure heap formed of its decomposing body." We must remember, however, that poisonous seeds are avoided by most species and prove harmless to others.

Among the Mammalia, the author declares that the evidences of a colour-sense are mainly wanting. "The antipathy of male ruminants for scarlet," which he mentions,* seems to us to point to a tolerably well-developed recognition of colour. The bull, *e.g.*, must be able to discriminate between scarlet or blood-red and that reddish brown so common in his own species. If, however, the author's theory be correct, an absence of the colour-sense, save in monkeys, and perhaps squirrels, need not surprise us. No other mammals feed upon fruits or are very specially connected with flowers. This same deficiency, Mr. Allen considers, is the cause why mammals display none of the brilliant colouration so common in other departments of the animal kingdom. We cannot, however, refrain from pointing out that the golden mole, the only mammal endowed with fur of a metallic lustre, is certainly no fruit-eater.

Among flower- and fruit-feeding species in the most different groups of the animal kingdom the author considers that there prevails a community of tastes, in strongly-marked contrast to those of the Carnivora and the carrion-feeders. With this view, however, we cannot entirely agree. Many of the perfumes which are most agreeable to us are equally attractive to cats. Nothing seems to delight a leopard more than a saucer of lavender-water, and the common cat rolls in delight upon a number of plants which secrete essential oils, and which certainly have no connection with her ordinary diet.† On the other hand, many butterflies, though feeding ordinarily upon the nectar of flowers, may be attracted by odours of a most antagonistic character. Perhaps the easiest way to capture high-flying species, such as *Apatura Iris*, and the splended *Papilios* and *Ornithopteras* of sunnier climates, is to bait for them with excrement or carrion, such as a dead rat or a weasel. Many of our common native butterflies may be seen haunting manure-heaps and sipping the fœtid moisture.

To a certain passage in Mr. Wallace's "Tropical Nature,"‡ in which this illustrious biologist considers that an insect's capacity to distinguish colours may probably be quite unaccompanied by a sense of radical distinctness,

* Is this antipathy confined to males? Mr. Allen elsewhere remarks that dogs are attracted by bright colours.

† "Senses of the Lower Animals." (Quarterly Journal of Science, vol. viii., p. 297.)

‡ Page 238.

Mr. Allen takes exception in a passage which closely coincides with our remarks on the same subject.*

In a chapter on the "Direct Reaction of the Colour-Sense upon the Animal Integuments" the author proceeds to the demonstration of what we may call his initial law. He seeks to show, by a multitude of instances, that the most beautiful insects and birds are such as haunt flowers and fruits, either feeding directly upon their pollen, nectar, sweet juices, &c., or preying upon minute insects attracted there for the same purpose. He cites the observation repeatedly made by travelling naturalists, that where the flora is most beautiful the fauna is correspondingly splendid, whilst if the one is dingy and insignificant the same characters apply to the other. Exceptions will doubtless occur to the mind of the naturalist, especially if he be a student of insect-life. Thus though the author remarks, with much general truth, that "first in order of ugliness must be placed the carrion-feeders who live upon decaying bodies or animal excrements," yet if we look over a tolerably complete assortment of beetles of the great neo-tropical genus *Phaneus*—all, be it remembered, dung- and carrion-devourers—we find almost every variety of rich iridescent and metallic hues, emerald-greens, golden and ruddy bronzes, gorgeous blues, and purples. Great splendour may also be found among the *Coprides*, another group of filth-devourers; and even our common English dor-beetles show on their under surfaces pure and beautiful metallic hues. Sometimes, again, one and the same pattern or arrangement of colour may be traced in a number of species belonging to distinct groups, and feeding on substances of a totally different nature. For instances of this kind the reader is referred to the "Monthly Journal of Science" for February (p. 196). To the cases there mentioned may be added the death's-head moth, the convolvulus-hawk moth, &c. Mr. Allen himself refers to the tiger-beetles as exquisitely beautiful and highly carnivorous; and many other rapacious beetles, of the genera *Carabus*, *Calosoma*, *Pœcilus*, &c., are remarkable for the loveliness of their tints. Still we do not feel warranted in concluding that these apparent exceptions, and others which might be adduced, justify the rejection of Mr. Allen's law. Our knowledge of the diet and habits of insects is still very imperfect. Perhaps, too, the cases we have mentioned are "residual phenomena," which on further study will point the way to some further generalisation.

* Quarterly Journal of Science, vol. viii., p. 459.

In speaking of the generation of pigments Mr. Allen quotes with approval the declaration of Mr. Lowne,* that these substances are apparently due to the waste-products of other organs, and do not take away anything from the effective energies of the system. It might, perhaps, be asked how this view agrees with the one put forward in an earlier part of his work, where the production of colour is connected with the expenditure of energy? We fully admit the very high proportion of nitrogen present in feathers, and we recognise the fact that those animals—insects, birds, and reptiles—which are most richly coloured excrete uric acid to a notable extent. At the same time it must be admitted that the most beautiful colouring of such animals is due not to any pigment, but to the interference of light arising from the micro-mechanical texture of the feathers, wing-scales, elytra, &c.

Concerning "prohibitive" or "warning" colour, the author declares himself somewhat sceptical. As regards the eggs of birds he suggests that "the colouration may act as a supplementary allurement to the instinct of incubation." But how if the nest, as occurs in not a few species, such as the kingfisher, is absolutely dark within?

The concluding chapters of the work treat of the colour-sense in man, of the esthetic value of colour, and of the growth of the colour-vocabulary. By testimony collected through the aid of consuls, missionaries, and others, from semi-civilised and savage tribes, it is shown that the colour-sense is universal. By the evidence of gems and glass and stone beads, obtained from the lake-dwellings of Switzerland from barrows of the Stone Age, and of pottery from Dr. Schliemann's excavations at Mycæna, a cumulative proof is furnished that this same sense is of no recent growth.

* Philosophy of Education, p. 75.

II. EARLY TRACES OF MAN.*

By G. De Mortillet.

QUATERNARY MAN.—The man of geological time—fossil man—is now a fact so clearly demonstrated that it is no longer called in question. The recent exposition of anthropological sciences showed us his works plentifully scattered throughout France, England, Spain, and Italy.

But, though the existence of quaternary man in the south-west of Europe is no longer denied, there is a school which, walking with fear and hesitation in the path of progress, has its mind made up to contest his existence in the Orient. What the leaders of this school maintain is this:—In the East, say they, civilisation, and consequently historic records, date back to a very remote time. Is it not, then, possible that geological time still persisted in Europe, and especially in Western Europe, while in Egypt the historic dynasties were being founded?

To put forth such a proposition as this, one must be ignorant of the data of geology. The remarkable collections exhibited at the Anthropological Exposition have shown that man was contemporary not only with the reindeer, the saiga, the chamois, and the marmot on our plains; not only with the mammoth and the *Rhinoceros tichorhinus*,—that is, with the fauna of the Glacial period, but also with the great hippopotamus, the *Elephas primigenius*, and the rhinoceros of Merk. All geologists are agreed that the duration of the period in which we live is as nothing compared with that of the Quaternary period. It is as a day compared to ages, as a drop of water in a stream. All palæontologists understand what a length of time is requisite for the rise and decline of animal species—species which, while they have been upon the earth, have been lavishly distributed over an enormous area.

But we have no need of the general data of geology and palæontology in order to meet the objection. The Exposition of the Anthropological Sciences furnished materials which reduce it to a nullity. There were exhibited perfectly characterised quaternary instruments of silex from the East

* **Translated by J. Fitzgerald, A.M., from the Revue d'Anthropologie.**

—from the most ancient seats of civilisation, Egypt and Syria. In those countries, then, no less than in France and England, quaternary man preceded all the historic civilisations.

The earliest Quaternary epoch, the preglacial, is characterised, so far as man's works are concerned, by a stone implement of peculiar form. It is dressed on its two sides, usually rather roughly chipped; it is rounded at the base, pointed at the top, and its edges are pretty sharp. In general form it is more or less almond-shaped. This implement, in past times called by workmen in quarries "langue de chat" (cat's tongue), is now called "hache de St. Acheul," or "hache acheulienne" (hatchet of St. Acheul), terms derived from the locality in which it has been oftenest found. They have been found in abundance in the quaternary alluviums of France, England, and Spain. Nay, within a few years they have been found in the valley of the Delaware near Trenton, New Jersey, by Dr. Charles C. Abbott. The figures which he has published, and his descriptions, tally exactly with the St. Acheul hatchets of France and England.

Nor is it in the New World only that the existence of man in the earliest portion of the Quaternary period has been proved; the same thing is true of the Old World. M. Place, the explorer of Assyria, has brought to light a St. Acheul hatchet of silex which he found under the ruins of the palace of Khorsabad. At the Exposition the Abbé Richard showed a St. Acheul hatchet, also of silex, from the Lake of Tiberias.

A still more conclusive proof is furnished by Prof. Henry W. Haynes, of Boston, who reports a number of wrought flints from Egypt, among them several clearly characterised St. Acheul hatchets.

In the February number (1869) of the "Matériaux pour l'Histoire de l'Homme," M. Adrien Arcelin first made the announcement that the grand Egyptian civilisation, like all other civilisations, was preceded by an age of stone. He had just collected in Upper Egypt several clipped flints. Toward the close of the same year this discovery was confirmed by Messrs. Lenormant and Hamy. All the specimens brought home by these earliest explorers might be regarded as belonging to the Robenhausen epoch, or age of polished stone; only one specimen, presented to the museum of St. Germain, came anywhere near the St. Acheul type.

After Arcelin's discovery, collections of dressed flints were multiplied in Egypt, though without throwing much light upon the question. But Sir John Lubbock, in an essay

illustrated with fine plates, gave figures of three flint implements found at Luxor and at Abydos, which are undoubtedly St. Acheul hatchets.

Among the wrought flints brought from Egypt and exhibited by Mr. Haynes are several which incontestably are of the quaternary type. Among them we see scrapers and arrow-heads, the latter belonging to a type which in France occurs only in glacial formations. The collection also embraces more ancient forms, preglacial forms, referable to the early portion of the Quaternary period, viz., St. Acheul hatchets of flint.

These St. Acheul hatchets come from two very distinct localities: one lot is from the neighbourhood of Luxor, in Upper Egypt; the other from the environs of Cairo, in Lower Egypt. The flint used, as is clearly proved by Delanoue, comes from the nummulitic formations. These formations are found *in situ* in Upper Egypt; and the St. Acheul hatchets of that region are as a rule heavier and better wrought,—above all, more completely wrought. In the environs of Cairo there are no rocks *in situ;* and as for flint, only rounded nodules are found. These nodules have been wrought into the forms of implements. This is easily seen, for all the St. Acheul hatchets of that locality still bear at their base traces of the original rounded surface of the nodules.

From these archæological data—*i.e.*, from the nature and form of the objects—we may conclude that the man of the earliest Quaternary times lived in Egypt simultaneously with his existence in Europe, and that in both of these regions his industrial development was about the same, extremely primitive. And geological observation confirms these deductions. It was not on the surface of plateaus that Mr. Haynes found these St. Acheul implements. On the contrary, most of them, at least from the neighbourhood of Luxor (forming the greater number), were found in the bottom of the ravines of Bab-el-Moluk. These ravines are cut deep into the quaternary deposits by the torrents which, in seasons of heavy rainfall, carry to the Nile the waters from the mountains of Libya.

Thus, then, thanks to the Exposition of the Anthropological Sciences, we are in a position to show that the oldest Egyptian civilisation,—that of the earliest dynasties,—which dates back 4000 years before our era, was preceded by an age of polished stone, and that before that period Egypt, like all the rest of the world, was occupied by quaternary man.

Tertiary Man.—Important as are the results of the Anthropological Exposition from the point of view of quaternary man, they are still more so from the point of view of tertiary man.

But first let us understand what is meant by the terms quaternary and tertiary man.

The fauna of the mammals serves clearly to determine the limits of these later geological periods.

The Tertiary is characterised by terrestrial mammals entirely different from extant species; the Quaternary by the mingling of extant with extinct species; the present period by the extant fauna.

The man of the early Quaternary, he who made the St. Acheul hatchets and used them, is the man of Neanderthal, of Canstatt, of Enggisheim, of La Naulette, of Denise. He is indubitably a man, but differing more widely from the Australian and the Hottentot than the Australian and Hottentot differ from the European. Hence unquestionably he formed another human species, the word species being taken in the sense given to it by naturalists who do not accept the transformation doctrine.

Tertiary man, therefore, must have been still more distinct—of a species still less like the present human species; indeed, so different as to entitle it to be regarded as of distinct genus. For this reason I have given to this being the name of man's precursor. Or he might be called anthropopithecus—the man-monkey.

The question of Tertiary man should therefore be expressed thus:—Did there exist in the Tertiary age beings sufficiently intelligent to perform a part of the acts which are characteristic of man?

So stated, the question is settled most completely by the various series of objects sent to the Anthropological Exposition.

The first and oldest of these collections was that made by the late Abbé Bourgeois, at Thenay (Loir-et-Cher). At the International Congress of Prehistoric Archæology and Anthropology, held in Paris in 1867, the Abbé Bourgeois exhibited tertiary flints which, he claimed, had been chipped intentionally. These early specimens were not very conclusive, lost as they were amid a multitude of other specimens which certainly had not been fashioned intentionally, unless one can suppose that they had been intentionally split by the action of fire. The result was that the Abbé's communication won to his side but few adherents. But, profoundly convinced of the reality of his discovery, the Abbé Bourgeois

did not lose heart on suffering this partial repulse. He continued his researches with vigour, and again, in 1872, provided now with better specimens, he raised the question at the Brussels Congress. There he made some headway among the best experts. But on the commission which was specially appointed to examine the flints were several members who knew but very little directly about the manner of working on flint, and they either hesitated or passed an adverse judgment. Hence the question was not definitively settled. This result—half success, half failure—stimulated the ardour of the accomplished naturalist; he continued his investigations, and so succeeded in collecting for the Anthropological Exposition a remarkable series of flint implements which dispels all doubt.

This collection was made up of flints which beyond a doubt had undergone the action of fire. They are full of cracks, and even quite discoloured. With these are other flints, far more numerous, which have simply been split by fire. Among them are some which unquestionably have been neatly and regularly retouched on one or both of their margins. Every one who has carefully and impartially examined them has admitted that the second dressing (*les retailles*) was certainly intentional, and consequently that it was the work of an intelligent creature.

It remains to determine the age to which these flints belong. They were collected at Thenay, in formations clearly *in situ* and intact, and belonging to the formation known among geologists as "calcaires de Beauce;" but now these calcaires de Beauce constitute the lower strata of the Middle Tertiary. This is shown by the fauna which the Abbé Bourgeois exhibited in connection with the flints. This fauna, which comes from the sands of the Orléanais, which directly overlie the calcaires de Beauce, comprises great mastodons and dinotheriums belonging to the Lower Miocene. Then there is the acerotherium, a genus akin to the rhinoceros, and which was found in the very same stratum as the fire-split and re-dressed flints.

It results, therefore, from the Abbé Bourgeois's researches, that during the Middle Tertiary there existed a creature, precursor of man, an anthropopithecus, which was acquainted with fire and could make use of it for splitting flints. It also knew how to trim the flint-flakes thus produced and to convert them into tools.

This curious and interesting discovery for a long time stood alone, and arguments were even drawn from this

isolatedness to favour its rejection. Fortunately another French observer, M. J. B. Rames, has found in the vicinity of Aurillac (Cantal), in the strata of the upper part of the Middle Tertiary,—here, too, in company with mastodons and dinotheriums, though of more recent species than those of Thenay,—flints which also have been re-dressed intentionally. Here, however, the flints are no longer split by fire, but by tapping. It is something more than a continuation, it is a development. Among the few specimens exhibited by M. Rames, whose discoveries are quite recent, is one which, had it been found on the surface of the ground, would never have been called in question.

The weighty facts developed by French investigators received striking confirmation in the Portuguese department of the Exposition. A distinguished *savant* of Lisbon, Senhor Ribeiro, director of the Geological Bureau of Portugal, sent a collection of flints and quartzites found in the strata of the Middle Tertiary or Miocene, and in the Upper Tertiary or Pliocene of the valley of the Tagus. Among these specimens—ninety-five in number—are twenty-two which bear unquestionable traces of intentional chipping. Nine specimens, all of flint, are described as coming from the Miocene. Of the others, purporting to be Pliocene, seven are of flint and six of quartzite. All these specimens are roughly chipped, and nearly all are triangular in form, and not re-dressed, whether the material be flint or quartzite.

Thus, then, the Anthropological Exposition, important though it was from the point of view of Quaternary man, is still more important from the point of view of Tertiary man —man's precursor. His existence can no more be denied.—*Popular Science Monthly.*

III. LEAVES AND THEIR FUNCTIONS.

By Rev. L. J. Templin, Hutchinson, Kas.

A LEAF, whatever may be its configuration or colour, is always an object of interest. But how few people, when they see a leaf as it waves and flutters in the breeze, really know what they are looking at. Leaves appear in an endless variety of forms, sizes, and colours. They are often so transformed that it is more by the place they occupy

than by their forms that we know they are leaves. Underground stems or rhizomas have them at each point or node as little thin scales. Buds are enveloped in peculiar cerements, which generally fall away soon after the ordinary leaves have begun to expand; those enveloping scales are only leaves in a modified form. They are quite prominent in the hickory and horse-chestnut. The scales of bulbs, as of the lily, are simply modified leaves. Flowers are only aggregations of metamorphosed leaves. But it is with leaves as foliage that we are more immediately concerned at present. A complete leaf consists of three parts—the stalk or stem (petiole) on which it rises, the expanded blade or lamina, and two small leaf-like appendages at the base of the leaf-stalk called stipules. The only essential part is the blade, as this may be sessile on the stem without petiole or stipules. The blade of a leaf consists of three portions:—the woody framework, ribs, or veins; the green cellular portion, pulp; and the outside covering, or epidermis. The epidermis, which is really an extension of the outer bark of the stem, is composed of closely united, transparent cells, with frequent openings through it called stomata or breathing-pores. These vary in number from 800 to 170,000 to the square inch of surface. It is through these that water is exhaled from the plant. They are more numerous in the leaves of plants growing in moist situations and surrounded by a damp atmosphere. The pores dilate with the increase of humidity and contract with the increase of aridity. Plants growing in arid climates have but few stomata, and these are very small. While the most of foliage appears to be made on the principle of exposing the greatest possible surface to the air, some forms of vegetation seem to be constructed for the accomplishment of the very reverse of this. Thus the various species of *Cactus*, whose native habitat is the hot, arid plains of the South-west, are constructed on the principle of presenting the least extent of surface to the air, and this surface is covered with an epidermis that is almost impervious to water. This is necessary to prevent excessive transpiration in that very dry climate. The pulp or parenchyma of the leaf is made up of several layers of cells. These cells are small globular sacs, varying from 1-1200th to 1-250th of an inch in diameter. A layer of these, of a rather elongated form, is arranged immediately beneath the epidermis of the upper side of the leaf with the ends to the surface. These are crowded quite closely together. Another layer, not quite so much elongated and less compactly arranged, is found on

the under side of the leaf. Between these two layers are numerous globular cells that seem thrown together without any great regularity or order. Among these are numerous irregular passages, intercellular spaces, through which water and air circulate. These reach the surface through the stomata of the epidermis. It is worthy of notice that by far the larger part of these breathing pores are on the under surface, and this surface always seems to avoid direct sunshine. If a leaf is inverted, turning the bottom side upward, it will, if possible, return to its natural position, and if prevented from so doing it will soon die. A few leaves have been known to grow in a vertical instead of a horizontal position. The framework of leaves consists of wood, and is intended to give firmness and support to the leaf. It is divided into numerous veins or nerves that ramify every part of the green parenchyma. There are two distinct systems of venation of leaves—the parallel-veined and the net-veined. In the former the fibres run nearly parallel from one extremity of the leaf to the other; such leaves are usually long and narrow, linear, as in the grasses, corn, &c. In the other the veins are netted, ramifying the leaf in all directions, and dividing the parenchyma into numerous small squares and diamonds. This style of venation exists under two forms: in one a principal vein, midrib, extends from the base to the apex of the leaf, and from this numerous smaller veins branch off and run to the margin; in the other there are three or five nearly equal ribs running the length of the leaf. The first is feather-veined, from its resemblance to a feather; and the other is palmately veined, the main ribs branching out like the fingers of a hand. The shape of a leaf is generally determined by the manner of its venation. The two principal styles of venation belong to and denote two different classes of plants, the parallel-veined belonging to the monocotyledonous, and the net-veined to the dicotyledonous divisions of the vegetable kingdom. Thus the veining of a small portion of a leaf will indicate to which of these classes the plant upon which it grew belonged.

The green colour of leaves comes from a granular substance, chlorophyll, found in the cells of the parenchyma. In its absence no true vegetable structure can be built up from the original elements, and it can operate only in the presence of sunlight. Low cryptogamic plants will grow in the dark, but they contain no proper chlorophyll. Chlorophyll has been found to be composed of two different substances,—xanthophyll, a yellow substance, and cyanophyll,

a blue material: their union forms chlorophyll, or leaf-green. It is thought that the yellow colour of leaves at maturity is caused by the predominance of xanthophyll at that time. Besides chlorophyll the leaf-cells contain the proximate principles of the plant; and here the real work of building plant structure is performed. But this brings us to the consideration of the second part of our subject, viz., the functions of leaves.

In treating this branch of the subject it will be necessary to consider the leaf under several different characters. Leaves should be considered as real living beings, capable of performing vital functions—as workers performing a large amount of important work. We may first consider *the leaf as a pump*. One of its most important offices is to pump up water from the soil through the roots and stems of plants. This it exhales through its stomata in the form of invisible vapour. By this means a large quantity of water is carried up from the soil to the atmosphere. Thus a large portion of water that would quickly settle down through the deeper soil, and find its way into underground passages, is carried up and given off to the atmosphere, where it is condensed into clouds and descends in rain, thus watering and making fruitful the earth. Without this work many parts of the earth that now blossom as the rose would become arid wastes. The amount of moisture thus carried up and exhaled by the foliage of trees and plants is immense. A sunflower, with a leaf surface of 39 square feet, exhaled 3 lbs. of water in twenty-four hours. A corn plant, in about three and a half months, gave off in vapour thirty-six times its own weight of water. A medium-sized forest tree will pump up and exhale about 5 barrels of water in twenty-four hours: this will give about 800 barrels to the acre. An acre of grain or grass will do about the same. From this it may be seen why forests exert such a powerful influence on the rainfall of a country.

Again, we may consider *the leaf as a lightning conductor*. It is one of the most efficient conductors of electricity ever made. Most leaves have notched edges; each of these "points" is powerful to attract the electric fluid from the air, and through the stem convey it silently to the ground. A single blade of grass is said to be three times as powerful to attract electricity as a fine cambric needle, and a twig covered with leaves is more efficient than the best constructed "patent point." A tree covered with leaves is the most efficient safeguard from lightning that can be found.

A green tree is constantly conveying electricity from the

earth to the air and from the air to the earth. True, it sometimes tries to carry too large a load in response to the efficient collecting power of the leaves. They gather it in faster than the trunk can carry it away, and it is burst. We say the tree is struck with lightning; but it has often been struck before, but this time it was overloaded and crushed. Trees are natural lightning-rods, more efficient than all the artificial ones that have ever been invented.

In the next place we may contemplate *the leaf as an organiser* of organic matter. It is here that it has performed its most efficient and important service for man. Through its agency every particle of both vegetable and animal organism has been either directly or indirectly built up. Every plant, tree, and shrub has been directly built up through the labour of the leaf. And even long before the present order of things existed the leaf was at work: through its labours vast beds of vegetable matter were laid away far back in the carboniferous ages, which by heat and pressure have become coal, forming vast storehouses of excellent fuel. And still farther back, in times when Silurian seas washed the shores of limited bodies of land, the leaf was at earnest, ceaseless toil. Thus we owe to the leaf not only what makes life pleasant, but our food and raiment and fuel, without which life would be impossible. Without the leaf as an organiser the earth would sink back into a lifeless, pulseless waste.

Lastly, we may consider *the leaf as a chemical agent*, withdrawing and consolidating various poisonous gases, which if left in the air would render it unfit to sustain life, and thus convert the earth into one vast charnel-house of the dead. The air contains 1-2500th of its own bulk of carbonic acid, consisting of two equivalents of oxygen and one of carbon. This gas is a deadly foe to animal life, and if permitted to accumulate in the air would soon render it unfit to sustain life. And yet there are certain processes constantly going on that tend to augment the proportion of this gas in the atmosphere. Every breath of every human being and every living animal, and every bit of fuel that is consumed, and every particle of vegetable matter that decays, and every volcano that sends forth its deadly fumes, are adding to the quantity of this gas in the atmosphere. By what agency, then, is the equilibrium maintained? It is through the agency of our little friend the leaf that the work so essential to life and health is performed. It is constantly employed as an analytic chemist imbibing this poisonous gas and analysing it, using the carbon to build up the organic sub-

stance of its own structure, and giving up the healthful, life-giving oxygen to the atmosphere again. This process is so regulated as exactly to keep pace with the liberation of carbonic acid through the agencies mentioned above. Other deleterious gases are thus taken in and rendered innocuous. The blue gum (*Eucalyptus globulus*), of Australia, has become famous for absorbing the deadly gases in miasmatic districts, and thus rendering them healthy. Thus the leaf labours preparing food for all living animals, and raiment and fuel for the lords of creation, as well as all wood and bone and ivory used in the arts. It also purifies the air, making animal life possible, and clothing the earth with beauty that the life thus preserved may be replete with the highest enjoyment.—*Kansas City Review of Science and Industry.*

IV. MOLECULAR PHYSICS IN HIGH VACUA.*

By WILLIAM CROOKES, F.R.S.

WHEN I was asked, a month or two ago, to illustrate in this theatre some of my recent researches on Molecular Physics in High Vacua, I exclaimed "How is it possible to bring such a subject worthily before a Royal Institution audience when none of the experiments can be seen more than three feet off?" If to-night I am fortunate enough to show all the experiments to those who are not far distant, and if I succeed in making most of them visible at the far end of the theatre, such a success will be entirely due to the great kindness of your late Secretary, Mr. Spottiswoode, who has placed at my disposal his magnificent induction-coil,—not only for this lecture, but for some weeks past in my own Laboratory,—thus enabling me to prepare apparatus and vacuum tubes on a scale so large as to relieve me of all anxiety so far as the experimental illustrations are concerned.

Before describing the special researches in molecular physics which I propose to illustrate this evening, it is necessary to give a brief outline of one small department of the modern theory of the constitution of gases.

* A short-hand report of a Lecture delivered at the Royal Institution, on Friday, April 4, 1879.

It is not easy to make clear the kinetic theory, but I will try to simplify it in this way:—Imagine that I have in a large box a swarm of bees, each bee independent of its fellow, flying about in all manner of directions and with very different velocities. The bees are so crowded that they can only fly a very short distance without coming into contact with one another or with the sides of the box. As they are constantly in collision, so they rebound from each other with altered velocities and in different directions, and when these collisions take place against the sides of the box pressure is produced. If I take some of the bees out of the box, the distance which each individual bee will be able to fly before it comes into contact with its neighbour will be greater than when the box was full of bees, and if I remove a great many of the bees I increase to a considerable extent the average distance that each can fly without a collision. This distance I will call the bee's *mean free path*. When the bees are numerous the mean free path is very short; when the bees are few the mean free path will be longer, the length being inversely proportional to the number of bees present. Let us now imagine a loose diaphragm to be introduced in the centre of the box, so as to divide the number of bees equally. The same number of bees being on each side, the impacts on the diaphragm will be equal; and the mean speed of the bees being the same, the pressure will be identical on each side of the diaphragm, and it will not move.

Let me now warm one side of this division so as to let it communicate extra energy to a bee when it touches it. As before, a bee will strike the diaphragm with its normal mean velocity, but will be driven back with extra velocity, the reaction producing an increase of pressure on the diaphragm. It will be found, however, that although the diaphragm is free to move, the extra strength of the recoil on the warm side does not produce any motion. This at first sight seems contrary to the law of action and reaction being equal. The explanation is not difficult to understand. The bees which fly away from the diaphragm have drawn energy from it, and therefore move quicker than those which are coming towards it; they beat back the crowd to a greater distance, and keep a greater number from striking the diaphragm. Near to the heated side of the diaphragm the density is less than the average, while beyond the free path the density is above the average, and this greater crowding extends to all other parts of the box.

Thus it happens that the extra energy of the impacts against the warm side of the diaphragm is exactly compensated by the increased number of impacts on the cool side. In spite therefore of the increased activity communicated to a portion of the bees, the pressure on the two sides of the diaphragm will remain the same. This represents what occurs when the extent of the box containing the bees is so great, compared with the mean free path, that the abrupt change in the velocities of those bees which rebound from the walls of the box produces only an insensible influence on the motions of bees at so great a distance as the diaphragm.

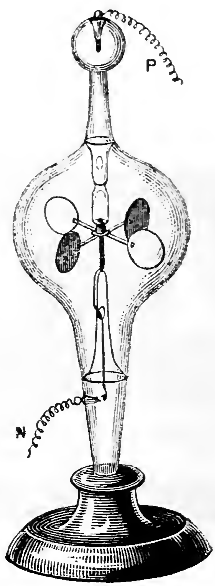
I will next ask you to imagine that I am gradually removing bees from our box, still keeping the diaphragm warm on one side. The bees getting fewer the collisions will become less frequent, and the distance each bee can fly before striking its neighbour will get longer and longer, and the crowding in front of them will grow less and less. The compensation will also diminish, and the warmed side of the diaphragm will have a tendency to be beaten back. A point will at last be reached on the warm side, when the mean free path of the bees will be long enough to admit of their dashing right across from the diaphragm to the side of the box, without meeting more than a certain number of in-coming bees in their flight. In this case the bees will no longer fly quite in the same direction as before. They will now fly less sideways, and more forwards and backwards between the heated face of the diaphragm and the opposed wall of the box. Because of this preponderating motion, and also because they will thereby less effectually keep back bees crowding in from the sides, there will now be a greater proportionate pressure both on the hot face of the diaphragm and on that part of the box which is in front of it. Hence the pressure on the hot side will now exceed that on the cool side of the diaphragm, which will consequently have a backward movement communicated to it.

I may diminish the size of the bees as much as I like, and by correspondingly increasing their number the mean free path will remain the same. Instead of bees let me call them molecules, and instead of having a few hundreds or thousands in the box let me have millions and billions and trillions; and if we also diminish the mean free path to a considerable extent, we get a rough outline of the kinetic theory of gases. (I may just mention that the mean free path of the molecules in air, at the ordinary pressure, is the ten-thousandth of a millimetre.)

Three years ago I had the honour of bringing before you the results of some researches on the Radiometer. Let me now take up the subject where I then left off. I have here two radiometers which have been rotating before you under the influence of a strong light shining upon them.

The explanation of the movement of the radiometer is this,—the light, or the total bundle of rays included in the term "light," falling upon the blackened side of the vanes, becomes absorbed, and thereby raises the temperature of the black side: this causes extra excitement of the air molecules which come in contact with it, and pressure is produced, causing the fly of the radiometer to turn round.

Fig. 1.



I have long believed that a well-known appearance observed in vacuum tubes is closely related to the phenomena of the mean free path of the molecules. When the negative pole is examined while the discharge from an induction-coil is passing through an exhausted tube, a dark space is seen to surround it. This dark space is found to increase and diminish as the vacuum is varied, in the same way that the ideal layer of molecular pressure in the radiometer increases and diminishes. As the one is perceived by the mind's eye to get greater, so the other is seen by the bodily eye to increase in size. If the vacuum is insufficient to permit the

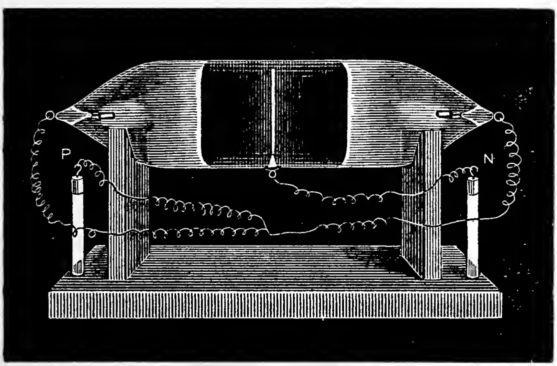
radiometer to turn, the passage of electricity shows that the "dark space" has shrunk to small dimensions. It is a natural inference that the dark space is the mean free path of the molecules of the residual gas.

The radiometer which has just been turning under the influence of the lime-light is not of the ordinary kind. Fig. 1 will explain its construction.

It is similar to an ordinary radiometer with aluminium disks for vanes, each disk coated on one side with a film of mica. The fly is supported by a hard steel instead of glass cup, and the needle point on which it works is connected by means of a wire with a platinum terminal sealed into the glass. At the top of the radiometer bulb a second terminal is sealed in. The radiometer can therefore be connected with an induction-coil, the movable fly being made the negative pole.

As soon as the pressure is reduced to a few millims. of mercury, a halo of velvety violet light forms on the

Fig. 2.



metallic side of the vanes, the mica side remaining dark. As the pressure diminishes, a dark space is seen to separate the violet halo from the metal. At a pressure of half a millim. this dark space extends to the glass, and positive rotation commences. On continuing the exhaustion the dark space further widens out and appears to flatten itself against the glass, when the rotation becomes very rapid.

You perceive a dark space behind each vane and moving round with it. In the first experiment, radiation from the lime-light falling on the metallic sides of the vanes, produced a layer of molecular pressure which drove the fly

round; so here the induction-current has produced molecular excitement at the surface of the vanes forming the negative pole, extending up to the side of the glass.

When the negative pole is in rapid rotation it is not easy to see this dark space, so I have arranged a tube in which the dark space will be visible to all present. The tube, as you will see by the diagram (Fig. 2), has a pole in the centre in the form of a metal disk, and other poles at each end. The centre pole is made negative, and the two end poles connected together are made the positive terminal. The dark space will be in the centre. When the exhaustion is not very great the dark space extends only a little distance on each side of the negative pole in the centre. When the exhaustion is very good, as it is in the tube before you, and I turn on the coil, the dark space is seen to extend for about 2 inches on each side of the pole.

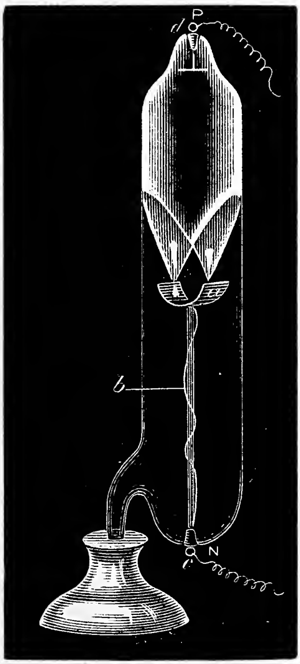
Here, then, we see the induction spark actually illuminating the lines of molecular pressure caused by the excitement of the negative pole. The thickness of this dark space—nearly 2 inches—is the measure of the mean free path between successive collisions of the molecules of the residual gas. The extra velocity with which the negatively electrified molecules rebound from the excited pole keeps back the more slowly moving molecules which are advancing towards that pole. The conflict occurs at the boundary of the dark space, where the luminous margin bears witness to the energy of the discharge.

I will endeavour to throw on the screen an illustration of this dark space. A stream of water falls from a small jet on to a horizontal plate of glass. The water spreads over the plate and forms a thin film. The jet of water in the centre, from the velocity of its fall, drives the film of water before it on all sides, raising it into a ring-shaped heap. As I diminish the force of the jet the ring contracts: this is equivalent to the exhaustion getting less. When I increase the force of water the ring expands in size, the effect being analogous to an increase of exhaustion in my tubes. The extra velocity of the falling particles of water drive the in-coming particles of water before them, and raises a ridge round the side which exactly represents the luminous halo to the dark space to be seen in this tube.

If, instead of a flat disk, a metal cup is used for the negative pole, the successive appearances on exhausting the tube are somewhat different. The velvety violet halo forms over each side of the cup. On increasing the exhaustion the dark space widens out, retaining almost exactly

the shape of the cup. The bright margin of the dark space becomes concentrated at the concave side of the cup to a luminous focus, and widens out at the convex side. When the dark space is very much larger than the cup, its outline forms an irregular ellipsoid drawn in towards the focal point. Inside the luminous boundary a dark violet light can be seen converging to a focus, and, as the rays diverge on the other side of the focus, spreading beyond the margin of the dark space; the whole appearance being strikingly similar

Fig. 3.



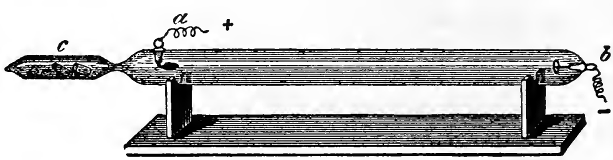
to the rays of the sun reflected from a concave mirror through a foggy atmosphere. This proves a somewhat important point; it shows that the molecules thrown off the excited negative pole leave it in a direction almost normal to the surface.

I can illustrate this property of the molecular rays by an experiment. This diagram (Fig. 3) is a representation of the tube which is before you. It contains, as a negative pole, a hemi-cylinder (*a*) of polished aluminium. This is connected with a fine copper wire, *b*, ending at the platinum terminal, *c*. At the upper

end of the tube is another terminal, *d*. The induction-coil is connected so that the hemi-cylinder is negative and the upper pole positive, and when exhausted to a sufficient extent, as is the case with this tube, the projection of the molecular rays to a focus is very beautifully shown. The rays are driven from the hemi-cylinder in a direction normal to its surface; they come to a focus and then diverge, tracing their path in brilliant green phosphorescence on the surface of the glass.

You will notice that the rays which project from the negative pole and cross in the centre have a bright green appearance; that colour is entirely due to the phosphorescence of the glass. At a very high exhaustion the phenomena noticed in ordinary vacuum tubes when the induction spark passes through them—an appearance of cloudy luminosity and of stratifications—disappears entirely. No cloud or fog whatever is seen in the body of the tube, and with such a vacuum as I am working with in these experiments—about a

Fig. 4.



millionth part of an atmosphere—the inner surface of the glass glows with a rich green phosphorescence, the intensity of colour varying with the perfection of the vacuum. It scarcely begins to show much before the 800,000th of an atmosphere. At about a millionth of an atmosphere the phosphorescence is very strong, and after that it begins to diminish until there are not enough molecules left to allow the spark to pass.*

I have here a tube which will serve to illustrate the dependence of the green phosphorescence of the glass on the degree of perfection of the vacuum (Fig. 4). The two poles are at *a* and *b*, and at the end (*c*) is a small supplementary tube connected with the other by a narrow aperture, and containing solid caustic potash. The tube has been exhausted to a very high point, and the potash heated so as to drive off moisture and deteriorate the vacuum.

*

1·0	millionth of an atmosphere			=	0·00076 millim.
1315·789	millionths of an atmosphere			=	1·0 millim.
1,000,000	"	"	"	=	760·0 millims.
"	"	"	"	=	1 atmosphere.

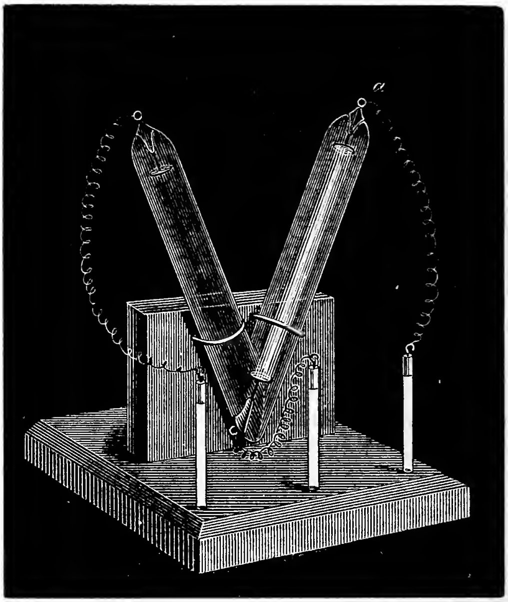
Exhaustion has then been re-commenced, and the alternate heating and exhaustion have been repeated until the tube has been brought to the state in which it now appears before you. When the induction spark is first turned on nothing is visible—the vacuum is so high that the tube is non-conducting. I now warm the potash slightly, and liberate a trace of aqueous vapour. Instantly conduction commences, and the green phosphorescence flashes out along the length of the tube. I continue the heat, so as to drive off more gas from the potash. The green gets fainter, and now a wave of cloudy luminosity sweeps over the tube, and stratifications appear. These rapidly get narrower, until the spark passes along the tube in the form of a narrow purple line. I take the lamp away, and allow the potash to cool; as it cools, the aqueous vapour, which the heat had driven off, is re-absorbed. The purple line broadens out, and breaks up into fine stratifications; these get wider, and travel towards the potash tube. Now a wave of green light appears on the glass at the other end, sweeping on and driving the last pale stratification into the potash; and now the tube glows over its whole length with the green phosphorescence. Would time allow I might keep it before you, and show the green growing fainter and the vacuum becoming non-conducting; but time is required for the absorption of the last traces of vapour by the potash, and I must pass on to the next subject.

This green phosphorescence is a subject that has much occupied my thoughts, and I have striven to ascertain some of the laws governing its occurrence. I soon perceived that the phosphorescence was not in the body of the tube itself, but was entirely on the surface of the glass. Another peculiarity of the rays producing this green phosphorescence is that they will not turn a corner in the slightest degree. Here is a V-shaped tube (Fig. 5), a pole being at each extremity. The pole at the right side (*a*) being negative, you see that the whole of the right arm is flooded with green light, but at the bottom it stops sharply, and will not turn the corner to get into the left side. When I reverse the current, and make the left pole negative, the green changes to the left side, always following the negative pole, leaving the positive side with scarcely any luminosity.

In the ordinary phenomena exhibited by vacuum tubes—phenomena with which we are all familiar—it is customary, for the more striking illustration of their contrasts of colour, to have the tubes bent into very elaborate designs. The positive luminosity caused by the phosphorescence of the

residual gas follows all the convolutions and designs into which skilful glass-blowers can manage to twist the glass. The negative pole being at one end and the positive pole at the other, the luminous phenomena seem to depend more on the positive than on the negative at an ordinary exhaustion such as has hitherto been used to get

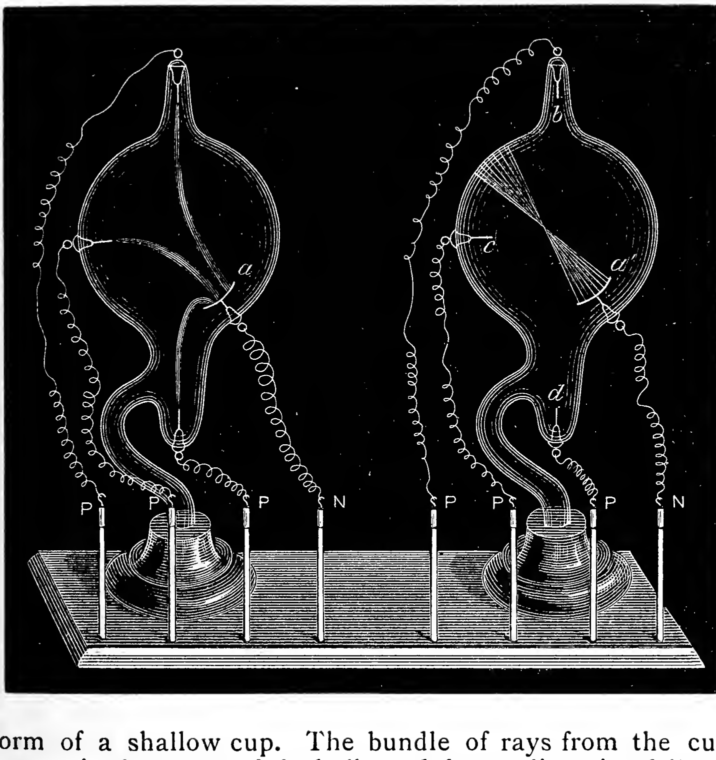
Fig. 5.



the best phenomena of vacuum tubes. I have here two bulbs (Fig. 6), alike in shape and position of poles, the only difference being that one is at an exhaustion equal to a few millimetres of mercury,—such a moderate exhaustion as will give stratifications or the ordinary luminous phenomena,—whilst the other is exhausted to about the millionth of an atmosphere. I will first connect the moderately exhausted bulb with the induction-coil, and, retaining the pole at one side (*a*) always negative, I will put the positive wire successively to the other three poles with which the bulb is furnished. You will see that as I change the position of the positive pole, the line of violet light joining the two poles changes. In this moderately exhausted bulb, therefore, the electric current always chooses the shortest path between the two poles, and moves about the bulb as I alter the position of the wires.

This, then, is the kind of phenomenon we get in ordinary exhaustions. I will now try the same experiment with a tube that is highly exhausted, and, as before, will make the side pole (*a'*) the negative, the top pole (*b*) being positive. Notice how widely different is the appearance from that shown by the last bulb. The negative pole is in the

FIG. 6.



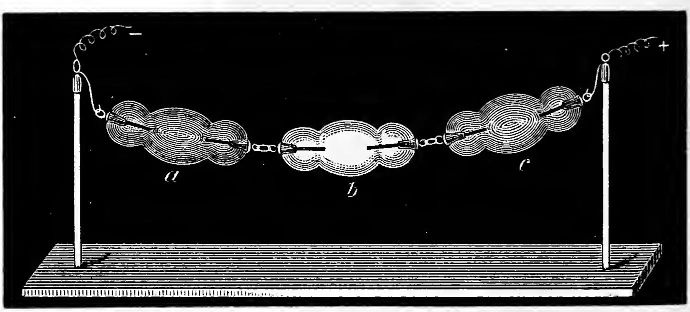
form of a shallow cup. The bundle of rays from the cup crosses in the centre of the bulb, and thence diverging falls on the opposite side as a circular patch of green light. As I turn the bulb round you will all be able to see the faint blue focus and the green patch on the glass. Now observe, I remove the positive wire from the top, and connect it with the side pole (*c*). The green patch from the divergent negative focus is still there. I now make the lowest pole (*d*) positive, and the green patch still remains where it was at first, unchanged in position or intensity.

This, then, gives us another fact which brings us a little nearer to the cause of this green phosphorescence. It is this—

that in the low vacuum the position of the positive pole is of every importance, whilst in a high vacuum it scarcely matters at all where the positive pole is; the phenomena seem to depend entirely on the negative pole. In very high vacua, such as we have been using, the phenomena follow altogether the negative pole. If the negative pole points in the direction of the positive, all very well, but if the negative pole is entirely in the opposite direction it does not matter: the line of rays is projected all the same in a straight line from the negative.

I have hitherto spoken of and illustrated these phenomena in connection with *green* phosphorescence. It does not follow, however, that the phosphorescence is always of that colour. This colouration is a property of the particular kind of glass in use in my laboratory. I have here (Fig. 7) three bulbs composed of different glass: one is uranium glass (*a*), which phosphoresces of a dark green colour; another is English glass (*b*), which phosphoresces of a blue colour; and the third (*c*) is soft German glass,—of which most of the apparatus before you is made,—which phosphoresces of a bright apple-green colour. It is therefore plain that this particular green phosphorescence is solely due to the glass which I am using. Were I to use English glass I should have to speak of blue phosphorescence, but I know of no glass which is equal to the German in brilliancy.

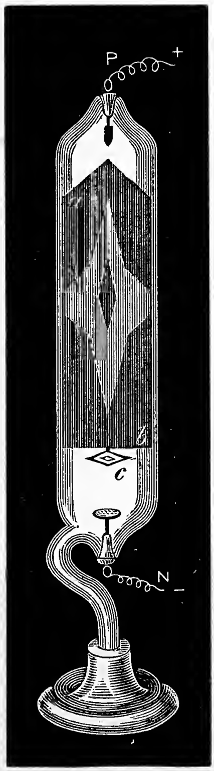
FIG. 7.



My earlier experiments were almost entirely carried on by the aid of the phosphorescence which glass takes up when it is under the influence of the electric discharge *in vacuo;* but many other substances possess this phosphorescent power, and some have it in a much higher degree than glass. For instance, here is some of the luminous sulphide of calcium prepared according to M. Ed. Becquerel's description. When it is exposed to light—even candlelight—it

phosphoresces for hours with a rich blue colour. I have prepared a diagram with large letters written in this luminous sulphide; before it is exposed to the light the letters are invisible, but Mr. Gimingham has just exposed it in another room to burning magnesium, and now it is

Fig. 8.

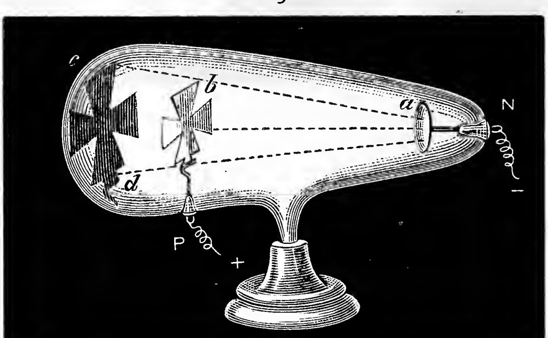


brought into the darkened theatre you will see the word "φως,"—*light*, a very suitable word for so beautiful a phosphorescence—shining brightly in luminous characters. The first letter, φ, shines with an orange light; it is a sulphide of calcium prepared from oyster-shells. The other letters, shining with a blue light, are sulphide of calcium prepared from precipitated carbonate of lime. Once the phosphorescence is excited the letters shine for several hours. I will put the diagram at the back, and we shall see how it lasts during the remainder of the lecture. This substance, then, is phosphorescent to light, but

it is also much more strongly phosphorescent to the molecular discharge in a good vacuum, as you will see when I pass the discharge through this tube (Fig. 8). The white plate (*a*, *b*) in the centre of the tube is a sheet of mica painted over with the luminous sulphide of which the letter ϕ was composed in the diagram you have just seen. On connecting the poles with the coil, the mica screen glows with a strong yellowish green light, bright enough to illuminate all the apparatus near it. But there is another phenomenon to which I now desire to draw attention: on the luminous screen is a kind of distorted star-shaped figure. A little in front of the negative pole I have fixed a star (*c*) cut out in aluminium, and it is the image of this star which you see on the screen. It is evident that the rays coming from the negative pole project an image of anything that happens to be in front of it. The discharge, therefore, must come from the pole in straight lines, and does not merely permeate all parts of the tube and fill it with light as it would were the exhaustion less good. Where there is nothing in the way the rays strike the screen and produce phosphorescence, and where there is an obstacle they are obstructed by it, and a shadow is thrown on the screen. I shall have more to say about this shadow presently; I merely now wish to establish the fact that these rays driven from the negative pole produce a shadow.

I must draw your attention to an important experiment connected with these molecular rays, but unfortunately it is a very delicate one, and very difficult to show to many at once; but I hope, if you know beforehand what

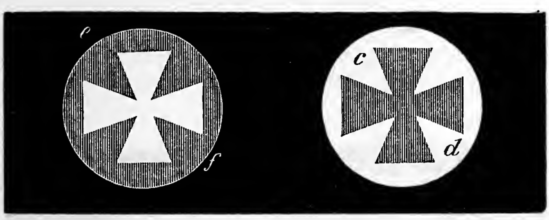
FIG. 9 A.



to look for, you will all be able to see what I wish to show. In this pear-shaped bulb (Fig. 9 A) the negative pole (*a*) is at the pointed end. In the middle is a cross (*b*)

cut out of sheet aluminium, so that the rays from the negative pole projected along the tube will be partly intercepted by the aluminium cross, and will project an image of it on the hemispherical end of the tube which is phosphorescent. I think you will all now see the shadow of the cross on the end of the bulb (*c*, *d*), and notice that the cross is black on a luminous ground. Now, the rays from the negative pole have been passing by the side of the aluminium cross to produce the shadow; they have been hammering and bombarding the glass till it is appreciably warm, and at the same time they have been producing another effect on that glass—they have deadened its sensibility. The glass has got tired, if I may use the expression, by the enforced phosphorescence. Some change has been produced by this bombardment which will prevent the glass from responding easily to additional excitement; but the part that the shadow has fallen on is not tired—it has not been phosphorescing at all and is perfectly fresh; therefore if I throw this star down,—I can easily do so by giving the apparatus a slight jerk, for it has been most ingeniously constructed with a hinge by Mr. Gimingham,—and so allow the rays from the negative pole to fall uninterruptedly on to the end of the bulb, you will suddenly see the black cross (*c*, *d*, Fig. 9 B) change to a luminous one (*e*, *f*), because the back-

Fig. 9 B.



ground is only faintly phosphorescing, whilst the part which had the black shadow on it retains its full phosphorescent power. The luminous cross is now dying out. This is a most delicate and venturous experiment, and I am fortunate in having succeeded so well, for it is one that cannot be rehearsed. After resting for a time the glass seems to partly recover its power of phosphorescing, but it is never so good as it was at first.

We have, therefore, found an important fact connected with this phosphorescence. Something is projected from the negative pole which has the power of hammering away at the glass in front of it, in such a way as to cause it not

only to vibrate and become temporarily luminous while the discharge is going on, but to produce an impression upon the glass which is permanent. The explanation which has gradually evolved itself from this series of experiments is this :—The exhaustion in these tubes is so high that the dark space, as I showed you at the commencement of this Lecture, that extended around the negative pole, has widened out till it entirely fills the tube. By great rarefaction the mean free path has become so long that the hits in a given time may be disregarded in comparison to the misses, and the average molecule is now allowed to obey its own motions or laws without interference. The mean free path is in fact comparable to the dimensions of the vessel, and we have no longer to deal with a *continuous* portion of matter, as we should were the tubes less highly exhausted, but we must here contemplate the molecules *individually*. At first this was only a convenient working hypothesis. Long-continued experiment then raised this provisional hypothesis almost to the dignity of a theory, and now the general opinion is that this theory gives a fairly correct explanation of the facts. In these highly exhausted vessels the mean free path of the residual molecules of gas is so long that they are able to drive across from the pole to the other side of the tube with comparatively few collisions. The negatively electrified molecules of the gaseous residue in the tube therefore dash against anything that is in front, and cast shadows of obstacles just as if they were rays of light. Where they strike the glass they are stopped, and the production of light accompanies this sudden arrest of velocity.

Other substances besides English, German, and uranium glass, and Becquerel's luminous sulphides, are also phosphorescent. I think, without exception, the diamond is the most sensitive substance I have yet met for ready and brilliant phosphorescence. I have here a tube, similar to those already exhibited, containing a mica screen painted with powdered diamond, and when I turn on the coil, the brilliant blue phosphorescence of the diamond can be seen, quite overpowering the green phosphorescence of the glass. Here, again, is a very curious diamond, which I was fortunate enough to meet with a short time ago. By daylight it is green, produced, I fancy, by an internal fluorescence. The diamond is mounted in the centre of this exhausted bulb (Fig. 10), and the negative discharge will be directed on it from below upwards. On darkening the theatre you see the diamond shines with as much light as a candle, phosphorescing of a bright green.

In this other bulb is a remarkable collection of crystals of diamonds, which have been lent me by Professor Maskelyne. When I pass the discharge over them I am

FIG. 10.

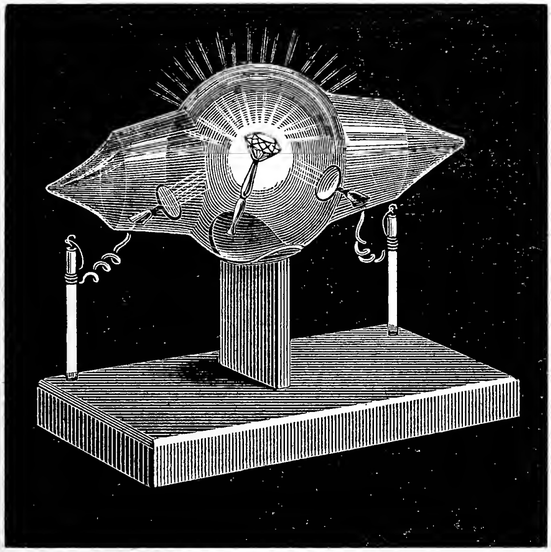
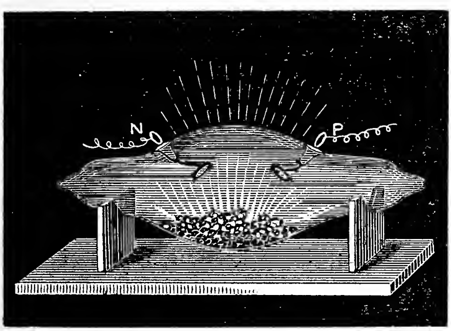


FIG. 11.



afraid you will only be able to see a few points of light, but if you will examine them after the Lecture, you will see them phosphoresce with a most brilliant series of colours—blue, apricot, red, yellowish green, orange, and pale green.

Next to the diamond the ruby is one of the most remarkable stones for phosphorescing. In this tube (Fig. 11) is a collection of ruby pebbles, for the loan of

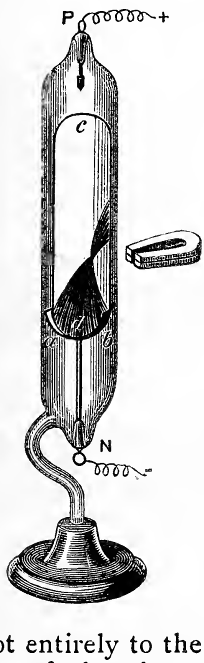
which I am indebted to my friend Mr. Blogg, of the firm of Blogg and Martin, who placed a small sackful at my disposal. As soon as I turn on the induction spark you will see these rubies shining with a brilliant rich red colour, as if they were glowing hot. Now the ruby is nothing but crystallised alumina with a little colouring-matter, and it became of great interest to ascertain whether the artificial ruby made by M. Feil, of Paris, would glow in the same manner. I had simply to make my wants known to M. Feil, and he immediately sent me a box containing artificial rubies and crystals of alumina of all sizes, and from those I have selected the mass in this tube which I now place under the discharge: they phosphoresce of the same rich red colour as the natural ruby. It scarcely matters what colour the ruby is, to begin with. In this tube of natural rubies there are stones of all colours—the deep red ruby and the pale pink ruby. There are some so pale as to be almost colourless, and some of the highly-prized tint of pigeon's blood; but in the vacuum under the negative discharge they all phosphoresce with about the same colour.

As I have just mentioned, the ruby is crystallised alumina. In a paper published twenty years ago by Ed. Becquerel* I find that he describes the appearance of alumina as glowing with a rich red colour in the phosphoroscope (an instrument by which the duration of phosphorescence in the sunlight can be examined). Here is some chemically pure precipitated alumina which I have prepared in the most careful manner. It has been heated to whiteness, and you see it glows with the rich red colour which is supposed to be characteristic of alumina. The mineral known as corundum is a colourless variety of crystallised alumina. Under the negative discharge in a vacuum, corundum phosphoresces of a rose-pink colour. There is another curious fact in which I think chemists will feel interested. The sapphire is also crystallised alumina, just the same as the ruby. The ruby has a little colouring-matter in it, giving it a red colour; the sapphire has a colouring-matter which gives it a blue colour, whilst corundum is white. I have here in a tube a very fine crystal of sapphire, and, when I pass the discharge over it, it gives alternate bands of red and green. The red we can easily identify with the glow of alumina; but what is the green? If alumina is precipitated and purified as carefully as in the case I have just mentioned, but in a somewhat

* Annales de Chimie et de Physique, 3rd series, vol. lvii., p. 50, 1859.

different manner, it is found to glow with a rich green colour. Here are the two specimens of alumina in tubes, side by side. Chemists would say that there was no difference between one and the other; but I connect them with the induction-coil, and you see that one glows with a bright green colour, whilst the other glows with a rich red colour. Here is a fine specimen of chemically pure alumina, lent me by Messrs. Hopkin and Williams; by ordinary light it is a perfectly white powder. It is just possible that the rich fire of the ruby, which has caused it to be so

FIG. 12.



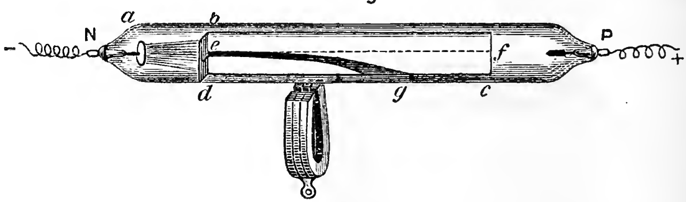
prized, may be due not entirely to the colouring-matter, but to its wonderful power of phosphorescing with a deep red colour, not only under the electric discharge in a vacuum, but whenever exposed to a strong light.

The spectrum of the red light emitted by all these varieties of alumina—the ruby, corundum, or artificially precipitated alumina—is the same as described by Becquerel twenty years ago. There is one intense red line, a little below the fixed line B in the spectrum, having a wavelength of about 6895. There is a continuous spectrum beginning at about B, and a few fainter lines beyond it, but they are so faint in comparison with this red line that they

may be neglected. This line may be called the characteristic line of alumina.

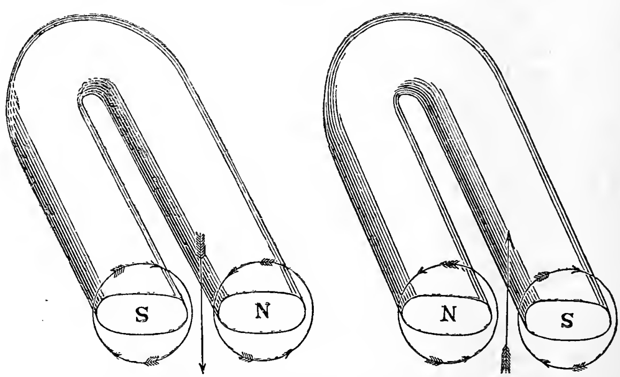
I now pass on to another fact connected with this negative discharge. Here is a tube (Fig. 12) with a negative pole (*a*, *b*) in the form of a hemi-cylinder, similar to the one you have already seen (Fig. 3), but in this case I receive the rays on a phosphorescent screen (*c*, *d*). See how brilliantly the lines of discharge shine out, and how intensely the focal point is illuminated; it lights the whole table. Now I bring a small magnet near, and move it to and fro; the rays obey the magnetic force, and the focus bends one way and the other as the magnet passes it. I can show this magnetic action a little more definitely. Here is a long glass tube (Fig. 13),

FIG. 13.



very highly exhausted, with a negative pole at one end (*a*) and a long phosphorescent screen (*b*, *c*) down the centre of the tube. In front of the negative pole is a plate of mica (*b*, *d*) with a hole (*e*) in it, and the result is that when I turn on the current, a line of phosphorescent light (*e*, *f*) is projected along the

FIG. 14.

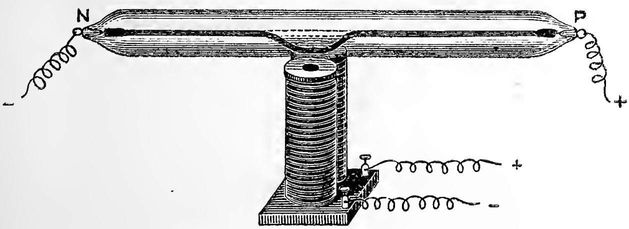


whole length of the tube. I now place beneath the tube a powerful horseshoe magnet: see how the line of light becomes curved under the magnetic influence (*e*, *g*), waving

about like a flexible wand as I move the magnet up and down. The action of the magnet can be understood by reference to this diagram (Fig. 14). The north pole gives the ray of molecules a spiral twist one way, and the south pole twists it the other way; the two poles side by side compel the ray to move in a straight line up or down, along a plane at right angles to the plane of the magnet and a line joining its poles.

Now it is of great interest to ascertain whether the law governing the magnetic deflection of the trajectory of the molecules is the same as has been found to hold good at a lower vacuum. The former experiment was with a very high vacuum. This is a tube with a low vacuum (Fig. 15).

FIG. 15.

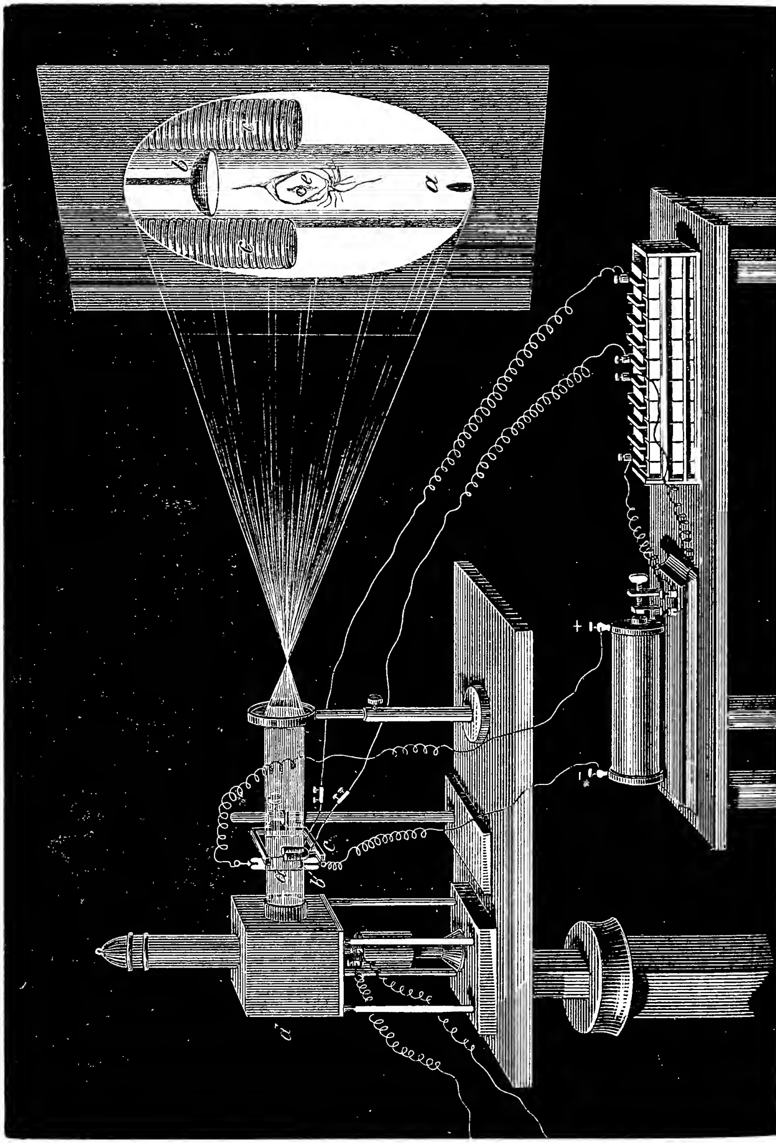


On passing the induction spark it passes as a narrow line of violet light joining the two poles. Underneath I have a powerful electro-magnet. I make contact with the magnet, and the line of light dips in the centre towards the magnet. I reverse the poles, and the line is driven up to the top of the tube. Notice the difference between the two phenomena. Here the action is temporary. The dip takes place under the magnetic influence; the line of discharge then rises, and pursues its path to the positive pole. In the high exhaustion, however, after the ray of light had dipped to the magnet it did not recover itself, but continued its path in the altered direction.

During these experiments another property of this molecular discharge has made itself very evident, although I have not yet drawn attention to it The glass gets very warm where the green phosphorescence is strongest. The molecular focus on the tube, which we have just seen (Fig. 12) would be intensely hot, and I have prepared an apparatus by which this heat at the focus can be intensified and rendered visible to all present. This small tube (*a*) (Fig. 16) is furnished with a negative pole in the form of a cup (*b*). The rays will therefore be projected to a focus

in the middle of the tube (Fig. 17, *a*). At the side of the tube is a small electro-magnet, which I can set in

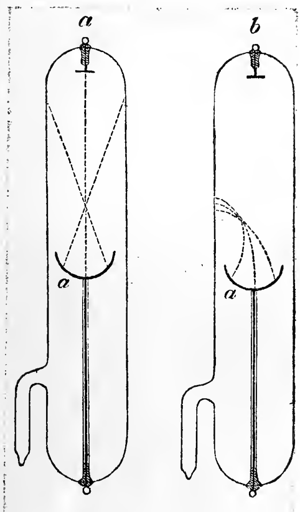
Fig. 16.



action by touching a key, and the focus is then drawn to the side of the glass tube (Fig. 17, *b*). To show the

first action of the heat I have coated the tube with wax. I will put the apparatus in front of the electric lantern (*d*), and throw a magnified image of the tube on the screen. The coil is now at work, and the focus of

FIG. 17.



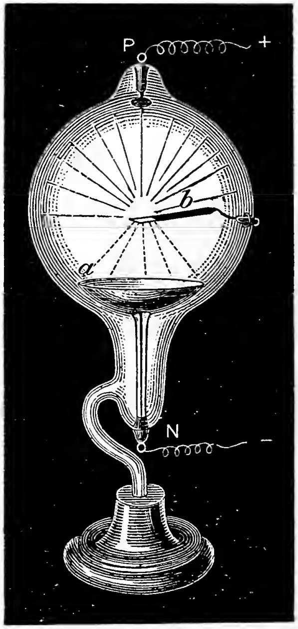
molecular rays is projected along the tube. I turn the magnetism on, and draw the focus on the side of the glass. The first thing you see is a small circular patch melted in the coating of wax. The glass soon begins to disintegrate, and cracks are shooting starwise from the centre of heat. The glass is softening. Now the atmospheric pressure forces it in, and now it melts. A hole (*e*) is perforated in the middle, the air rushes in, and the experiment is at an end.

Instead of drawing the focus to the side of the glass with a magnet, I will take another tube (Fig. 18), and allow the focus from the cup-shaped negative pole (*a*) to play on a piece of platinum wire (*b*) which is supported in the centre of the bulb. The platinum wire not only gets white-hot, but you can see sparks coming from it on all sides, showing that it is actually melting.

Here is another tube, but instead of platinum I have put in the focus that beautiful alloy of platinum and iridium which Mr. Matthey has brought to such perfection, and I think that I shall succeed in even melting that. I first turn on the induction-coil slightly, so as not to bring out its full power. The focus is now playing on the iridio-platinum, raising it to a white-heat. I bring a small magnet near,

and you see I can deflect the focus of heat just as I did the luminous focus in the other tube. By shifting the magnet I can drive the focus up and down, or draw it completely away from the metal, and render it non-luminous.

Fig. 18.

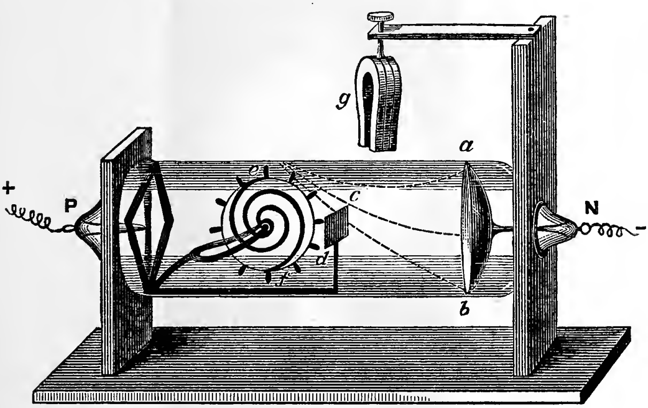


I withdraw the magnet, and let the molecules have full play again; the metal is now white-hot. I increase the intensity of the spark. The metal glows with almost insupportable brilliancy, and at last melts.

There is still another property of this molecular discharge, and it is this:—You have seen that the molecules are driven violently from the negative pole. If I place something in front of these molecules, they show the force of impact by the heat which is produced. Can I make this mechanical action evident in a more direct way? Nothing is simpler. I have only to put some easily moving object in the line of discharge in order to get a powerful mechanical action. Mr. Gimingham, with great skill, has constructed a piece of apparatus which I will presently put in the electric lantern, so that all will be able to see its action. But first I will explain the construction by means of this diagram (Fig. 19). The negative pole (a, b) is in the form of a

very shallow cup. In front of the cup is a mica screen (*c*, *d*), wide enough to intercept nearly all the molecular rays coming from the negative pole. Behind this screen is a mica wheel (*e*, *f*) with a series of vanes, making a sort of paddle-wheel of it. So arranged, the molecular stream from the pole *a b* will nearly all be cut off from the wheel,

Fig. 19.



and what escapes over and under the screen will hit the vanes equally, and will not produce any movement. I now put a magnet, *g*, over the tube, so as to deflect the stream over or under the obstacle *c d*, and the result will be rapid motion in one or the other direction, according to the way the magnet is turned. I now throw the image of the apparatus on the screen. The spiral lines painted on the wheel show which way it turns. I arrange the magnet to draw the molecular stream so as to beat against the upper vanes, and the wheel revolves rapidly, as if it were an over-shot water-wheel. I now turn the magnet so as to drive the molecular stream underneath; the wheel slackens speed, stops, and then begins to rotate the other way, as if it were an under-shot water-wheel. This can be repeated as often as I like to reverse the position of the magnet, the change of rotation of the wheel showing immediately the way the molecular stream is deflected.

This experiment illustrates the last of the phenomena which time allows me to bring before you, attending the passage of the induction spark through a highly exhausted atmosphere. It will now be naturally asked, What have we learned from the phenomena described and exhibited, and from the explanations that have been

proposed? We find in these phenomena confirmation of the modern views of matter and energy. The facts elicited are in harmony with the theory that matter is not continuous but composed of a prodigious number of minute particles, not in mutual contact. The facts also are in full accordance with the kinetic theory of gases—to which I have already referred—and with the conception of heat as a particular kind of energy, expressing itself as a rapid vibratory motion of the particles of matter. This alone would be a lesson of no small value. In Science, every law, every generalisation, however well established, must constantly be submitted to the ordeal of a comparison with newly-discovered phenomena; and a theory may be pronounced triumphant when it is found to harmonise with and to account for facts which when it was propounded were still unrecognised or unexplained.

But the experiments have shown us more than this: we have been enabled to contemplate matter in a condition hitherto unknown,—in a fourth state,—as far removed from that of gas as gas is from liquid, where the well-known properties of gases and elastic fluids almost disappear, whilst in their stead are revealed attributes previously masked and unsuspected. In this ultra-gaseous state of matter phenomena are perceived which in the mere gaseous condition are as impossible as in liquids or solids.

I admit that between the gaseous and the ultra-gaseous state there can be traced no sharp boundary; the one merges imperceptibly into the other. It is true also that we cannot see or handle matter in this novel phase. Nor can human or any other kind of organic life conceivable to us penetrate into regions where such ultra-gaseous matter may be supposed to exist. Nevertheless, we are able to observe it and experiment on it, legitimately arguing from the seen to the unseen.

Of the practical applications that may arise out of these researches, it would be now premature to speak. It is rarely given to the discoverer of new facts and new laws to witness their immediate utilisation. The ancients showed a perhaps unconscious sagacity when they selected the olive, one of the slowest growing trees, as the symbol of Minerva, the goddess of Arts and Industry. Nevertheless, I hold that all careful honest research will ultimately even though in an indirect manner draw after it, as Bacon said, "whole troops of practical applications."

NOTICES OF BOOKS.

Transactions and Proceedings of the Royal Society of Victoria. Vols. xiii. and xiv. Melbourne: Mason, Firth, and McCutcheon. London: Williams and Norgate.

We cannot congratulate the Royal Society of Victoria on the subjects of the papers printed in the thirteenth volume of their "Transactions." More than half the volume is taken up with a treatise on practical geodesy, and of the remaining memoirs there is not one which might not quite as easily have been written in London, Paris, or Berlin. What the learned world tacitly expects from such societies is that they shall prominently, and before all things, busy themselves with the rich crop of unrecorded facts and phenomena which surrounds them, waiting, so to speak, for recognition. The geology, the palæontology, botany, zoology, and ethnology of their own region constitute the sphere in which they can render the greatest services to Science and win for themselves the highest honour.

Vol. xiv. contains four noteworthy papers:—"Notes on the Coast-line of the Western District, and Proofs of the Uniform Condition of Meteorological Phenomena over Long Periods of Time," by Mr. T. E. Rawlinson; on some "New Marine Mollusca," by the Rev. J. E. Tennison-Woods—a most able and persevering scientific worker; "Extracts from a Diary in Japan," by F. C. Christy; and "History of Palæozoic Actinology in Australia," by R. Etheridge, jun. The last-mentioned memoir is a summary of what has been already discovered concerning the fossil corals of Australia. Mr. Christy's diary teems with interesting facts concerning the climate, the vegetation, agriculture, and horticulture, the architecture, religion, government, domestic economy, mining, and natural history of these wonderful islands. The author remarks that in Japan eight adults live from the produce of one acre, and keep it in good condition with their excreta, whilst in England the excreta from 800 to 1200 persons are used per acre without profitable result. The fauna of Japan agrees wonderfully closely with that of Europe. Mr. Christy gives a list—too long for insertion here—of butterflies, moths, and bees common to Japan and England, and remarks:—"In referring to the very many species identical with those of England it is remarkable, because Japan consists of a series of islands so very distant and isolated from England, and goes far to disprove Darwin's theory that the farther species are from species—that is, the more they are diffused by distance—the more they must differ, having to struggle for existence over

so great a space." It must, however, be remembered that the isolation between Britain and Japan is less in reality than in seeming. Both are merely outlying portions of one and the same zoological region with which they were at one time actually connected. Most of the species enumerated by Mr. Christy are traceable across the European and Asiatic continents.

Victorian Year-Book for 1877-8. By H. H. Hayter, Government Statist of Victoria. Melbourne: J. Ferries and G. Robertson. London: G. Robertson.

From among the valuable matter to be found in it we extract the following account of the appliances for scientific education existing in the colony of Victoria:—There are Schools of Mines both at Ballarat and Sandhurst. At the former there are classes for different branches of mathematics, for engineering, surveying both above and below ground, chemistry, metallurgy and assaying, telegraphy, and the French and German languages. The chemical laboratory contains twenty work-tables for students, and in the metallurgical laboratory there are twelve reducing furnaces. During 1877 the number of students in the January, April, July, and October terms respectively was 48, 58, 79, and 60. The Museum of the Institution contains 1594 mineralogical and geological specimens, besides models, but—like the library of three hundred bound books—is obviously still in its infancy. The buildings of the Melbourne Public Library have cost £111,604, and are still unfinished. The total number of volumes at the end of 1877 was 101,276.

The Industrial and Technological Museum contains 1100 publications, 23,725 specimens, and 112 drawings. Class lectures are given here on chemistry, mining, and telegraphy, the last-mentioned subject attracting as many pupils as the two former conjointly.

The so-called National Museum occupies a building on the grounds of Melbourne University. It comprises specimens of minerals, stuffed animals and birds, insects, "and other objects of curiosity,"—an expression which savours of the pre-scientific ages when biological collections were not understood to be means for study.

The account of Melbourne University throws no light on the all-important question whether it is preparing students for actual research or merely turning out examinees.

Journal of Proceedings of the Royal Society of New South Wales for 1877. Vol. xi. Sydney: Richards. London: Trübner and Co.

We may fairly congratulate the Royal Society of New South Wales both on the quantity and quality of the work it is doing. Of the nineteen papers herein contained all save three deal—and deal usefully—with local facts. We must more particularly notice an account of *Dromornis Australis*, a newly-discovered extinct gigantic bird of Australia. The species was founded on a femur discovered at Peak Downs, in Queensland, which the Rev. W. B. Clarke and M. G. Krefft at first referred to the genus *Dinornis*, and which consequently seemed a proof of a whilom land-connection between New Zealand and Australia. Prof. Owen, however, pointed out that this bone agrees in its essential characters more with the emu than the moa, and that it indicates the former existence of a bird nearly of the stature of the ostrich, but with relatively shorter and stronger hind limbs. Hence it appears that Australia, like all other tracts of land in the southern hemisphere, whether insular or continental, was at one time inhabited by gigantic "flightless" birds.

The Rev. W. B. Clarke communicates another paper on a new fossil extinct kangaroo (*Sthenurus minor*), part of the skull of which was found in the shaft of a gold-lead in Phillip County, New South Wales. He expresses a fear that in the indiscriminate slaughter of marsupials now in progress some species not yet recognised may perish unnoticed.

A paper on the "Forest Vegetation of Central and Northern New England" may at first sight be open to a misunderstanding. This name has, as if for the very purpose of creating confusion, been given to a district in Australia. The author, Mr. W. Christy, gives an account of a carnivorous plant *Drosera peltata*, —the "bottle-weed" of the squatters,—which is accused of causing disease in sheep, There is also a description of twenty-one species of *Eucalyptus* growing in this region.

Dr. W. J. Barkas communicates memoirs on the "Sphenoid, Cranial Bones, Operculum, and supposed Ear Bones of *Ctenodus*," and on a "Dental Peculiarity of the Lepidosteidæ."

Natural History of Victoria. Prodromus of the Zoology of Victoria, or Figures and Descriptions of the Living Species of all Classes of the Victorian Indigenous Animals. Decade I. By Frederick McCoy. Melbourne: J. Ferries and G. Robertson. London: Trübner and Co.

We have here the beginning of what promises to be a most valuable work, creditable alike to its author and the Victorian

government. The animal species are carefully figured in their natural colours, and the descriptions of their characteristics, general structure, habits, and locality are full and apparently accurate. The first three plates are devoted to three of the moft dangerous snakes of the province—the black snake (*Pseudechys porphyriacus*), the copper-head (*Hoplocephalus superbus*), and the tiger-snake (*H. curtus*). Concerning these species there prevails some confusion of nomenclature, which tends to render accounts of the effect of their bite and of the value of antidotes untrustworthy. Thus in Tasmania the tiger-snake is known as "carpet-snake," a name given on the mainland to an innocent species. In the same island the black snake has received the name of "diamond-snake," which properly belongs to a harmless species of New South Wales. As regards the bites of these Australian "death-snakes," ammonia taken internally or injected into a vein has in some cases at least proved successful, whilst for the bite of the Indian cobra it appears to be invariably useless. This important distinction suggests some questions weighty at once from a speculative and from a practical point of view. Do the poisons of different serpents differ merely in concentration, or are they chemically distinct, requiring consequently a distinct line of treatment? If so, do we find identity, or at least close relation, among the venoms derived from snakes of the same family, just as in the vegetable kingdom we find allied groups of alkaloids pervading certain families, as in the Cinchonaceæ, the Strychnaceæ, &c. The chemists, and no less the biologists, of Australia have here a most enviable field for research laid before them.

Plate VII. is devoted to *Megascolides Australis*, the giant earthworm, which, when fully extended, is about 6 feet long. The remaining three plates represent Lepidopterous insects. One of these, *Agarista glycine*, a day-flying moth of the curious family Alraniidæ, is remarkable for the change of habits it has undergone. Its original food was the common weed *Gnaphalium luteo-album*. But since the planting of vineyards it has completely abandoned its former food, and devours the leaves of the vine, occasioning enormous injury. How the female moth could learn that a foreign plant having no structural resemblance to the *Gnaphalium* would yet afford suitable nourishment to her future brood is one of the many "nuces zoologicæ" which our successors may some day crack. Certainly she could not in this case be guided by "hereditary habit." Fowls will not eat them, and the Indian minah—introduced into the colony as a vermin-killer—has developed a taste for grapes instead of devouring the larvæ. *Thyca Harpalyce* and *Aganippe* are butterflies resembling our "whites," but with rich red spots on the under side of the posterior wings. In the former, the angularity of the tip of the upper wing varies in different individuals of the same brood.

We trust that other of our colonial governments will follow the example set at Melbourne.

Tenth Annual Report of the United States Geological and Geographical Survey of the Territories. Embracing Colorado and parts of Adjacent Territories; being a Report of Progress of the Exploration for the Year 1876. By F. V. Hayden. Washington: Government Printing-Office.

This bulky and profusely illustrated volume embraces the geology of the district, its mineralogy, topography, archæology, ethnology, and, lastly, its palæontology. In all these departments good work has been done, and important results are not wanting. In a mine on Mount McClellan, near George Town, solidly frozen masses were found at the depth of more than 200 feet from the surface. The walls and roof of the workings are lined with ice-crystals. The cause of this remarkable phenomenon is not known, though it is surmised that chemical changes are in progress in the surrounding rocks which effect a fall of temperature. What these processes may be remains to be discovered. The deposits of tellurets and of uraniferous ores in Colorado are remarkably rich, and splendid specimens have been obtained.

Mr. A. S. Packard, jun., contributes a report on several injurious insects, especially those attacking the cranberry. If the cranberry of America is identical with that of North-eastern Europe we should regard its extinction with much equanimity, and we can assure all who admire this fruit that they can easily reproduce its flavour by adding a stale decoction of nut-galls to the common red currant. An instance is given of a beetle (*Monohammus titillator*) which had remained alive in wood for at least fifteen years.

CORRESPONDENCE.

IMPERFECTIONS OF NATURE.

To the Editor of the Monthly Journal of Science.

Sir,—The writer of the article "Is Nature Perfect?"—which appeared in your number for April—might have usefully controverted a mistaken notion as to the difference between natural objects and works of art. We are often told that the latter, however beautiful they may seem to the naked eye, on examination with a high magnifying power betray a multitude of flaws and defects, whilst the former will bear the most rigid microscopic scrutiny. This is by no means universally the case, not a few natural products displaying a want of symmetry and finish when viewed with a strong lens. To our unaided sight the antennæ of a male gnat appear like beautiful plumes, but if magnified they remind us irresistibly of a bottle-brush. Or take the finger-tip of the most delicate lady; to the eye it seems beautifully smooth and even, whilst with a lens we perceive its surface to be rough, furrowed, and rugged. These cases are by no means singular.—I am, &c.,

Plain Facts.

SOUND AS A NUISANCE.

To the Editor of the Monthly Journal of Science.

Sir,—I fully agree with the writer of an article in your April number, that man's inability to shut out sound at will is a misfortune. But is it not possible that the vibration set up by powerful sounds might have a disturbing action upon our nerves even were we unable to hear? Thus the sensations akin to sea-sickness which some persons, myself included, experience if saluted by the deep notes of a large organ, seem to depend more on the undulations of the floor than upon the sound as such.—I am, &c.,

H. H.

PROCEEDINGS OF SOCIETIES.

Royal Society, *May* 8.—" On the Sensitive State of Electrical Discharges through Rarefied Gases," by William Spottiswoode, P.R.S., and S. Fletcher Moulton. It has frequently been remarked that the luminous column produced by electric discharges in vacuum tubes sometimes displays great sensitiveness on the approach of the finger, or other conductor, to the tube. This is notably the case when with an induction-coil a very rapid break is used, or when with any constant source of electricity an air-spark is interposed in the circuit leading to the tube. The striking character of the phenomena, and the opportunity which they showed for affecting the discharge from the outside during its passage, led the authors of this paper to consider that a special examination of this sensitive state would be desirable. If a conductor be made to approach a tube conveying a sensitive discharge, due to an air-spark in the positive branch of the circuit, a series of effects is produced, of which the feeblest and the strongest are the most pronounced. In the first case, the luminous column is repelled by the conductor; in the second, it is broken into two parts which stretch out in two tongues towards the point on the tube nearest the conductor, while a negative halo appears between them. These effects are proved to be due to the inductive action of the conductor, or more particularly to re-distributions of electricity in it, co-periodic with the air-spark, and not to any permanent charge. Instead, however, of connecting a point on the tube with a large conductor or with earth, it may be connected with one or other terminal of the tube. And a further study of the subject shows that all the phenomena due to action from without may be produced by means of one or other of these connexions. The effects described need not be confined to a single patch or ring of conducting material placed upon the tube; but they may be produced many times over in the same tube by a series of rings arranged at suitable distances. By this means the column may be broken into a series of sections, all terminating with well-defined configurations towards the negative end, and having greater or less length, according to the position of the rings. The authors give evidence, derived mainly from the revolving mirror, and from the discharges of a partially charged Leyden jar, for the following conclusion:—That the passage of the discharge occupies a time sufficiently short in comparison with the interval between the discharges to prevent any interference between successive pulses. Certain experiments are then described which indicate that the discharge is effected, under ordinary circumstances, by the passage through

the tube from the air-spark terminal of free electricity, of the same name as the electricity at that terminal. The authors are further led to the conclusion that the discharges at the two terminals of a tube are in the main independent, and that they are each determined primarily by the conditions at their own terminal, and only in a secondary degree by those at the opposite terminal. Having traced the relation between the two parts of the discharge, and having found means for controlling their range and influence, the authors were led to enquire whether there be any experimental evidence of the state of the tube during the occurrence of the discharge. The phenomena appear to require for their interpretation that, in front of the pulse coming from the (positive) air-spark terminal, there is, during the interval between the pulses, a rising negative potential: this is entirely swept out by the pulse as it advances along the tube; after which the process is repeated. The condition of things behind the pulse is more difficult to determine; but an experiment with the telephone gives reason to think that parts of the tube nearer to the non-air-spark end are in a condition to demand relief, before those nearer to the air-spark terminal have ceased to require it. And on this account the discharge may, perhaps, be more nearly represented by a lazy tongs than by a bullet. The marked similarities in the phenomena, and the predisposing circumstances of striation or non-striation, as well as in the terminal peculiarities of the two kinds of discharge, point strongly to the conclusions that all vacuum discharges are disruptive; and that sensitive differ from non-sensitive discharges mainly in the scale of the discontinuity due to the disruptiveness, causing a difference between the two classes of phenomena analogous to that between impulsive and continuous forces in dynamics.

"On the Relation between the Diurnal Range of Magnetic Declination and Horizontal Force, as observed at the Royal Observatory, Greenwich, during the years 1841 to 1877, and the Period of Solar Spot Frequency," by William Ellis, F.R.A.S. In this paper the author draws attention to the long series of magnetical observations which have been made at the Royal Observatory, under the direction of Sir George B. Airy, K.C.B., Astronomer Royal. Commencing in the year 1841, the observations for a few years consisted of eye-readings of the various instruments, made every two hours; since the year 1848 the motions of the magnets have been registered by photography, according to a plan arranged by Mr. Charles Brooke. Attention is briefly drawn to the question of magnetic variations, and to the circumstance that examination of the Greenwich records shows that, in addition to the ordinary diurnal and annual changes, there appears to exist, in the magnetic diurnal ranges, an inequality of marked character and of longer period, resembling in its features the well-established eleven-year sun-spot period. The following are the general conclusions supposed to be derived from the whole inquiry:—

1. That the diurnal ranges of the magnetic elements of declination and horizontal force are subject to a periodical variation, the duration of which is equal to that of the known eleven-year sun-spot period.
2. That the epochs of minimum and maximum of magnetic and sun-spot effect are nearly coincident; the magnetic epochs, on the whole, occurring somewhat later than the corresponding sun-spot epochs. The variations of duration of different periods appear to be similar for both phenomena.
3. That the occasional more sudden outbursts of magnetic and sun-spot energy, extending sometimes over periods of several months, appear to occur nearly simultaneously and progress collaterally.
4. That it seems probable that the annual inequalities of magnetic diurnal range are subject also to periodical variation, being increased at the time of a sun-spot maximum, when the mean diurnal range is increased, and diminished at the time of a sun-spot minimum, when the mean diurnal range is diminished.

Conclusions Nos. 1, 2, and 3 appear to be sufficiently certain, but the evidence in favour of No. 4 is not so decisive.

"On the Results of the Magnetical Observations made by the Officers of the Arctic Expedition, 1875-76," by Staff-Commander E. W. Creak, R.N., attached to the Admiralty Compass Department. This narrative and results form the sequel to the "Memorandum on Terrestrial Magnetism," prepared by Prof. J. C. Adams, M.A., F.R.S., and Capt. F. J. Evans, R.N., F.R.S., published in the "Manual and Instructions for the Arctic Expedition," 1875," suggested by the Arctic Committee of the Royal Society. The *Alert* and *Discovery* left Portsmouth on May 29th, 1875. On September 1st, 1875, the *Alert* reached her winter quarters at Floeberg Beach, lat. 82° 27′ N., long. 61° 22′ W. The magnetic observatory, constructed entirely of snow, was situated at a distance of 200 yards from the ship. It was divided into three distinct houses 27 feet apart, and in these the horizontal force magnetometer, Barrow's dip-circle, and the differential declination magnetometer were secured to snow pedestals. As no stove was used the range of the thermometer registered was from $-10°$ to $+4°$. The *Discovery* took up winter quarters at Discovery Bay, 53 miles S.W. of the *Alert*, in lat. 81° 44′ N., long. 65° 3′ W., on August 26th, 1875. The magnetic observatory house was constructed of wood, and stood on the shore 197 yards from the ship, and the pedestals formed of the ship's binnacles sunk into the ground were found to be very firm. For the differential declination magnetometer an ice house was built on the ice-floe 90 yards from the ship, the magnetometer being frozen on to a pedestal of ice. One of the chief subjects of interest in the magnetical results of the observations

at the winter-quarters' observatories is that of the diurnal variation or inequality of the declination, and frequent magnetic disturbances, the latter especially, as the ships wintered in a region remarkable—as is proved—for an absence of brilliant auroras, and in which no connexion was observed between the appearances of that phenomenon and movements of the declinometer magnet. Hourly observations were made with the differential declination magnetometers, and on all occasions of great disturbance special observations at frequent intervals. The greatest range of the declination was observed on February 19th, 1876, about the same hours, at Floeberg Beach and Discovery Bay, reaching 5° 9·4′ and 5° 47·9′ respectively. The smallest range was observed on January 12th, when only 0° 4′ was recorded at the first-named station, and 0° 6·9′ at the last. On an average about every eighth day the higher values of the daily range were attained, and comparing the highest with the lowest scale-reading during the whole period, it shows that the magnet moved over 8° of arc. For computing the mean hourly values of the disturbances of the declination, the formula used by Sir J. H. Lefroy, in the published volume of his "Magnetical and Meteorological Observations at Lake Athabasca," &c., was adopted. From the values of the mean hourly disturbance, without regard to sign, it is found that the disturbing force never ceases, and that in the mean monthly values it *decreases* as the winter solstice is approached, and *increases* rapidly towards the equinox. Taking the difference between the mean easterly and mean westerly disturbances, it is found that the easterly disturbance is both moderate in amount and monthly change when compared with the westerly. The comparison of the disturbances at Kew and the two winter-quarters' observatories of the Arctic Expedition, 1875-76, appear to confirm M. Gauss's conclusion, that "the synchronous disturbances of the same element not only differ widely in amount, but occasionally appear to be even *reversed* in direction." The appearances of auroras and the synchronous movements of the declinometer magnet were subjects of special observation during the stay of the *Alert* and *Discovery* at their winter quarters. On all occasions they were observed to be faint, with none of those brilliant manifestations which are described by our own officers as seen at Point Barrow, and by the Austro-Hungarian Expedition in Franz Josef Land, where the magnetical instruments were so sensibly disturbed. These phenomena were not observed either in the *Alert* or *Discovery*,—especially no connexion between magnetical disturbances and the appearances of auroras could be traced. The following description of the aurora observed on November 21st, 1875, is given by Commander Markham and Lieut. Giffard in their abstract of observations at Floeberg Beach:—"Between 10 and 11 p.m. bright broad streamers of the aurora appeared 10° or 15° above the north horizon, stretching through the zenith, and terminating in an irregular curve about 25° above the south horizon, bearing S.S.W.

During the aurora's greatest brilliancy the magnet was observed during five minutes to be undisturbed." The aurora was visible on forty-nine days between October 22nd, 1875, and February 27th, 1876. The observations from which the foregoing magnetical results have been obtained were made by Commander (now Captain) A. H. Markham and Lieut. G. A. Giffard of the *Alert*, and Lieuts. R. H. Archer and R. B. Fulford, of the *Discovery*.

PHYSICAL SOCIETY, *March* 22, 1879.—Prof. W. G. Adams, President, in the chair.

Capt. Abney, R.E., F.R.S., read a paper "On obtaining Photographic Records of Absorption Spectra." Absorption spectra have hitherto been recorded by the difficult process of hand copying; but the discovery by Capt. Abney of a silver salt sensitive to all rays in different degrees renders the photographic method available. The records thus obtained are photographs of the spectrum of the naked light of the source, and of that of the same light reduced by insertion of the absorbing material in its track, and these are taken parallel, so that the dark absorption lines can be readily compared. Examples of these were thrown on the screen. This method can be used as a new weapon in attacking solar physics, and determining whether or not compound bodies exist in the sun. Absorption spectra to compare with the sun's can be got for compound bodies by burning the matter in question in a flame in front of the slit, and passing a bright light through the flame.

Prof. Guthrie, F.R.S., then read a paper "On the Fracture of Colloids," as illustrated by experiments on the breakage of glass plates, either by pressure or heating at the centre or round the circumference. Circular plates of glass pressed at centre or circumference break in radial lines. However supported, a plate breaks in the same fashion if heated in the same way. If heated in the middle the crack is peak-shaped, like an obelisk on a double pedestal, two cracks forming the outline, with sometimes a third down the middle. The two cracks unite before they reach the edge on one side, and (as afterwards pointed out by Prof. W. G. Adams) the three extremities of the two cracks all meet the edge at right angles to it. The crackage varies with the size and shape of the plates, the flame, and kind of glass; but the type is the same for all. Cracks cross each other. Prof. Guthrie defined a crack as the line where the ratio of cohesion to strain is least, and likened it to the lightning flash.

April 26.—Mr. C. V. Boys gave an account of some experiments made by Dr. Guthrie and himself on the subject of Arago's rotation. The experiments were begun with a view to determine if the drag on a copper disk when a magnet is made to revolve beneath it, or on the magnet if the disk is made to revolve above

it, could be made use of for determining the velocity of running machinery. They made the magnet revolve, and obtained the angle of deflection of a disk suspended by a torsion thread (the hair-spring of a watch). They found, as Snow Harris and others found before, that, other things being equal, the drag is directly proportional to the speed, so that if the torsion of the thread could be relied on, and the strength of the magnet did not change, a perfect velocimeter could be constructed. They consider that this method is better than observing the deflection of a magnet over a revolving disk, as in this case they are limited to less than a right angle, and changes in the absolute magnetism of the earth would affect the results. They also determined the effect of change of distance, thickness, diameter, and nature of the disk, &c., their results agreeing with those of former experimenters. They observed that the effect of concentric circular cuts was far greater than that of even many radial cuts; and that when radial sectors were entirely separated from each other the effect was much less than when these were united at the centre. They then experimented on liquids by suspending a sphere or cylinder of the liquid between the poles of a revolving electro-magnet, and succeeded in getting a decided and measurable effect. The importance of this is very great, for they have thus a means of determining the conductivity of liquid electrolytes by currents induced in the liquid without the use of electrodes and without polarisation.

Dr. Guthrie stated that as the push on the liquid is directly proportional to the current quantity, they hope to measure the conductivities of liquids, and connect these to the conductivity of solids through the intervention of mercury.

Prof. Sylvanus Thompson then communicated five laboratory notes from University College, Bristol. The first related to the source of sound in the Bell telephone receiver. Two theories are now being discussed as to this effect: the molar theory regards the motion of the diaphragm in mass as the source of sound; the molecular theory finds it in the molecular motions of the magnetic core of the instrument. Prof. Thompson applied his method of getting magnetic curves with iron filings dusted on gummed glass to this problem. He found that when no currents passed in the telephone the magnetic lines springing from the pole of the magnet are gathered together on the diaphragm opposite, over a central region, which is magnetised lamellarly, or like a magnetic shell. The rim of the plate beyond this region is, however, magnetised radially, and between these two zones there is a neutral circle. It was remarkable, too, that the lines of force touching the plate were bent back around the circle, forming a kind of valley. When the current passed in the coil in a direction so as to reinforce the magnetism, the lines are gathered more closely on the central region of the plate. If the current diminishes the magnetism the lines are, on the other

hand, repelled from the plate. The neutral line is also altered. In the first case it shrinks in size, in the second it expands. A small thick disk is wholly magnetised lamellarly; a disk entirely magnetised radially becomes slightly conical in shape. In the actual telephone the disk is flat at the middle and conical at the edges. As the neutral ring shifts, the diaphragm will assume new nodal lines. Dr. Thompson concludes that the molecular theory is not, therefore, necessary to account for the speech of the telephone, although it may assist. As confirming this view, he found that with iron rings round a cardboard diaphragm and an iron centre-piece the enunciation was good, though the *timbre* was altered; whereas with radial pieces of iron on the cardboard the *timbre* was good, but the enunciation bad.

Dr. Thompson next wrote on a saw blade with a magnet, and dusted iron filings on it, which arranged themselves so as to trace the writing. This is usually shown on a steel plate, but a saw retains the virtue for six or eight months. A modification of this experiment due to himself consisted in writing on the blade with one pole of a powerful battery, the other pole being connected to the end of the blade.

The third "note" recommended the use of fine steel fibres, got by breaking iron gauze of thirty-two meshes to the inch, instead of iron filings for exhibiting magnetic lines. The fourth note showed that the lines of force got by filings fixed on cards are magnetic, that of a magnet acting as a magnet. The fifth note explained that solid magnetic "figures" could be got by coating iron filings in shellac to make them light, and floating them in water, or by mixing filings in a soft paste of plaster-of-paris, which could be cut into sections on hardening.

May 10.—Mr. Wollaston explained the construction of Gower's improved form of Bell's speaking telephone. The older form, made of wood or ebonite, is open to the objection that it has a very weak voice, soon gets out of adjustment from changes of temperature, and requires a twisted hand wire, which is liable to break. Gower's form has a comparatively loud utterance, is constant, and does not require to be held in the hand, but may be laid on a table or hung on a wall, a speaking-tube leading from it to the operator's ear or mouth. The "call for attracting attention is also within the Gower telephone itself; whereas in the hand telephone it is an auxiliary apparatus. Every organ of the old telephone has been modified to form the Gower. The magnet in the Gower is of a horse-shoe form, very powerful, the two poles being brought very close together, and each pole is mounted with a small coil of fine wire. The diaphragm is much thicker and larger than the Bell diaphragm. The case is of brass, to expand equably, and a speaking-tube is fitted to the front of the diaphragm. The call consists of a musical reed attached to the diaphragm, so as to be opposite a small slit in

the latter. To sound the call it is only necessary to send a sharp puff of wind up the speaking-tube, and the reed gives out a note which is heard throughout a room at the distant end. Speaking and cornet music was transmitted to the instrument exhibited, between the third storey over the hall and the meeting. It was very distinct and audible several feet from the receiver. Speaking done some 30 feet from the transmitter was also sent. Conversation was likewise carried on while considerable noise was being made in the room.

Prof. W. F. Barrett then gave an account of some attempts which he had made to overcome the induction clamour on telephones caused by the ordinary telegraph currents on neighbouring wires. He had tried recently the Bell telephone on a line from Dublin to Armagh, 95 miles long, but the induction noises completely stifled the speaking, whereas the Edison transmitter gave good results. The clamour could be got rid of either by neutralising the induction currents or by eliminating the noises from the speech.

Mr. Wollaston pointed out that a perfect cure for induction on underground wires consisted in twisting the going and returning wire of the telephone circuit round each other.

Mr. Wilson then read a paper "On the Divisibility of the Electric Light by Incandescence." The question of divisibility resolves itself into our being able to divide a single incandescent source into a number of smaller ones giving the same total illumination. The author concludes that this can be done by arranging the subdivided sources in "multiple arc," or parallel circuits, provided the total mass, length, and sectional area of the united sources be the same as in the original single source. The objection that increased radiation from the various sources would diminish the first total of light and heat can be met by making the smaller wires still smaller than is theoretically required so as to generate more heat. The author regards the "voltaic arc" as probably falling under the same law, the mass, however, being smaller in this case.

Dr. Coffin then exhibited a Trouvê Polyscope, which consists of a small hand incandescent platinum wire electric light, designed for illuminating the more inaccessible cavities of the body in surgical examinations. The current is supplied by a Planté secondary battery, and the light is half enclosed in a small silver reflector, fitted with a convenient handle. The apparatus is portable. Dr. Coffin found that it was open to several objections, which he has remedied. Dr. Coffin has superseded the secondary battery by a Leclanché battery of eight elements, made by Messrs. Coxeter and Sons, in which the carbon pole is replaced by a copper plate faced with platinum, and no porous diaphragm is employed. This gives a constant light for hours.

NOTES.

Biology.

Mr. E. Gittins, of Tivoli, Queensland, in a letter to the Editor, communicates some interesting facts concerning ants. He writes :—"If meat shows the least possible tendency to decompose—and it will do so in the course of twelve hours in summer —the ants will find it, though suspended by a wire or string from the house-top or the top of a tent. The ant perceives decomposing animal matter at a long distance, and does not go exploring for such matter, but goes straight to it from the ant-hill. A snake killed in the Bush is generally placed on the branch of a tree, so as to be seen by travellers, and as soon as decomposition sets in the ants find it, and the flesh is soon carried off to the ant-hill; even their own comrades, when killed, are carried off to the underground cells. They never stay to feed, but they take up the booty and off they go." The writer then describes a number of experiments, showing that portions of meat placed near ant-roads were overlooked till putrefaction set in, and were then eagerly carried off. He remarks that "ants that feed on saccharine matter are as difficult to keep off as the carrion-feeders; they smell the sugar, and endeavour to get at it wherever it may be placed. The largest kind of sugar-ants will feed until the cold air of night comes on, and then fall into a stupor and there remain during the day." We should feel much obliged if our correspondent would determine the two following points:—Whether his meat ants prefer tainted meat to fresh when both are placed equally near, as, *e.g.*, close to one of their roads; and whether they will attack animal matter in an advanced stage of decomposition? It certainly seems that they occupy a more prominent place among "Nature's Scavengers" than has been hitherto supposed.

According to M. Max Cornu, a new malady is attacking plants belonging to the order Rubiaceæ preserved in hot-houses in France. An *Anguillula* deposits its eggs in the roots, which swell and decay, while the plant speedily perishes. The depredator is closely allied to the species which has occasioned so much havoc in the coffee-plantations of Brazil.

M. G. Bonnier has laid before the Academy of Sciences an anatomical and physiological study of the nectaries of plants. He criticises the views of Messrs. Darwin, Müller, Lubbock, &c., and in opposition to their views contends that the dimensions of the corolla, the development of colour in the flowers, of perfumes, spots, and stripes, are not correlated to the formation of

nectar, and are independent of the visits of insects. He maintains that, in nectariferous diœcious plants, insects do not visit the male flowers first and the female flowers afterwards, the greater visibility of the former being indifferent. One and the same flower may be visited in various manners by the same insect. The form of a flower may be altered without sensibly modifying the visit of insects. Insects may very often collect the nectar of flowers without effecting their fecundation; the insect-guests of one and the same flower differ according to the quantity of nectar which it produces, and, as this quantity varies with altitude and latitude, the insect-guests of one and the same species differ in different countries. We cannot conclude, from facts observed, that the colour of flowers, the shape of the corollas, &c., are arranged for the exclusion of insects not adapted for cross-fertilisation. In short, there is no reason for admitting a reciprocal adaptation between insects and flowers. There are, further, nectaries without external nectar and all intermediate stages. There are also numerous nectariferous tissues unconnected with flowers. The nectariferous tissues, floral or extra-floral, whether they emit a liquid or not, are special nutritive reserves, in direct relation with the life of the plant. [Without seeking to anticipate the discussion to which this paper must give rise, we cannot avoid pointing out that the facts detailed by M. Bonnier are by no means incompatible with the theory of a mutual adaptation between flowers and insects.]

A writer in "Science Gossip" calls attention to the peculiar position of the mouth in sharks as very difficult of explanation on the Natural Selection hypothesis.

The small annual sum allowed for the purchase of specimens for the zoological department of the British Museum is to be reduced from £1200 to £900.

An anti-vivisection hubbub has sprung up in Germany,, and has now reached a dangerous stage. Unless our Teutonic colleagues are watchful they will some day find research fettered as it is amongst us.

A collection of Lepidoptera from the mouth of the Amur River, exhibited at the February meeting of the Entomological Society, was strikingly European in general character, some of the species being even identical.

M. Lichtenstein has communicated to the Academy of Sciences an account of *Ritsemia pupifera*, a kind of cochineal insect living on the elm, and holding an intermediate position between the Cocci and the Phylloxeræ. In August there appear, on the bark of the elm, small red unisexual lice, with six-jointed antennæ; they evolve a cottony matter, and lay in it not eggs, but pupæ. From these are developed, in the following May, the bisexual individuals, the males of which are apterous, having nine-jointed antennæ and no rostrum. The females, which

appear a few days later, much resemble the autumnal generation, but their antennæ are eight-jointed. This mode of reproduction, anthogenesis, has been hitherto traced among the Phylloxeræ and all the Pemphigians. We now recognise it among the Coccidæ.

M. Ch. Richet has studied the mode of contraction of the muscles of the crayfish. He remarks that in the muscles of the tail the shock of contraction is very short, whilst in the muscle of the claws it is much longer than in the muscles of vertebrate animals, excepting the cardiac muscle. The claws of the crayfish, if separated from the body and protected from heat and evaporation, preserve their contractility for more than four days.

Dr. L. Frédéricq, in an inaugural dissertation, shows that the plasma of the blood is made up of fibrinogen, coagulating at 56°; paraglobulin, coagulating at 75°; and serin, at 65°. He finds that the coagulation of blood is a phenomenon in which gases do not intervene in any manner. The carbonic acid is shared between the globules and the plasma (or serum) in the same manner both in circulating blood and in coagulated blood. The red globules are capable of absorbing a notable quantity of carbonic acid.

The "Colonies and India" gives a description of a species of *Coccus* found in Yucatan, and known locally as the niin or neen. It feeds on the mongo, and secretes a large quantity of semifluid fat used for various purposes by the natives. If strongly heated a portion evaporates, leaving a plastic waxy matter capable of serving as a varnish. If burnt, it changes to a thick semi-fluid like dissolved caoutchouc, but which solidifies on standing. Numerous applications of these substances are possible.

The editor of the "Scottish Naturalist" recommends, for special observation this season, the possible effects of the abnormal winter and spring upon birds, insects, molluscs, &c.; what species have suffered most; how the reappearance of migratory and hybernating species may be influenced; and especially the degree of colour-variation, and whether such variation, if it occur, tends to melanochroism or leucochroism. Such observations, he justly considers, will aid in throwing light upon the causes influencing the geographical distribution of species.

Mr. R. Ward records, in the "Standard," the discovery of certain fossil remains in excavations now in progress at Charing Cross. The identified specimens are tusks and molars of an extinct elephant (*E. primigenius ?*), a horn of the Irish elk, and bones and teeth of a gigantic ox (*Bos primigenius*).

According to the "American Naturalist" there is reason to fear that the biological department of the great Survey of the Territories is to be abolished. The admirable work which for many years has been done in this department pleads trumpet-tongued against its discontinuance.

Benzol, taken internally, reduces the arterial tension by one-half, and thus aproximates in its action to nicotin. The venous tension is at the same time increased, and the action of the heart accelerated. The secretions are modified; glycosuria is produced in guinea-pigs, but rarely in rabbits, and never in dogs. Phenic acid has been in some cases detected in the blood and the urine.

M. J. Rambosson has communicated to the Academy of Sciences ("Comptes Rendus," April 14th) a paper on the propagation of nervous phenomena. He proposes the following law: —"A purely physical movement may be transformed into a physiological movement, and into a psychic or cerebral movement, by transmission to these different media; and reciprocally a psychic movement may be converted into a physiological and into a physical movement by a corresponding transmission, without any alteration in its nature,—that is to say, it reproduces the same phenomena after all these transmissions and transformations on returning into the same medium.

Dr. Broca has received the brain of an individual of the species *Gorilla Savagii*, aged 2½ years. In the fresh state it weighed 416 grms., which is heavier than the specimen sent in 1876 by Dr. Négre, which weighed only 363 grms., and belonged to another species.

At a recent session of the Anthropological Society of Paris a debate took place on the origin of the blonde race of mankind. Some of the speakers considered that the region of Turkestan was their original seat, whilst others—in particular Madame C. Royer—maintained that they had originated in Europe.

MM. Bancel and Husson have communicated to the Academy of Sciences observations on the phosphorescence of the flesh of the lobster. They consider it due to a fermentation in which carbo- and phospho-hydrogens are liberated, and which is destroyed by putrefaction, just as the bacteria of carbuncle are destroyed by the vibriones of putrefaction.

M. L. Collot has discovered the true *Phylloxera vastatrix* upon *Vitis caribæa*, a wild species of vine found in the forests of Panama, far removed from any vineyards or localities where the true vine (*V. vinifera*) is cultivated. This strongly confirms the opinion that the *Phylloxera* is indigenous in America.

Experiments conducted in the laboratory of Prof. Kühn, of Heidelberg, lead to the conclusion that the so-called "visual purple" of the eye is not essential to vision. It is wanting in several animals which we must consider to be possessed of the sense of sight, and in men it is absent in the "yellow spot,"—that part of the retina in which vision is most distinct.

Dr. A. Horwarth, writing in "Pflüger's Archive" (xvii., Heft I. and II.), contends that—in addition to heat, light, oxygen, and nutrition—rest is needful for the production and maintenance of

life. He finds that bacteria are incapable of living in the arteries of animals. Continued motion for twenty four hours hinders the multiplication of *Bacterium termo* and of the *Bacillus* of Cohn, whilst if left in absolute repose they increase rapidly. [This conclusion does not hold good universally; certain low vegetable forms, known familiarly as sewage fungus, seem to flourish best in waters which move swiftly, and are scarcely to be detected in the same waters when perfectly at rest.]

Physics.

From a Report on the Electric Light on the Thames Embankment, which has been submitted to the Metropolitan Board of Works by Sir Joseph Bazalgette and Mr. Keates, we learn that the total cost of twenty lamps per night, of five and a half hours, was about 5·73 pence per lamp per hour. With regard to the value of the light, it is found that with opal shades not less than 59 per cent of the whole light produced was wasted, while with shades made of frosted glass the loss only amounted to 29·9 per cent. Comparing the light produced from gas with the electric light, it is shown that the cost of gas equivalent in power to the electric light is not much over one-half the cost of the latter.

At a recent meeting of the Academy of Sciences M. Jamin submitted a new electric burner, which he also recommended to chemists and physicists as a blowpipe. Two carbons are supported vertically abreast, hinged below, and drawn together at the top by a spring. A current is sent up one (A), down the other (B), then round a rectangular circuit inclosing the two, and passing first round A by current attraction the carbons are drawn apart, and the arc appears at the top and descends gradually, consuming one or both carbons. When the action of the rectangle is sufficient, the arc driven beyond the points is like a gas-flame, and M. Jamin receives it on a piece of lime, magnesium, or zirconium, getting intense light. It is also so hot as to fuse the lime. For the electric light this burner has considerable advantages—such as simplicity, since it requires no mechanism and requires no preliminary preparation beyond a support and the coke points; mechanical economy, since the number of flames is almost doubled; augmentation of light, since each of the new foci is almost twice as powerful as those of the old construction; quality of the light, which is whiter; more advantageous arrangement of the foci, which direct their greatest quantity of light downwards, where it is wanted, instead of up into the air, where it is useless; and, lastly, economy of combustible material.

M. Boudet de Paris has communicated to the Academy a paper "On the Electrical Inscription of Words." The transmitting apparatus is a very sensitive microphonic speaker, the carbons of which, instead of being pressed by a spring, are simply main-

tained in contact by the pressure of a small piece of paper folded in form of a V. The vibrations of the diaphragm of the receiving apparatus cannot be written, since the movements of the style, however delicate the apparatus, can scarcely be distinguished upon the lamp-black. To enlarge the magnetic vibrations of the receiver, the cover and the diaphragm of a Bell's telephone are taken away, and on the wood of the instrument there is fixed the end of a small, stiff, steel spring. The other end of the spring abuts on the surface of the magnetic nucleus, surrounded by its coil; to this extremity is soldered a small mass of soft iron weighing about 10 grms., and upon this mass, and in the produced line of the axis of the spring, is fixed a light style of bamboo, 10 centimetres in length, and terminating in a slender whalebone pen.

An improved siren with an electro-magnetic regulator is described by M. Bourbouze in the "Comptes Rendus." By means of this instrument sound can be made to pass from 8162 vibrations per second, through all the intermediary notes, to 128 vibrations.

The following resolution respecting the standard unit of Micrometry has been passed by the Royal Microscopical Society:—"That, in the opinion of this Society, the 1-100th of a millimetre is too large a unit for micrometric measurements, and that it is not expedient at present to prescribe by any formal resolution the adoption of a fixed standard for Micrometry."

The oil immersion objective has been made successfully by Messrs. Powell and Lealand.

Herr Zeiss proposes the following fluids as substitutes for oil of cedar wood for use with oil immersion objectives:—

1. Chloride of cadmium in glycerin ($CdCl_2$), 1·504.
2. Copaiva balsam oil, 1·504.
3. Chloride of zinc in water ($ZnCl_2$), 1·504.
4. Sulpho-carbolate of zinc in glycerin, 1·501.

Prof. Abbe considers the chloride of cadmium in glycerin very good optically, but somewhat too thick for convenient use. The oil of copaiva balsam is in every respect equal to oil of cedar wood, but not quite so fluid, and therefore better adapted to objectives of such large working distance as the $\frac{1}{8}$th. Chloride of zinc is not suited to prolonged observations, as after a few minutes' use it deposits small crystals on the slide and front lens: great care is required in the use of this and other aqueous fluids, lest the brass setting of the objective should be corroded. Platinum has been suggested as a material for mounting the front lens, but Herr Zeiss has found that its want of rigidity is fatal to its employment, as it will not stand when turned to the mere shell required.

THE MONTHLY

JOURNAL OF SCIENCE.

JULY, 1879.

I. THE ANOMALOUS SEASON.

THE exceptional weather which the British Islands, along with Western Europe as a whole, have experienced during the last eight months, makes meteorologists of us all. Men are eagerly asking whether the present season is a calamity hitherto without precedent, showing perhaps a lasting deterioration of our climate, and announcing the return of a glacial epoch, or a mere periodical phenomenon recurring at fixed intervals? If we accept the latter, or cyclical, hypothesis, of how many years must we suppose the cycle to consist? There is a widespread tendency to connect recurrent phenomena, famines, pestilences, inundations, storms, and even commercial depressions, with the periodicity of the sun's spots—a term between ten and eleven years. That many facts speak in favour of such a supposition is evident, and it may therefore be worth our while to consider whether a pre-eminently severe and ungenial season may be looked for every ten or eleven years. We must ask, in the first place, how, in looking back over the past, are we to recognise these bad seasons? Mere meteorological returns, average, maximum and minimum temperatures, quantity of rainfall, and even hours of sunshine and proportions of the equatorial and the polar currents, throw but an imperfect light on the subject. It is not so much the quantity of cold, of wind, of rain, and of clouded skies, as their manner of distribution, which decides whether a given season shall be pleasant and fruitful or the reverse. The best criterion of the character of the winter and the spring must be found, we think, in certain pheno-

mena of the organic world, such as the leafing and flowering of trees. In like manner the nature of the summer and the autumn may be most conveniently judged by the ripening of seeds and fruits, by their abundance or scarcity, and their quality. This is merely extending a principle fully recognised in climatology. If we say that in a certain district the sugar-cane, or the banana, or the cacao, or the date-tree flourishes, everyone versed in botanical geology will at once form a fairly precise notion of its climate. In like manner if we say that in England, in a certain year, the sloe was in full bloom on May 13th, we at once characterise that season.

What evidence, therefore, has the vegetable life of the country to give for or against a recurrence of bad seasons every ten or eleven years, between each pair of which a strikingly good season is interposed, though not generally in the midway ?

Within the past forty years there have been four, or perhaps five, signally inclement years: the winter 1844-45, with the succeeding spring and summer ; 1854-55 ; 1860-61 ; 1870-71 (a less decided case) ; and lastly, 1878-79. The first of these seasons showed the ordinary features of a prolonged winter, a backward spring, and a wet and cold summer. In some parts of the north of England skating was practicable in March. The Rev. L. Jenyns—who has tabulated the earliest, average, and latest times of a number of organic phenomena, as observed in the years from 1820 to 1831—notes that in 1845 these occurrences were later than his latest dates, the differences being, however, variable in their amount. Hence we learn that different plants, as might be expected, are very unequally affected by bad weather in their times of leafing or flowering. In a bad season there is not merely a general translation of periodic phenomena to a later part of the season, but there is also respective displacement. Animals, we must observe, are a less trustworthy guide than plants, because they more readily escape our notice. We may, for instance, register the first appearance of some bee or butterfly on a certain day, whereas had we been in another field we might have observed it a week earlier. The summer of 1845 fully maintained the character of the earlier portion of the year.

The season 1854-55 was in almost every respect similar. The cold set in early and continued late. Frosts, snow-storms, and sleet continued into June, and on the morning of July 1st a frost cut off the potato-plants over much of the north and east of England. No fine weather ever came,

and the harvest was late and deficient in quantity and quality.

In 1860-61 all these characters were still further intensified. Already in October, 1860, so severe a frost occurred that rocks and railway embankments were fringed with icicles. On the morning of Christmas-day was observed the severest cold probably ever experienced in England. Large well-established apple-trees, and even oaks of a yard in girth, were killed. Through the spring and summer the polar current still retained its ascendancy, and in many parts of the north of England frost was experienced almost every night in the month of July. The corn rotted where it had grown, and was ploughed into the ground in the ensuing season.

The claims of the season 1870-71 to rank in this evil catalogue are not unquestionable. It was undoubtedly the severest winter between 1860-61 and 1878-79, and the snow which fell on December 22nd lay unmelted into February. There was, further, no summer weather till the beginning of August. But November and the earlier part of December, 1870, were mild and genial; and in March, April, and May, notwithstanding an excess of easterly winds, the leaves and flowers were not far behind the average in their time of appearance.

We now come to the present season, the characteristics of which are too fresh in the public mind to need recapitulation. The following facts will show the effects upon vegetation :—

	Mean Time.	1879.
Sloe and plum flower ...	April 7	May 13
Maple in leaf	April 10	May 24
Lilac flowers	April 30	June 3
Ash in leaf	April 30	May 25
Oak	May 2	May 30
Horse-chesnut flowers ...	May 3	June 12
Whitethorn flowers... ...	May 5	June 4 (a few flowers in sheltered places).

The animal world is much less gravely affected. The cuckoo was first heard and seen here on April 25th, two days before its mean time of appearance, and the swallows returned to us on April 20th, only one day behind its average. The nightingale, also, was singing at its accustomed season, though among branches nearly leafless. These facts, we

submit, show that migratory birds do not hasten or delay the time of leaving their winter-station by reason of any mysterious knowledge of the weather prevailing in the country whither they are bound. Among insects *Gonopteryx Rhamni*, *Vanessa Urticæ*, and *V. Io* were seen as early as March 7th, their respective average times, according to Jenyns, being March 10th, 27th, and 31st. *Euchloe cardamines* appeared this year on June 5th—nearly a month behind its average time. *Callidium violaceum* was seen this year exactly at the same time as in the two previous seasons. Aphides have already descended in swarms upon fruit-trees, and are committing serious damages. Hence we may argue that long and severe winters have little direct influence upon insect life, and that there is consequently little hope of frost ridding us of noxious species. Not a few countries whose winter temperature is far below our own are much richer both in insect species and individuals than in England.

If we next look to the intervals between the five exceptionally bad seasons we have mentioned, we find them separated respectively by 10, 6, 10, and 8 years. If we reject the claims of 1870-71 we have, of course, instead of the two last mentioned, a single space of 18 years. Before attempting any comment upon these figures it may be useful to turn to the exceptionally fine seasons. As a starting-point we may take 1868 as utterly unquestionable in its character. It was preceded by a winter milder and shorter than the average. The equatorial current early got and maintained the upper hand. March was milder than an ordinary May, and the middle of July brought a splendid wheat-harvest. In October wet weather set in, but unattended with cold. Christmas-day was warm and genial, with primroses and violets in bloom. The winter was not marked by either severe, lasting, or often-repeated cold. The two following summers and the intervening winter, though inferior to the splendid season of 1868, were, to speak in the most guarded manner, a very fair average. Since then no summer of an exceptionally fine character has occurred, so that we have the whole period between 1870-71 and 1878-79 without an intervening season of heat.

Turning to the past, if we wish to find another year similar to 1868 we must go back forty-three years, to 1825, when the wheat-harvest also fell in July. Still there have been in the meantime seasons very much superior to anything experienced since 1869. Thus the summer of 1857

was dry and warm. In the succeeding winter the only frost experienced was in February, and was of no long duration. The summer of 1858 was even finer than its predecessor, and was followed by winter free from frost and snow, save from about November 12th to 17th. Here, therefore, we have two epochs of heat separated by an interval of about ten years. Had the series been carried out we should have had another warm epoch in the year 1878 or 1879, or in both, followed by unusual cold in 1880-81.

It will be apparent, on examining the above facts, that an unusually warm summer does not stand alone. As a rule it is preceded or followed by mild winters, and may be regarded as a culminating point from which the temperature descends on either side through a total space of about two years. With exceptionally cold and prolonged winters the case is inversely similar; they are either ushered in or succeeded by chilly, wet summers, and the depression of temperature is rarely limited to a single season. Thus the summer of 1862, though not absolutely frosty like its predecessor, was still cool, and not until 1863 had we any true summer weather.

Much of this becomes intelligible if we remember that the character of the seasons in Western Europe is more immediately at least determined by the conflict of the two great atmospheric currents, the polar and the equatorial. In what may be called average years the opponents are about equal in strength, and have their battle-ground over the British Islands. Hence the proverbial fickleness of our climate, the cloudiness of our heavens, and constant possibility of warm weather at Christmas, or of frost in summer or early autumn, according as one or other of these aërial rivals wins a temporary advantage. Sometimes, however, though rarely, the equatorial current gets decidedly the upper hand, as in 1868, and retains it for a year or more. In this case the battle-ground is transferred to Norway, whilst we are deeply and continually immersed in southerly winds. The weather then is not merely warm, but dry, for these winds do not deposit their moisture unless chilled by the near proximity of a polar current of air. More commonly, however, as in the present season and in 1861, the polar current gets and maintains a long superiority. The conflict is then transferred to Spain, and we are completely plunged in icy winds. If a southerly or south-westerly air blows for a short time, its stock of moisture is immediately precipitated by contact with the chilled earth or by polar

currents in the upper regions of the air, so that the only result is a torrent of rain followed by a fresh rush of the north-east wind. Even the storms which cross over from America fail to give any relief; they are deflected southwards by some unknown cause, and strike the coasts of France, Spain, and Morocco.

But these considerations, after all, take us but a very little way; we ask why should the equatorial current prevail in one season and the polar in another? Why is either, when supreme, apt to retain its sovereignty so long? Why is the change from a cold to a warm epoch, or inversely, generally effected about the autumnal equinox? Although the cold and the warm seasons by no means exactly and invariably succeed each other at intervals of from ten to eleven years,—or otherwise we should now be enjoying a recurrence of 1868,—we feel by no means free to maintain that the alternation in question is not connected with the periodicity of the solar spots. The exceptions may very possibly be residual phenomena due to the intervention of some as yet unsuspected cause, which may either displace the epochs of greatest heat and cold or may obliterate one of them altogether.

A practical consideration is how the approach of an abnormal season may be foreseen. If northerly winds begin in October, and continue with little intermission till the end of the year, there is already room for grave apprehension. But if, after an early commencement of winter, there is no decided change before the end of February, a cold late spring and a chilly summer become almost a matter of certainty. Hence it will be seen that the writer is not one of those who expect heat and drought as characteristics of the approaching summer.

We may at any rate safely say that a severe and prolonged winter is far from announcing a warm summer. Nor is it safe to conclude that in a late spring the fruit will escape the effects of frosty nights. Such frosts in a late season simply occur in June instead of May.

II. THE HISTORY OF VESUVIUS DURING THE LAST TEN YEARS.

By G. F. Rodwell, F.R.A.S., F.C.S.,
Science Master in Marlborough College.

THE earlier eruptions of Vesuvius were of a violently paroxysmal character. The pent-up forces did not discharge themselves little by little at frequent intervals of time, but a long period of tranquillity, or of slight dynamic activity, was followed by one of great eruption. Prior to the eruption of A.D. 79 the mountain was not even regarded as an active volcano. From this period until the year 1139 there was no eruption of any magnitude, and then more than five hundred years elapsed before the mighty outburst of 1631, which entirely changed both the shape of the mountain and the aspect of the surrounding country. From this time the eruptions occur more frequently. During the 18th century the mountain was rarely in a state of rest, and more than four-fifths of the recorded eruptions occurred in this and the present century. These, as far as, and including the eruption of 1868, have been described so fully by Prof. Phillips in his admirable account of the mountain, that we need only refer to them, and may pass on at once to the history of Vesuvius since 1868.

The eruption of 1868 terminated in November, and the mountain remained quiet till the end of December, 1870, with the exception of a few fumeroles of the crater, which emitted gases, and deposited near their mouths sublimations of chloride and sulphide of copper, and sulphide of potassium. Early in 1871 the seismograph of the Observatory showed signs of disturbance; slight detonations occurred within the crater, and incandescent particles were discharged in small quantity. On January 13th an aperture made its appearance on the northern side of the cone, from which a little lava issued, and around which a small cone was soon formed. A red-coloured smoke soon afterwards issued from it, and incandescent scoriæ were ejected, while the detonations proceeding from the great crater were louder and more frequent. Lava continued to flow from the small cone, at short intervals, until the beginning of March, when the cone subsided, leaving a cavity about 30 feet in depth, the walls of which were covered with sublimations, while at

the bottom a small cone 6 feet in diameter made its appearance, and ejected smoke and steam with a hissing noise. This lilliputian cone increased in size by the accumulation upon its sides of the scoriæ which it ejected, until it rose to a height of 16 feet above the brim of the cavity; it then commenced to discharge lava, which flowed into the Fossa della Vetrana to a distance of about 1000 feet from the new cone. This lava is very leucitic in appearance, and appears to consist almost entirely, according to Palmieri, of minute crystals of leucite imbedded in a homogeneous paste. The stream continued to flow slowly in the direction of Canteroni for several months.

In October, 1871, another small crater opened near the edge of the large cone of Vesuvius, and in November, lava was emitted from the great crater, which ran down the western slopes of the cone. In the following January the crater of October became active; loud bellowings were heard, and it emitted lava and projectiles. In February the activity somewhat abated, but in March an opening was made in the side of the cone, from which a good deal of lava was discharged into the Atrio del Cavallo. The stream flowed for a week, and then ceased. On April 23rd the seismograph showed a good deal of disturbance, and on the 24th a considerable quantity of lava was emitted, and it continued to flow for two days. Early on the morning of April 26th a number of persons went with inexperienced guides to see the new lava streams: as they were proceeding towards the summit the great cone was suddenly rent open in the north-westerly direction, commencing at the new cone and extending to the Atrio del Cavallo, and a copious torrent of lava proceeded from the fissure. Soon afterwards two small craters appeared near the summit of the principal cone, which discharged incandescent particles and quantities of white ash. The whole series of effects took place so suddenly that eight medical students who had approached too near to the scene of action were overwhelmed by the lava, and a number of persons were injured by the hot ashes and scoriæ. The great fissure of the principal cone was both broad and deep, and it extended for about 1000 feet into the Atrio. A ridge of little hills, like a miniature chain of mountains, marked the course of the fissure. The highest point of the ridge was about 150 feet above the plain. A smaller fissure opened on the south side of the cone, and a small stream of lava flowed from it towards Camaldoli.

The lava which flowed from the great fissure on the north-

west side of the mountain divided itself into two branches, the larger of which was 875 yards in width, and it flowed to a distance of 1425 yards in the Fossa della Vetrana in three hours. It then descended into the Fossa di Faraone, where it again divided into two streams, one of which ran over the lava of 1868 in the Piano delle Novelle, while the other flowed through the Fossa di Faraglione over the lava of 1855, and reached the villages of Massa and S. Sebastiano. The stream passed between them, and afterwards continued its course through an artificially constructed channel, until it reached some highly cultivated ground, which it overwhelmed. The lava moved with great rapidity; between 10 and 11 a.m. it traversed more than 3 miles, and covered an area of 2 square miles. The supply of lava fortunately ceased at this source, otherwise in less than twenty-four hours the stream would have reached Naples and flowed into the sea. About one-third of Massa was destroyed, and less than one-fourth of S. Sebastiano. The stream which separates the two villages is more than half a mile in breadth and 20 feet in depth.

On the night of April 26th, 1872, the Vesuvius Observatory lay between two streams of lava, and the inmates found the radiated heat very oppressive. The lava seemed to ooze through the sides of the cone. The two craters which had opened near the edge of the great crater gave vent to loud bellowings, produced great clouds of smoke and ashes, and projected volcanic bombs to a height which Palmieri estimated at nearly 1500 feet. This would imply an initial velocity of 655 feet per second. The white ashes were carried by the wind as far as Cosenza. Dark-coloured sand, lapilli, fragments of scoriæ, and much smoke were at intervals emitted during the eruption. Many people fled in terror from Naples; Palmieri remained, however, at his post in the Observatory.

On the morning of April 27th the lava ceased to flow. It was noticed that in three several places in the lava bed great masses of smoke were emitted, and projectiles were driven into the air, as if small volcanic rents were existing beneath. But this effect, which sometimes lasted from fifteen to twenty minutes, was probably due to the sudden evolution of gases produced by the passage of the lava over wet places. On the evening of the 27th the pine-tree cloud of smoke from the crater became darker in colour, and frequent flashes of lightning were emitted. On the following day the pile of ashes and lapilli was so great that it darkened the air, and great terror pervaded the inhabitants of Resina,

Portici, and Naples. On the 29th a strong wind blew from the crater towards the Observatory, and the scoriæ broke some of the windows. Ashes accumulated to a depth of $7\frac{1}{2}$ inches on the roof. Towards midnight the noise of the craters diminished; the next day the detonations were few and far between; and by May 1st, 1872, the eruption was completely at an end.

On ascending to the summit of the cone Palmieri found the great crater divided into two parts by a wall with vertical sides 750 feet in height. The interior showed no sublimations, but alternate beds of scoriæ and compact lava. Fumeroles emitting sulphurous anhydride, hydrochloric acid, and sulphuretted hydrogen at a temperature of 150° C. were found at the outside edge of the crater.

Palmieri regards the great eruption of April 26th, 1872, as the last phase of the eruption which commenced early in 1871. He calculates that the total amount of lava emitted amounts approximately to 20,000,000 cubic metres, about three-fifths of which was deposited on pre-existing lavas. Its destruction was estimated at 3,000,000 of lire. Among the products of the eruption were noted:—Black oxide of copper, chloride of lead, chloride of iron, chloride of ammonium, chloride of calcium, and chloride of sodium. Traces of the chlorides of thallium and lithium were also detected.

For some months after the dying-out of the eruption of 1872 Vesuvius remained in a quiescent state, and the only manifest sign of the hidden fires appeared in the few fumeroles on the floor of the crater. These, however, gradually increased in number, the temperature rose, and steam and carbonic acid were abundantly evolved. Afterwards the carbonic acid diminished, and sulphurous acid made its appearance. Early in January, 1875, the writer ascended the mountain, and found the crater full of steam and sulphurous acid. It was quite impossible to descend into it. As the wind from time to time blew the fumes over the edge of the crater we suffered from a feeling of suffocation, and violent coughing was produced. One of our party wore a pea-jacket of fluffy serge dyed with indigo, and the sulphurous acid was in sufficient quantity to bleach it superficially.

Later in the year hydrochloric acid appeared—a sure sign that the volcanic activity was increasing. This gas is produced by the action of silicates at a red-heat upon salt in the presence of aqueous vapour, and is a constant product of intense volcanic action in all parts of the world. To-

wards the end of the year (December 18th) a deep fissure opened in the floor of the crater, at the bottom of which molten lava could be seen. The surging liquid mass afterwards rose to the top of the fissure, and a cone was thrown up over the point of greatest activity. From this lava flowed in small quantities and at intervals during 1876, 1877, and 1878. It overspread, and gradually raised, the floor of the crater, until, on November 18th, 1878, it rose to the edge of the lowest portion of the rim of the crater on the north-west side of the mountain, and ran down the great cone towards the Atrio del Cavallo. The flow soon ceased, but the new cone continued to exhibit considerable dynamic activity, and is still ejecting scoriæ and evolving volumes of steam.

When the writer visited the scene of the new eruption, a few weeks after its commencement (December 29th, 1878), the cone exhibited considerable activity. We ascended with a guide from Resina, and the ascent thence to the foot of the cone occupied an hour and three-quarters, while that of the cone itself—which slopes at an angle of 32°, and is composed of loose ashes—occupied fifty-five minutes. The cone on the north side presented precisely the same aspect as in 1875. On arriving at the rim of the crater we turned to the west, until we came to a portion which had been broken down by the recent flow of lava. Near this north-western extremity of the rim we turned aside, and descended to the floor of the crater by a very steep path. At the bottom of the crater we at once found ourselves upon the new lava, which in many places was red-hot a few feet beneath the surface. Dense fumes of hydrochloric acid escaped from fissures in the lava nearest to the new cone, and certain large cavernous places in the lava were coated with brilliant red and yellow incrustations of sesquichloride of iron—often erroneously described as sulphur. Masses of nearly white porous lava were sometimes found near the cone. These had probably been submitted to the action of hydrochloric acid at a high temperature, which had removed the iron from the mass in the form of chloride. Such decomposed masses consist almost entirely of silicates of lime and alumina: they may be artificially produced by passing hydrochloric acid over volcanic scoriæ heated to redness in a porcelain tube.

Palmieri has detected chloride of calcium and chloride of lithium among the products of the last eruption; also boracic acid and various sulphates. In specimens collected in December we have found, among other substances, chloride

of aluminium, and microscopic crystals of a compound of copper, probably chloride. The lava is black and lustrous, and it contains a good deal of leucite. It does not resemble that which was emitted in 1872.

The new cone of November, 1878, presents several points of great interest. It is so small, and its dynamic activity is of such a character, that it is possible to approach quite close to it, and thus to observe the exact nature of its action. We approached within a dozen yards of its crater, and stood nearly on a level with it on a heap of scoriæ, which sometimes shook beneath our feet. A peculiar loud surging sound was heard within the crater, such as we have heard within the Strokkr Geysir, in Iceland, but much louder and more intense—in fact the difference that one would expect between violently agitated water and violently agitated molten lava. At intervals loud detonations occurred, the ground shook under our feet, and a thousand red-hot pieces of scoriæ were shot on to a height of several hundred feet. The majority of these fall back into the crater, or on that side of the cone away from the wind; but sometimes, on the occurrence of an unusually violent outburst, the masses would be scattered far and wide. Two such masses fell unpleasantly near; the second, which must have weighed at least a pound and a half, fell within 4 feet of us. They can generally be avoided if they come down singly, but of course if a shower descends the best thing to do is to run for it. A guide had been killed by a falling projectile fifteen days before our visit. The new cone presented altogether one of the most remarkable sights we have ever witnessed; the earth-shakings, concussions, and detonations; the showers of red-hot scoriæ, rivalling any fireworks; the clouds of steam and lurid smoke, and the entire surroundings—all contributed to make it a remarkable sight, and to prove that a really great eruption, like that of 1631 or 1872, must be altogether the most wonderful sight in Nature.

In the last number of the "Bullettino del Vulcanismo Italiano," which was published a few weeks ago, we look in vain for any news of Vesuvius. In the "Cronaca Vesuviana" an account of our visit in December (communicated to "Nature," and very inaccurately translated for "La Nature") is reprinted with all the errors. Of the products Prof. Palmieri writes—"Le sublimazioni, peculiare obbieto di studi scientifici, sottoposte continuamente all' azione delle pioggie, non han potuto accumularsi in

gran copia. Tra' soliti cloruri e solfati si è confermato allo spettroscopio la presenza dell' acido borico e del litio." A correspondent of "La Nature" writes from Naples on January 24:—"L'éruption du Vésuve vient de recommencer. Ce soir a 7 heures 30 minutes, la coulée est superbe, la flamme plus vive et le torrent de feu plus animé qu'il ne l'a été pendant toute la période antécédent. . . L'éruption continue au grand cône; au sortir de ce qu'on appelle ici la 'Fenètre' la lave se déverse en deux courants dont l'un suit le lit de la coulée antérieure vers Somma, et dont l'autre se dirige vers Naples." No augmention of intensity seems to have taken place since the above was written, and the eruption which commenced last November still justifies Palmieri's appellation of *una modesta eruzione.*

A record of the history of Vesuvius during the last decade would be very incomplete without reference to a notable addition to the literature of the mountain, in the "Report on the Chemical, Mineralogical, and Microscopical Characters of the Lavas of Vesuvius from 1631 to 1868," by Profs. Haughton and Hull, printed in the "Transactions of the Royal Irish Academy" for March, 1876. Herein the authors have endeavoured to discuss the composition of the Vesuvian lavas as a branch of the Indeterminate Analysis, founded on what Prof. Haughton calls "the principle of least paste." These lavas are mainly composed of nine minerals, viz., leucite, plagioclase, magnetite, olivine, augite, hornblende, mica, nepheline, and sodalite, of known composition, together with an unknown quantity of a paste of unknown composition. The object of the authors is to group the constituents given in the ultimate analysis of the lavas into their several compounds; for example, to say how much of the silica exists in leucite, how much in augite, and so on. Of course it is well nigh impossible to say how the various constituents of lava are grouped in the form of different distinct minerals, but we can gain some idea of the abundance or scarcity of certain minerals in a lava by the microscopic analysis. In applying the principle ofleast paste Prof. Haughton has proceeded on the assumption that "of the numerous solutions possible that one will occur in Nature which involves the largest amount of Definite Minerals and the least amount of Indefinite Paste."

The following proximate and ultimate analyses will illustrate the result of Prof. Haughton's work;—

	Lava de Granatello. Vesuvius, 1631.	Lava Stalactite. Vesuvius, 1868.
Silica	48·54	46·58
Alumina...	14·86	20·00
Iron peroxide ...	4·17	3·20
Lime	11·89	9·09
Magnesia	5·75	3·16
Soda	2·71	2·74
Potash	6·45	7·35
Iron protoxide ...	4·82	5·69
Titanium protoxide	0·21	0·27
Manganese protoxide	1·18	1·07
Phosphoric acid ...	0·18	Not determined
Fluorine...	None	None
Chlorine...	Not determined	Not determined
Water	0·16	0·32
	100·92	99·47

The same lavas with the constituents grouped according to the principle of least paste:—

	Lava de Granatello.	Lava Stalactite.
Leucite	33·60	41·80
Anorthite	0·60	18·20
Magnetite	4·45	3·75
Olivine	trace	—
Augite	41·20	22·60
Hornblende	trace	—
Nepheline	10·00	—
Sodalite	trace	6·04
Apatite	0·44	—
Paste	9·71	7·61
	100·00	100·00

The percentage composition of the minimum paste in the case of the Lava di Granatello is—

Silica...	45·0
Lime	27·0
Protoxides...	28·0
	100·00

And of the other—

Silica	21·6
Lime	13·2
Iron protoxide	65·2
	100·00

The details of the calculations are given at length in the memoir, which is accompanied by a description of the microscopical examination of thin sections of the lavas by Prof. Hull, and a coloured plate giving the more marked peculiarities. We trust that the principle of least paste will hereafter be applied to the lavas of Etna, the Lipari Islands, Iceland, and Hawaii.

Addendum.—Towards the end of last month, when Etna was in a state of violent eruption, Vesuvius showed signs of an increase in dynamic activity. A telegram from Naples, dated June 11th, states that "A slight eruption has commenced to-day from Mount Vesuvius, and is gradually increasing." A private letter from Naples, dated June 16th, contains the last piece of news which we have been able to collect concerning the present condition of Vesuvius:—"It shows some small activity at the summit. The upper crater is full of liquid lava, which at times boils over, and sometimes also is thrown up, but no great amount of force is to be observed."—R.

III. A NEW THEORY OF DEW.

INVESTIGATIONS which Prof. Levi Stockbridge has made at the Amherst Agricultural College, upon the comparative temperature of soil and air, and the disposition of dew upon the earth and plants, have led him to conclusions very different from those commonly received in regard to the formation of dew. It is usually held that dew is the moisture of the air, condensed through contact with objects of a lower temperature, and that it does not form till radiation has reduced the temperature of the earth and other objects below that of the atmosphere. The experiments referred to seem to indicate that, as regards objects in the immediate vicinity of the earth, at least, the process is

the converse of this, viz., that dew is the result of condensation, by the air, of warm vapour as it rises from the soil. The course of experiments from which this novel scientific theory was deduced is outlined below.

The basis of the theory is the discovery that in summer the average temperature of the earth at night is higher than that of the atmosphere. The temperature of the earth in an enclosed space on a level with the surrounding soil, and the temperature of the air, were taken at the warmest time of day and the coldest time of night for several months, and the average temperature of the air for the season was found to be 72·940°, and that of the soil 72·061°. But the average temperature of the air at night was 49·664°, and that of the soil 56·370°, the earth thus averaging at night over 6° warmer than the atmosphere. The temperature of the soil and air at night was also taken at various points within 10 miles of the college, on all kinds of grass land and bare soil, and in the forest, and the same facts were obtained, the soil being at all times warmer at night than the air.

These results led to experiments on dew-fall. Two boxes, each of a cubic foot capacity, were filled with soil without disturbing its particles or disarranging its strata; one receiving absorbent, retentive loam, and the other peat. These boxes were placed in a trench, in an open field, level with the surrounding ground, and exposed to the weather. Through the month of June they were weighed night and morning, and unless there was a rain in the night they uniformly weighed less in the morning than at night, the loss being from 1 to 3 ounces for the loam, and 1 to 4 ounces for the peat. This, Prof. Stockbridge thought, indicated that the soil at night gave forth water, and that the moisture found on the surface of a field in the morning came from a deeper soil rather than from the air. Other similar experiments followed. In one a cabbage-plant was enclosed in an air-tight tin case. Where the stem of the plant protruded through the top of the case, wax was used to make it impossible for moisture to escape through the leaves. The can was first kept within doors and weighed night and morning, when it always showed a loss during the night of 1·21 to 1·78 grms. When left out-doors at night, with the can wrapped in cloths to prevent moisture reaching it, the loss was from 0·55 grm. to 4·23 grms., showing a loss even when there was moisture or dew on the leaves.

These experiments, continued through the season, gave Prof. Stockbridge these proofs of his proposition that the dew on the ground in the summer is the condensation of

vapour that rises from the earth:—1. The vapour of the soil is much warmer at night than the air, and would be condensed by it. 2. Vapour from the soil is soon diffused and equalised in the whole atmosphere, but in largest proportion when evaporation is taking place near the surface of the soil; and, other things being equal, plants nearest the earth have the most dew. 3. Dew under haycocks, boards, and like objects on the ground, could receive it from no other source.—*Kansas City Review of Science and Industry.*

IV. ARTIFICIAL FLIGHT.*

By F. W. Brearey,
Honorary Secretary to the Aëronautical Society.

In the Paper which I read before the Aëronautical Society at the General Meeting in 1878 I stated that, with respect to those of my models which are actuated by wing-movement, I had found a difficulty of sustaining the weight which appertains to the living example. But from experiments which I have lately made I find that the weight to be sustained depends upon power and strength of material employed, and also upon the right application of that power. For instance, in the wing-action of the bird, that portion of the pectoral muscle which depresses the wing is considerably more powerful than that which elevates it, because its function is to sustain the whole weight of the bird by impact upon the air. In my models I attain that action by affixing india-rubber cord, more or less in tension (according to the power used), underneath the wings, so that when the wings are elevated there shall be a strong downward impetus. There is, however, a disadvantage not shared by the bird, —the upward stroke in the model absorbs much power in working against the tension of the india-rubber underneath. Given that power, and with material to endure the pressure considerable weight can no doubt be sustained, What immediately concerns us, however, is this:—Can the weight equal to that of a man and the additional power necessary to propel him be sustained by any material which man can construct light enough and strong enough?

* A Paper read before the Aëronautical Society of Great Britain.

Mr. Wenham, in his Paper read at the first meeting of the Aëronautical Society, says—"Having remarked how thin a stratum of air is displaced beneath the wings of a bird in rapid flight, it follows that, in order to obtain the necessary *length* of plane for supporting heavy weights, the surfaces may be superposed, or placed in parallel rows, with an interval between them. A dozen pelicans may fly one above the other without material impediment, as if framed together; and it is thus shown how two hundredweight may be supported in a transverse distance of only 10 feet." But I think that the difficulty of actuating the wings of these twelve mechanically imitated pelicans would present as great a difficulty as the vibration and construction of wings of the dimension of "60 feet from end to end, and 4 feet across at the widest part," which in the same Paper Mr. Wenham estimates as necessary to support the weight of a man. If to this we add the weight of motive power—for it is evident that man does not possess the necessary power—we might, but for other encouraging reasons, give up the hope of his ever being able to navigate the air, because its possibility, in the way that a bird flies, is held to rest either upon the necessary surface extended laterally or else superposed, and in either case—were the difficulty overcome—there would remain the important question of balance, which in the bird is maintained, I believe, similarly to that unconscious muscular action in the sole of the foot and leg, when a man stands in an upright position. Under such a surface, therefore, there would be, I maintain, no safety.

I grant that a light and very powerful motor would favourably alter some of these conditions, because the rapid vibration of a much smaller surface would effect the same result, but increase of strength of structure would be necessary.

I had hoped that some rotatory action, which would have presented to the air in rapid succession all the equivalents of flight by wing-surface, would have solved the difficulty; but the only attempt hitherto made in that direction has been effected by Mr. Moy, at a cost of several hundred pounds. The velocity which was requisite in its preliminary run was fatal to the attempt, because it could not be attained even if the machine had been effective on leaving the ground.

Then what has man to propose to evade these difficulties, which appear insuperable? Need I mention that he resorts to balloon propulsion in one of the various forms which are always recurring to minds who take freshly to the subject, and which are always supposed to be new.

Is there any need for me to dwell upon this negation of all hope for those who desire to navigate, and not to float and stagger about, in the air?

There remain, therefore, the advocates of the screw-propelled plane. For the lecture-room this is a very pretty and effective experiment, and I have propelled such for years with screws both fore and aft; but I have failed to see safety to life under such a surface, even were there not another objection, and that a fatal one, to their use.

I have always said that any aërial machine, to be safe, should have sufficient power to rise from the ground. It will then possess power to control its descent. But no plane has hitherto been devised capable of rising from the ground with any preliminary run which could be imparted to it. I have watched such experiments with much interest. Mr. Linfield's ambitious attempt failed to afford any indication of rising power, although he has travelled upon wheels at somewhere about 20 miles an hour, with a surface overhead of about 300 square feet set at an angle originally. The dimensions are—Length 40 feet, width 18 feet, height 15 feet. The weight of all, including Mr. Linfield, is 304 lbs.

Mr. Moy also tried with a model weighing 1½ lbs., also upon wheels, the screws actuated by twisted india-rubber, but he failed in my presence to effect any rise at all. He succeeded in attaining only 10 miles an hour.

The preliminary run with the velocity requisite for success seems to me to be fatal to any attempt of this nature—equally so any descent from an elevation—to attain the initial velocity.

The bird with extended wings does not rise by passively holding out his wings as a plane, but during his preliminary run he either slowly waves his wings or else tremulously vibrates them, which enables him to feel the air, and gradually to obtain a fulcrum upon it.

There is yet another group of inventors who hope, by presenting a suitable supporting surface to an accommodating breeze, to be lifted thereby, and then, by the aid of gravity and this friendly breeze, to sustain themselves like a kite, and even to progress when they get accustomed to the position.

For the last three years I have myself been a continuous experimenter, and one can scarcely work in any field of science without advancing a step in knowledge. Much more likely is the investigator to be rewarded if the field has been almost abandoned. Many a nugget has been dug out of a deserted claim. I will remark, however, in passing, that

experimenters in flight are placed at a great disadvantage. The laboratory, or anything of the dimensions of a laboratory, would suffocate flight. Space and privacy are two necessities. The absence of these greatly deters progress, because for experiments in flight the object must *fly;* and we are necessarily, most of us, confined to models of small dimensions, which upon an increased scale would have more than a proportionate effect.

There is a mode of progression adopted by some fishes which is not reproduced by any flying creature, and in observing that motion—which was an undulating one—I asked myself whether in the air it would act also as a support? The fish is about the same gravity as the water; but how would the undulating motion of a loose fabric support a weight in the air? The result of my experiment was so satisfactory that it seems to me to get over the great difficulties that always suggest themselves in all plans except our own. For instance, in some designs which possessed encouraging features there seemed a want of provision for a safe and gradual descent, so that a parachute naturally suggested itself as a necessary adjunct. But the addition of a parachute means weight, and also an incumbrance. In the loose material, however, which I employ, both for support and propulsion, there *exists* the parachute, which acts upon cessation of the motive power, and brings the machine down with a gently gliding motion in the direction of its travel.

In none of the plans which have been submitted to the Aëronautical Society has there been the slightest hint that flight could be attained by imparting a wave-like action to a loose surface extended in the direction of its length; nor do I well see how it would occur to anyone to try the effect, unless he had been experimenting with flying models, so that he could readily substitute one arrangement for another. Certain it is that this discovery enables us to make a very large surface effective for support, which with wings alone appeared to be impossible. Of course, like every machine intended to find support in the air, it must be balanced; but in the model which I have made, such is its stability that an inch more or less does not affect it. So that we are at liberty to contemplate the construction of an aërial vehicle whose dimensions would suffice to maintain in wave motion six or seven hundred square feet of canvas, actuated by steam-power, and capable of supporting the additional weight of a man, whose weight, together with the machine, would certainly not exceed 500 lbs.; and we can contemplate the man as being able to move a few feet backward or

forward without much affecting the stability of the machine. His descent under the parachute action can thus be graduated at will. This can also be effected by a cord attached to the tail, which by that means can be elevated or depressed at pleasure. Placed upon wheels it has, of course, yet to be ascertained what distance of preliminary run would be required, assisted by the action of the fabric, before it would rise from the ground.

In Mr. Linfield's construction I see the framework required. The alteration would be flexible arms, and I think a looser arrangement of the cloth. The treadle action might or might not be able to vibrate the wing-arms. These flexible arms I construct of a bundle of canes, about 12 or 14 feet long, bound tightly together with whipcord throughout their whole length, and cutting off a cane every 2 feet or so in order to taper them. The wing-arm then affords lightness combined with strength, for it is almost impossible to fracture it. In this case we should be spared the weight of the screw, which is something considerable.

It has to be determined by experiment what velocity and what arc of vibration is best suited to the weight to be sustained. There will be also some relation between the arc of vibration of the wing-arm and the amount of surface in undulation.

In the model which I have made there exists a strong illustration of the assertion that any device made in miniature which will fly, can, when constructed upon a scale of utility, perform not only all, but much more than all, the promises extracted from it in the model form. In the first place, the experimenter is greatly in want of some motive power which will last sufficiently long to allow of close observation. Now in the recoil of the india-rubber which I have employed to produce the revolution of the crank which vibrates the arms, there exists only power sufficient to give a dozen strokes or so. And were I to increase the throw of the crank so as to increase the arc of vibration at the end of the wing, I should require so much greater power that either I could not with my own exertions wind it up, or else the parts would fail to bear the strain. For want of that continuous power I cannot say what amount of weight the model which I have made will carry, but I know *this* to a certainty, viz., that the weight to be carried depends upon the power and the strength of material for a given surface kept in a state of vibration. The weight of the larger model is 3 lbs. 1½ ounces, of which 7 ounces is added weight, which it easily carries. The dimensions are 6 feet wide by 10 feet

long, with about 16 square feet of surface. By this arrangement of loose waving surface I have had the smaller model (5×8) fly from my hand perfectly horizontally to the extent of 60 feet.

What has assured me that there is a necessary relation between the arc of vibration, the velocity, the weight, and the surface, is the fact that through breakage I had to review the parts, and I have never since regained that chance relationship; but the model appears to descend until the loose fabric is seen to hold the air, so that if the ballast is behind the centre of effort the surface is forced outward at an angle of perhaps 10°. Upon stoppage of the motive power there would then be no parachute action, but the machine would descend tail first. The ballast, however, properly arranged, the velocity of the model is increased, and the head being heavier, and power ceasing, onward motion continues under the concave extension of the surface.

All this would be at the command of a man with a power upon which he could depend. It is probable that if this wave-action were used in addition to a wing about a foot wide, a greater impact upon the air at starting might assist the supporting effort.

I perhaps need scarcely say that the judgment must be formed not upon the duration of flight or the distance travelled, but upon the effect observed in any part of its short flight which with my limited power I am enabled to give the model. It then becomes evident that we should only require a continuance of the power.

I think that after fourteen years' study of the subject by the members of the Aëronautical Society we are now in a far more favourable position than we have ever yet been to form a fair estimate of the probabilities of success.

The conditions necessary for flight are pretty well understood. There remain but the mechanical difficulties. How far those difficulties have now been minimised can only be satisfactorily determined by actual experiment upon a scale commensurate with the importance of the subject. And with respect to the model which illustrates this wave-action we may predicate for it that—largely constructed with power, strength, and surface—there would be as much difference in the effect as between a child's halfpenny toy, with four paper vanes pinned to the end of a stick, and the windmill whose larger vanes grind our corn.

V. BIRDS'-NESTING.

By ALLEN HARKER.

AS the time of year again comes round when the nesting operations of birds begin, it may not be unseasonable to make to our readers some plea on behalf of our native birds, and to indicate to all who may have influence to exert, or authority to exercise, on the question of birds'-nesting, methods by which alike the study of Ornithology and the interests of our bird population may be best served, and their preservation insured.

Surely of all God's creatures which delight us, those which give us the purest, most unalloyed pleasure, are the

"Feather'd songsters of the grove"!

How much of the charm of English or Scottish landscape, of deep wooded lane and open breezy heath and common, is due to the presence of ever active, ever musical bird-life, may be best appreciated by a visit to countries where the birds are songless, or where, as in many parts of the Continent, for great stretches of country, scarce a bird is to be seen. Such a visit could not fail to impress the lover of Nature with a sense of his duty to use his endeavours to preserve from the dull monotony of a birdless country the melodious fields and hedgerows of rural England.

It is not solely on behalf of the birds themselves, and their nests and eggs, that it seems desirable to awaken greater interest and action, but also in favour of a more systematic and accurate study of the subject, combined with a true scientific use of the knowledge thus obtained—knowledge which is now in a purposeless manner allowed to become lost.

The building of a bird's nest, the choice of site, the selection and gathering of materials, the deft skill that carries on and perfects the work; the number, size, and colour of the eggs, their variations, the period of incubation, the sex of the incubator, the condition of the new-born young, their growth, and first essays at independent flight—these are events in the natural history of the bird, no less interesting or important to the ornithologist than its anatomy, its distribution, or any other of the chapters in its history. But

in addition to their interest to the ornithologist, these events have ever possessed a special charm and interest for the young of all ranks of life. The beauty of the nest, and the beauty of form and colour of the egg, accompanied too by the novelty of finding such a treasure, combine to present a temptation to the finder too strong for any schoolboy to resist. The desire of possessing coloured and shining objects, which he possesses in common with the savage whose adult condition of mind he represents, leads to the destruction by him of a vast number of eggs and nests; for the youth, having taken the prize, speedily tires of his acquisition, and the eggs are soon broken, and new "sensations" sought for.

Most people will agree that such wanton destruction is to be deplored; but for ages birds'-nesting has been the schoolboy's license, and while most parents disapprove of it, few go the length of absolutely prohibiting it—still fewer use it as a means of imparting that pleasing knowledge of the natural history of birds which would soonest cure the propensity, and teach the youth at once humanity and zoology. That some efficient protection should be given to eggs and nests is the unanimous feeling of naturalists.

At present the parent birds themselves are protected during the season of nesting by a somewhat imperfect—but still, as far as it goes, a very beneficent—Act of Parliament; but it need hardly be pointed out that if a female bird is not allowed to rear any young, the chances against it (personally or by its offspring) surviving the winter, or, if it is a migratory bird, of returning in the following spring, are increased by as many times more as the number of young it might be supposed to rear; and in this way persistent and injudicious birds'-nesting may soon diminish the number of birds in a given area. And that birds'-nesting has this effect will not be doubted by any one whose study of the subject has led him to remark the diminution of small and rare birds in particular districts, or by one who has had opportunities of knowing what immense numbers of birds' eggs are annually destroyed by marauding youths.

There are always at work enough of causes—some natural, some artificial—which are not preventible, tending to diminish the numbers of our birds, more especially of our smaller song birds, either by cutting off their food supplies or by destroying their nesting grounds. Every field, marsh, or swamp that is drained, lessens the supply of insect-life on which a great majority of small birds live; every piece

of land that is reclaimed from waste robs the ground or low bush nesting birds of their *habitat;* every wood that is cut down, every gorse-patch that is burned,—in short, every advance of cultivation,—drives before it some species of birds.

It was my fortune to revisit, after a lapse of ten years, a part of the country where some of my earliest birds'-nesting exploits had been carried out. "High-farming" had taken the place of a more primitive agriculture; the thick high hedges where red-backed shrikes, bullfinches, linnets, and long-tailed tits were wont to nest, were supplanted by neat trim-cut hedges 3 feet high, and not thick enough to offer cover for the smallest of birds. The deep ditches with high grass-grown banks, once the haunt of wood wren, lesser whitethroat, or whinchat, had disappeared, and patches of gorse and heather, where redpole and linnet once dwelt, had been burnt and stubbed out long ago. These causes, which for the birds' sake we may deplore, we cannot nor should we wish to prevent; and even consolation is to be found in that while one species of bird may be driven out, another suited to the new condition may follow and take its place. The richest arable land are especially the resort of the lark, who dispels the monotony with his "sweet jargoning." It is rather with preventible causes that we have to deal; and to the indiscriminate and utterly wanton birds'-nesting, for no intelligent or intellectual aim or object, which goes on in every parish in the country, a check must be applied. Here it is that the authority of parents and schoolmasters should come into force. It is in most cases due as much to *ignorance* as to wantonness or destructiveness that the youthful birds'-nester takes eggs for which he has no use—no idea of use, in fact; they serve to gratify his instinct for finding and possessing pretty objects, and then are strung on a string as an ornament, or made cockshies of as an amusing pastime. Had he been taught anything of the importance of the nest and eggs to the continuance of the parent birds,—or had any facts of their history, as of how birds differ from other animals, or how, in a sense, a nest and eggs are as much a part of the mother as the embryos of viviparous animals,—his nesting for pure wanton destruction of his spoil would at least be checked; or if he proved not amenable to such reasoning, should be forcibly prevented and heavily punished. But a remedy for checking birds'-nesting in the intelligent boy who wishes to avoid wantonness, but at the same time claims a right to make his

collection of eggs as much as another collector has one to make his of butterflies or birds, must be sought in another direction, and is worth the consideration of lovers both of birds and of intelligent and inquiring schoolboys.

In passing it must be remarked that schoolboys alone are not to be blamed for purposeless birds'-nesting, and it would be unfair to pass over as great a culprit, the amateur adult collector, whose condemnation should be as much greater as is his opportunity of knowing better. It has been well remarked that few, if any, advances have been made by human beings in their history, but have been accompanied by a concomitant development of special vices, originating in a perverted application or use of the benefits gained by the advance. The form which this aberration assumes in connection with the rise and progress of biological science is as a mania for amassing large collections of animal structures, whether shells or birds' eggs, or the animals themselves, without any reference whatever to their structure or history, or to the educational purpose they might serve, when this latter exists at all. When you see in the drawers of a collector of birds' eggs a long series of the eggs of the kingfisher or a wild duck, not one egg in each series differing in any way from another, the inane purposelessness of the thing—not to use any stronger term—is evident. No clearer proof could be given that the great majority of egg-collecting—that is, by others than schoolboys—arises from a barbarous desire of possession alone, than that it is seldom if ever accompanied by the collecting of birds' *nests*, from which probably much more is to be learned of bird history than from the eggs. I need only refer to the learned observations of Pouchet on the changes which he remarked in the building of the nests of species of *Hirundo*, to instance the interesting and important results which a study of nests might lead us to.

Compare the nest of a chaffinch with those of its congeners the greenfinch or the bullfinch, or that of a sedge warbler with that of the wood wren; how totally different they are—one feels inclined to say, comparing incomparables, more different than the birds themselves. Or to go further, compare one chaffinch's nest with another, and note the variety in material, and even in construction, adapted to some peculiarity of situation or surroundings. Here is a field for observation and comparison by means of which the philosophical student may hope to catch some

glimpses of the working of the laws which have taught birds, as they must in time have been taught, to build nests in endless variety of form and material. Little, if anything, has been written beyond mere speculation on this subject, which presents so much scope for investigation. In spite, too, of all the vast collections of birds' eggs which have been made, we are still almost entirely in the dark as to any theory, even, of the causes which have tended to produce such infinite variety of form and colour for what is really but for one and the same end.

The method of, at one and the same time, limiting the tendency to purposeless egg-collecting, and systematically extending our knowledge of the whole subject, which it is the purpose of this paper to suggest, is the formation in connection with local museums, or, where these do not exist, with larger schools, of complete educational collections of birds' nests and eggs. Under the judicious guidance of the head-master this might be done with but a minimum of wrong to the parent birds in at most three years, and the eggs and nests which would be required to complete such a collection would be many times fewer than what are annually destroyed in the same area, and would, by being carefully housed and attended to, obviate, as I shall endeavour to show, the necessity for repeated nesting in time to come.

Many years ago I assisted at the formation of such a collection for a small country museum, and our method of preserving and displaying it to the best advantage being devised chiefly with a view to economy of space and material, it may be useful to briefly describe it. We took our nests generally before any eggs—or but the first—had been laid, and binding them carefully with tape or cord we saturated them or sprinkled them with some preservative liquid (methylated spirits and corrosive sublimate solution is best), to kill any vermin and keep away moths in the future, and then dried them and packed them in drawers or boxes. The eggs we got when we could. Our collection being nearly complete, we had a wall case about 5 feet high and 12 inches wide, made with shelves sloping at an angle of 45°. On the bottom shelf we placed the large nests, such as those of the crow, rook, jackdaw, magpie, &c., putting into each nest the number of eggs usually laid, not necessarily, and indeed seldom, taken from that particular nest. On the second shelf, such nests as consist of little more than a few reeds or grasses, or a mere hollow scraped

in earth, were contained in round card-board boxes, to hold the materials together. These included the nests of the curlew, sandpiper, gulls, coot, water-hen, ducks, grebes, &c. Smaller boxes held the collection of bones on which the kingfisher lays its eggs, the few grasses that the skylark lines a hole in the soil with, and so on. Upper shelves held the smaller nests; and when they had been built in holes of trees we cut away the branch, where possible. In time we had not only a very complete collection, but an additional case of such eggs as vary from a common type, and our little museum was the favourite resort of all the schoolboys in the district; and it is certain that much less bird-nesting for mere nesting's sake was one outcome of our efforts. Sometimes we had brought us varieties which were not found in our cases—a result which was most desirable and pleasing, as it evidenced an intelligent appreciation of the uses of a collection. A schoolmaster who will but inaugurate such a collection will certainly be conferring a lasting benefit on his pupils, and sparing many a nest from careless destruction. Certain it is, too, that if every country town and village had its small, well-ordered, local museum, where the common objects of animal and plant life—which ever will possess an absorbing interest for the young—were displayed in such a manner as to convey some intelligent ideas of their life-history and relation to each other, among the innumerable benefits which would accrue, one if not the least would be that there would be created a sympathy between the animals and their keen-eyed observers, and the wanton destruction of myriads of them would be diminished.

One of the most pleasant recollections of a short residence in a French country town is associated with its admirable museum, and the character of its visitors on Sunday afternoons. On week-days the students from an *école de medecine* shared with me the examination of its well-arranged cases; but on Sunday afternoons troops of peasants and their families took the place of the systematic student, —the husband in his clean blouse, the wife in clean starched cap of marvellous and stupendous proportions, and the children in clean Sunday best. The shouts of the youngsters at the wonders of strange foreign *bêtes* were only passed by their delight at the discovery of old friends like *M. Crapaud* or *les petites papillons bleues;* while older schoolboys conferred together over cases of beetles or bottles of reptiles, or related, in audible whispers, exploits with beast

and bird which the preserved specimens recalled to mind. For two or three hours the rooms were filled with these happy-faced students of zoology; and the remembrance of Sunday afternoons nearer home, spent by British peasants in less creditable ways, rose, not unnaturally, in the mind, with the reflection that "they manage these things better in France."

In forming such a collection of birds' eggs and nests as I have described, with the desire, at the same time, to collect with such discrimination and judgment as to limit to a minimum, if not to avoid entirely, cruelty or injury to the birds, there are certain broad maxims which must be held in mind; and it is these which it is my object to strongly impress. It is well established by experience that many birds will go on laying eggs in the same nest after the loss of their first eggs; and, physiologically, there exists the most ample provision in the mother bird for such a contingency. Others will build new nests again and again after the destruction of their first efforts; but manifestly there is a limit, if only in point of time and season, to these persistent efforts at propagating their kind. No egg collector should, therefore, ever take eggs or nests after a certain date,—say, 1st of June,—except in the case of very late migrants. This limiting date should, of course, vary with different species of birds, and in different parts of the country, but should be fixed and rigidly adhered to.

A second maxim should be, that no collector should ever take partially incubated eggs, or disturb the nest in such case; and lastly, that he should never take any egg or nest at all that is not intended to form part of some new public collection, or to supply a blank in such already-established one. The common possession of a perfect collection, which might thus be speedily formed, would not only have the influences which experience has proved similar ones to have on schoolboys, but with "schoolboys of a larger growth" birds and their nests and eggs would become more familiar and interesting objects, and the ranks of ornithologists be swelled by new and devoted students. Furthermore, with such opportunities of comparing observations and discovering variations in habits of nesting, or in the phenomena connected with birds' eggs, the study of birds' nests and eggs might in time be raised to the dignity, which it scarcely at present occupies, of a science. Vast as is the number of facts which we are in possession of regarding the subject, the time scarcely seems to have arrived when these may be

formulated, and a theory of birds' eggs be established. The interesting researches of Mr. Sorby into the nature and composition of the colouring-matter of birds' eggs belong rather to the sciences of chemistry and spectroscopy than to those of oology or animal physiology. What has been done towards an elucidation of the subject may form matter for future consideration. If this appeal should induce one lover of birds to take any steps to form a permanent and accessible record of their habits of nidification, it will have more than served its object.—*The Scottish Naturalist.*

NOTICES OF BOOKS.

Evolution, Old and New; or the Theories of Buffon, Dr. Erasmus Darwin, and Lamarck, as compared with that of Mr. Charles Darwin. By SAMUEL BUTLER. London: Hardwicke and Bogue.

THE work before us appears to have a threefold object. The author undertakes the rehabilitation of certain Evolutionist worthies of days bygone—possibly too much neglected and even misinterpreted—the refutation of the hypothesis of Natural Selection, as put forward by Mr. C. Darwin and by Mr. A. R. Wallace; and, lastly, the exposition of his own peculiar views on the development of species. Mr. Butler does not appear to be a working biologist, nor indeed a man of science at all. Still, unlike the bulk of outsiders who have written upon Evolution, he rejects the old hypothesis of Special Creation, and if his contentions do not everywhere command our implicit assent, they cannot fail to supply every true naturalist with abundant matter for profitable reflection.

It is generally, and perhaps too hastily, assumed that the Doctrine of Development, as met with in the works of Buffon, Erasmus Darwin, and Lamarck, was so crude and imperfect as to necessitate its rejection by men of sound and sober judgment, whilst Mr. C. Darwin, by supplying his idea of Natural Selection, has converted the chaos into a cosmos, and rendered the adoption of Evolutionism not merely possible, but almost imperative. Mr. Butler, on the contrary, holds the very contrary opinion:—"Fresh from the study of the older men, and also of Mr. Darwin himself, I failed to see that Mr. Darwin had 'unravelled and illuminated' a tangled skein, but believed him, on the contrary, to have tangled and obscured what his predecessors had made in great part, if not wholly, plain. The older men, if not in full daylight, at any rate saw in what quarter of the sky the dawn was breaking, and were looking steadily towards it." Again we read—"Those were the days before 'Natural Selection' had been discharged into the waters of the Evolution controversy, like the secretion of a cuttle-fish." One of Mr. Butler's charges against "Darwinism" is that, shutting out purpose and intelligence from the work, it refers the formation of species to the accumulation of small and hap-hazard variations—an objection which has been urged by Mr. J. J. Murphy and by Mr. Mivart. A not less capital point is that Natural Selection, though it may preserve any variation that proves advantageous, has plainly nothing whatever to do with the origin of such variation. This

will at once appear if we reject the words "Natural Selection," and write in their stead the expression "Survival of the Fittest," which Mr. Darwin himself declares to be equivalent in meaning. Everyone will then admit that before the fittest can survive it must come into existence. Here Mr. Darwin leaves us in the dark, giving us an "Origin of Species" with the "Origin" cut out. But this is not all: the bulk of the readers of Mr. Darwin's work go away with the impression that Natural Selection is the most important cause of modification. Now Mr. Darwin certainly protests against being understood to assert that Natural Selection induces variability, and yet, perhaps from inadvertence, uses language open to a very different interpretation. Thus he declares Natural Selection the most important "means" of modification. Here Mr. Butler remarks—"'Means' is a dangerous word; it slips too easily into cause. We have seen Mr. Darwin himself say that Buffon did not enter on 'the *causes or means*' of modification, as though the two words were synonymous or nearly so. The use of the word 'means' enables Mr. Darwin to speak of Natural Selection as if it were an active cause (which he constantly does), and yet to avoid expressly maintaining that it is a cause of modification."

It will now be asked, what does Mr. Butler offer us instead of Natural Selection as a more satisfactory solution of the mutation of forms and the rise of species. Taking up the views of Lamarck and of the elder Darwin, he contends that "differentiations of structure and instinct are due to the desires under changing circumstances of an organism, which must be regarded as a single creature, though its development has extended over millions of years."

Between parents and offspring there is oneness of personality —a view which explains the phenomena of heredity and of instinct, or, as it might be called, unconscious memory. Purpose and intelligence are thus brought into play in the genesis of species, but the intelligence is not that of an Allwise Creator, which cannot be supposed as taking tentative steps, or abandoning its plans as unsatisfactory and working in a different direction.

With reference to the nidification of birds, we find a case quoted from Miss Seward's biography of Erasmus Darwin which supplies the crucial experiment asked for by Mr. Wallace in his Essay on the "Philosophy of Birds' Nests."* A pair of young canaries, who had never seen a nest built, are described as constructing their nest on the slovenly regulation pattern of their race, "even to the precise disposal of every hair and shred of wool." For such a fact Mr. Butler's theory accounts without the invocation of a mysterious "instinct."

Into the biographies of Buffon, Erasmus Darwin, and Lamarck, and into the author's exposition of their views, space does not allow us to enter. We can only quote two passages, very different

* Contributions to the Theory of Natural Selection, p. 211.

in their scope, but equally suggestive :—" Science is not a kingdom into which a poor man can enter easily if he happens to differ from a philosopher who gives good dinners, and has 'his sisters and his cousins and his aunts' to play the part of chorus to him."

" For our skull is as a kind of flower-pot, and holds the soi from which we spring,—that is to say, the brain ; our mouth and stomach are roots in two stories or stages ; our bones are the trellis-work to which we cling; *we* are the nerves which are rooted in the brain, and draw thence the sustenance supplied by the stomach; our lungs are leaves folded up within us, as the blossom of a fig is hidden within the fruit itself." We are thus likened to " perambulating vegetables turned upside down."

" Evolution, Old and New " is well worth reading."

A Manual of Organic Chemistry, Practical and Theoretical. For Colleges and Schools, Medical and Civil Service Examinations, and especially for Elementary, Advanced, and Honours Students at the Classes of the Science and Art Department, South Kensington. By Hugh Clements, of H.M. Civil Service; President of the Amateur Mechanics' Workshops Association, London ; and Lecturer on Various Sciences at St. Thomas Charterhouse and several other Science Institutions in London. London : Blackie and Son.

This Manual is in great part a reprint from the " English Mechanic," and is unquestionably an examinational work. It contains chapters on the distinction between organic and inorganic substances ; on organic analysis and empirical formula (*sic*) ; on the determination of rational formulæ ; on organic substitution ; on the theory of compound organic radicals ; the preparation and properties of their hydrides ; the distillation of coal ; on alcohols, ethers, aldehyds, organic acids, anhydrides, ketons, alkaloids, and organo-metallic compounds. Then follows a section on the identification of organic compounds ; and a description of oils, fixed and essential. We have next an account of certain articles of apparatus, a selection of exercises, and a list of " Papers set in Organic Chemistry at the Examinations of the Science and Art Department from 1868 to 1878, with Answers," —a section which occupies nearly one-third of the entire book, and which may give the student, or rather the intending examinee, some idea of the class of questions likely to fall to his lot.

That the bulk of the matter contained in the work is on a level with the knowledge of the day is unquestionable. Still we come here and there upon passages which, to us at least, seem to require emendation. Thus the author tells us that as carbonic

anhydride "is a product of the respiration of animals, it is decidedly an organic product, and should therefore be included with organic compounds"—an argument which we can scarcely admit. Blood receives a formula $C_{45}H_{40}N_6O_{15}$, as though it were a definite chemical individual. Yeast, we learn, "uninfluences it" (*i.e.*, gum). Human bile "has a nauseous fragrant odour."

On p. 50 we have, *verbatim et literatim*, the following passage:—"Picric or trinitrophenylic acid, like any acid, forms salts that crystallise well. It has the odour of tar or creosote, and has a hot taste. It is much used as a disinfectant, rendering sewage, &c., inoffensive. It quickly arrests all fermentative and putrefactive changes, &c." Now the chemist who knows the properties of picric acid will here call a halt, and, looking back, perceive that what is said concerning disinfecting and anti-putrescent properties must refer not to picric acid, but to phenol, of which the author has been previously speaking. But the student who may have no previous knowledge of picric acid and of phenol, and who can merely follow the grammatical construction of the passage, will here, we submit, be led somewhat astray.

Speaking of fats Mr. Clements says—"The fat of warm-blooded animals is generally solid, while that of cold-blooded animals *and* fish is liquid." Fish are generally supposed to be "cold-blooded animals," but this sentence shows that our "lecturer on various sciences" is of a different opinion. He continues—"The principal solid fats are butter, grease, lard, and suet, and the fluid animal fats are cod-liver, neats' foot, sperm and whale oils." It is curious that, of the fluid fats enumerated, all, save one, should be the products of warm-blooded animals.

These peculiarities, which are not the only ones that might be selected on close scrutiny, show that the "Manual of Organic Chemistry" needs revision before it can be pronounced a trustworthy guide.

Bulletin of the United States Geological and Geographical Survey of the Territories. Vol. iv., No. 4, and Vol. v., No. 1. Washington: Government Printing-Office.

The first of these two numbers contains an account of the fossil insects found in the Tertiary shales of the Green River, Wyoming, by that well-known entomologist S. H. Scudder. The collection, eighty species of which have been identified, is remarkable for the distinctly tropical aspect of its forms. Dr. D. S. Jordan reports on the collection of fishes made in Dakota and Montana by the indefatigable Dr. Elliott Coues; whilst Prof. J. W. Chickering furnishes a catalogue of phænogamous and vascular cryptoga-

mous plants collected by the same naturalist in the above districts. Mr. F. M. Endlich's paper on "Erosion-products in Colorado" would prove a most instructive study for those—and such writers exist—who deny that erosion has any share in the formation of the earth's surface. Dr. C. A. White discusses the palæontology of the "Laramic group," and Mr. J. A. Allen gives a synonymatic list of the American *Sciuri*, or tree-squirrels.

The first number of Vol. v. contains much interesting matter. Dr. C. V. Riley and Mr. J. Monell contribute a monograph of the American Aphididæ—a group of insects of loathsome aspect, but interesting from their peculiar system of reproduction, and from the ravages they commit in the fields and gardens. The first annual generation of *Schinoneura Americana*, the American elm Aphis, consisting entirely of females, issues from impregnated eggs which have survived the winter, and without sexual intercourse produces a second generation like themselves in structure. The third generation is winged. The fourth and fifth again are wingless, the sixth winged, whilst the seventh consists for the first time of individuals of both sexes. Each female deposits a single egg, which is not hatched till the ensuing spring. Amongst the natural enemies of this species the author enumerates several Coleoptera, especially Coccinellidæ, two Hemipterous species, and even the larva of a moth (*Semasia prunivora*). The misfortune is that the enemies of Aphides are, in point of number and voracity, utterly inadequate to the task set before them.

Prof. E. D. Cope discusses the relations of the horizons of extinct Vertebrates of Europe and North America. The author points out, as a consequence of the principle of descent, that the types of each age have taken their origin from the generalised types of preceding ages, there being no descent from the more specialised types. Hence in discriminating the subdivisions of geological time we have to be guided by the disappearance rather than by the appearance of species. He finds that portions of all the faunæ of all the primary divisions of geologic time have been recognised on both continents,—that parallels requiring general identification of the principal divisions of these faunæ may be detected, but that exact identifications of restricted divisions may be made in a few instances only. Between the tables of extinct animal and vegetable life there is a discrepancy. The plant-life of North America reached its present condition one epoch earlier than did the higher Vertebrata. Hence either the animal-life of North America has lagged behind that of Europe, or the flora of America has been in advance of the European. The author mentions that the earliest land vertebrates had a persistent *chorda dorsalis*.

This number further contains papers on the miocene fauna of Oregon; on the birds of Dakota; on the fossils of the Jurassic trias of south-eastern Idaho and western Wyoming; on the fossilised forests of the volcanic tertiaries in the Yellowstone

National Park; on the conditions of preservation of invertebrate fossils; and a supplement to the bibliography of North American invertebrate palæontology.

The Journal of the Cincinnati Society of Natural History. January, 1879. Vol. i., No. 4.

In this number we have a paper by Mr. V. T. Chambers on the tongue of certain Hymenoptera. The tube in this organ is in the hive-bee 1-500th of an inch in diameter, but in some of the Andrenidæ only half that width and closed at the apex. It is curious that though Fritz Müller and Wolff, as well as the author, have demonstrated the tubular character of a bee's tongue, it should be described as solid in the latest edition of the "Encyclopædia Britannica."

The most important article, however, is a very elaborate list of the birds occurring in the Cincinnati district. We shall not be surprised if, in spite of the "British" mania prevalent among ornithologists, entomologists, and botanists in this country,—a whim which leads to little save the extirpation of our rarer forms, the range of each native species will be mapped out accurately in America sooner than in these islands.

We learn, with little surprise, that the introduction of the European sparrow into the United States is now looked upon by competent authorities as a deplorable mistake. We hope that other parts of the world, not yet suffering from the presence of this "winged rat," will take warning from the experience of our American friends. Acclimatisation is not a matter to be rashly undertaken.

Euclid and his Modern Rivals. By Charles L. Dodgson, M.A., Senior Student and Mathematical Lecturer of Christ Church, Oxford. London: Macmillan. 1879.

Mr. Dodgson's drama, or rather dialogue, opens with a soliloquy from "*Minos*," an examiner tortured by having to look over papers in Geometry, containing a mixture of the old and new systems. Minos, after some conversation with "*Rhadamanthus*," another equally unhappy examiner, falls asleep, and to him appears the shade of Euclid ready to do battle for his Manual. A discussion follows, in which the reasons for *retaining* Euclid's Manual are treated, but it is noticeable that those for rejecting it are only just mentioned; and even that is denied to many.

In fact, throughout his book, the author is far more inclined to attack Euclid's Rivals than to defend Euclid himself, so that a one-sided view of the question is shown, and Euclid's faults are kept steadily in the back-ground. Euclid and Minos then discuss the method of procedure in examining Modern Rivals, and a few alterations proposed by them.

In the Second Act Minos is discovered still sleeping, and to him enters a shade which is addressed as "*Niemand*," with copies of the Modern Rivals to be discussed. These are divided into two classes—those which reject Euclid's treatment of parallels, including Legendre, Cooley, Cuthbertson, Wilson, Pierce, and Willock; and those which adopt Euclid's treatment of parallels—these are Chauvenet, Loomis, Morell, Reynolds, Wright, the Syllabus of the Association for the Improvement of Geometrical Teaching, and Wilson's Manual founded on that Syllabus.

In the Fourth Act the shade of Euclid again converses with Minos, and, with the exception of some one or two slight additions and alterations, such as a proof that all right angles are equal, it is decided that Euclid's Manual ought to be left untouched.

The chief points of attack on Euclid's Modern Rivals are Mr. Wilson's two works—"Elementary Geometry" and the "Manual founded on the Association Syllabus." The author makes comparatively short work of Legendre's book as unsuited to beginners, though doubtless valuable to advanced students; and of Cooley's, in which a certain theorem breaks down through a faulty definition of parallel lines. But to Mr. Wilson's two Manuals he devotes nearly a third of his volume. Much of the criticism on these, however, is mere cavilling; for instance, at page 177, Minos says, speaking of Wilson's "Syllabus" Manual:—"At p. 57 I see an 'Exercise' (No. 5). '*Show that the angles of an equiangular triangle are equal to two-thirds of a right angle.*' In this attempt I feel sure I should fail. In early life I was taught to believe them equal to *two right angles*—an antiquated prejudice no doubt; but it is difficult to eradicate these childish instincts." This is mere straw-splitting; *strictest* accuracy would of course require the insertion of "*each*" before "equal," but if the sum of the interior angles had been intended to be understood "*together*" before "equal" would have been absolutely necessary. The very next paragraph, containing an accusation of the fallacy *Petitio Principii*, is another instance of cavilling criticism; for to take a line greater than half another, surely we need only take it greater than the whole, and that involves no knowledge of the bisecting point of the line.

At page 160 there is a criticism on the definition of a right angle as given by the Association for the Improvement of Geometrical Teaching in their Syllabus. This is—"When one straight line stands upon another straight line, and makes the adjacent angles equal, each of the angles is called a right angle."

Since the Association Syllabus admits of angles equal to or greater than two right angles, this is open to the objection that it does not debar the case in which one line stands on the *end* of the other, making the adjacent angles equal to one another, and to *two* right angles as right angles are generally considered. That is certainly a grave objection, but the same applies equally to Euclid's definition, or else a proof must be supplied, which is not that in the case mentioned the two lines are in one and the same straight line; and so this interpretation is debarred by Euclid's limiting clause.

In fact, though Mr. Dodgson's book is interesting and often witty, he fails to prove his point, because he takes a one-sided view of the question, and merely exhibits the blunders of Euclid's Rivals without balancing them against Euclid's own. Besides which—though perhaps it is more readable than an essay—a dialogue does not seem to be the clearest form for setting forth arguments and facts. It is certainly an advantage that the more solid part of a treatise of this sort should be enlivened here and there by lighter matter, more especially as it is so managed as not to break the thread of the argument.

The Tables of Problems—though there is a somewhat complex system of notation to be mastered before they become intelligible—are extremely interesting; more especially Table II., which is described as "consisting of theorems admitted to be real and valid, but *not* deducible from undisputed axioms."

CORRESPONDENCE.

THE SENSES.

To the Editor of the Monthly Journal of Science.

Sir,—In an article which you inserted not very long ago the possible existence of other senses than the five with which we are personally familiar was suggested. You will perhaps therefore permit me to call your attention to a lecture by Professor Pierce, of Cambridge (U.S.). He points out that auditory vibrations are not more than twenty thousand per second, whilst the slowest visual vibrations are four hundred millions of millions. Between these two limits, therefore, there is a range sufficient for more than forty additional senses, "each of which might have its own peculiar effect upon the nerves of the observer, and give a corresponding variety of information." Surely this consideration alone ought to convince us of the vast extent of our own ignorance and of the folly of pronouncing on the non-existence of whatever escapes our scanty means of research.—I am, &c.,

Caution.

HEREDITY.

To the Editor of the Monthly Journal of Science.

Sir,—A recent writer explains the heredity of talents, character, and instincts by the hypothesis that the young animal is a continuation of its parents, and enters upon life with a remembrance of their actions, which, though latent, is roused into activity by the force of circumstances. Hence the young bird, when the breeding-season comes on, builds a nest on the pattern followed by its ancestors. This assumption offers an excellent explanation of the peculiar type of nest adhered to by each species, and even of the architectural peculiarities of the solitary wasps, bees, &c., who have never even seen their parents. But I find a difficulty as regards the song of birds. Experience shows that a young bird brought up without the society of its own species will never adopt their characteristic note, any more than a child reared among wild beasts—of which there have been cases—will

speak the language of its parents. Now if a bird is a continuation of the personality of its ancestors, why should it not adopt their song as instinctively as their style of nest-building.—I am, &c.,

AN OLD NATURALIST.

THE BAND PATTERN IN ANIMALS.

To the Editor of the Monthly Journal of Science.

SIR,—In reply to J. W. S. permit me to say that the facts he mentions respecting the band pattern in animals have often been observed. In a recent number of "Science for All" (vol. i., p. 252), Mr. W. Ackroyd remarks that a bilateral symmetry of marking exists in the head of the Bengal tiger and bodies of zebra, the Indian tapir, the Aard wolf, and some of our domestic cats. He then proceeds to adduce many facts which seem to point out that colour uniformities—*i.e.*, sexual differences, bilateral symmetry, and colour distribution generally—are "regulated by some deeply-seated and symmetrically distributed portion of the organism, such as the nervous system.

I may observe that the bands are often parallel to the axial line, there being many examples of this in the British Museum. May we not, therefore, look upon the band pattern as of three kinds with respect to the axial line:—1st, the right-angle pattern; 2nd, the parallel pattern; and 3rd, what may be taken as a mixture of the two—the spotted pattern?—I am, &c.,

L. J. DE WHALLEY.

June 23, 1879.

PROCEEDINGS OF SOCIETIES.

Royal Society, *May* 1.—"On the Origin of the Parallel Roads of Lochaber, and their bearing on other Phenomena of the Glacial Period," by Joseph Prestwich, M.A., F.R.S., F.G.S., &c., Professor of Geology in the University of Oxford. Of the various hypotheses that have been brought forward since the time of Macculloch and Dick-Lauder to account for the origin of the parallel roads of Glen Roy, the one so ably propounded by Mr. Jamieson, in 1863, has been most generally received and adopted. It is a modification of the views originally expressed by Agassiz, to the effect that the barriers of the lakes—to the shore action of which both the above named geologists attributed the "roads," but were at a loss to account both for the formation and removal of barriers—had been formed during the Glacial period by glaciers issued from Glen Treig and Glen Arkaig, supplemented by others from Ben Nevis. The subsequent determination, by the Scotch geologists, of an intermediate milder period succeeded by a second cold period, led Mr. Jamieson to conclude that the extension of these two glaciers took place during the second cold period, which he thinks was of little less intensity than the first, and that, while the glacier from Glen Arkaig blocked up Glen Gluoy, the glacier from Glen Teig formed a barrier to Glen Roy. This and other objections are considered by the author to be fatal to the hypothesis advanced by Mr. Jamieson; but while objecting to this exposition of the Glacial theory, he considers that that theory affords the most satisfactory solution of the problem, only that he would suggest a different interpretation in explanation of the phenomena. Dismissing the hypothesis of local glaciers of the second period of glaciation, the author falls back upon the original idea of Agassiz with the development acquired by more recent research, and assigns the Lochaber lakes to the close of the first period of great glaciation. The general conclusions drawn by the author from the phenomena in Lochaber and surrounding district are—

1st. That at the period of the first great glaciation of Scotland the ice-sheet in Lochaber attained a thickness of not less than 2000 to 2500 feet, but that in consequence of the peculiar physiographical conditions of the district the large ice-currents from Ben Nevis so clashed with others in the Spean and Lochy Valleys that a block ensued which led to an exceptional heaping up and accumulation of the ice in front of Glen Roy and Glen Gluoy.

2nd. In consequence of the lowering and partial submergence of the land, and its conversion from a continental area to an archipelago, combined possibly with some other more general cause, an amelioration of the climate took place, attended by a gradual melting of the ice-sheet in all the lower tracts. The snow and ice wasted from the valleys and from the lower mountain summits, and, in the absence of any established water-courses, the hollows and depressions in the ice, when not fissured, were converted into pools and tarns, until the continued liquefaction opened out surface channels or interior fissures, by which the water could ultimately escape.

3rd. That pending the establishment of natural lines of drainage, and in presence at places of unusual obstruction, the water accumulated in some valleys in larger bodies or lakes; and if in those cases, the mouth of the valleys being closed by the main ice-barriers, other channels of escape—such as cols or passes—communicating with adjacent glens or valleys presented themselves, the water overflowed through these channels as soon as it rose to the height of such cols or passes. Or should the cols have been also barred by ice, that ice would have given way as soon as the increasing height and pressure of the water proved sufficient. When this happened the water would at once fall to the fixed level regulated by the col, and thus no record, such as we have in Lochaber, might be left of the presence of the original bodies of water.

4th. In the Lochaber district, while the exceptional accumulation of ice in the Spean Valley barred the entrance of the glens on the north side of that valley, their passes were also blocked by remnants of the great ice-sheet; and the formation of the detrital shelves is due to the sudden bursting of these minor barriers, when the waters of the lake were discharged with great rapidity, until they fell to the level of the col. Under these circumstances the mass of loose *débris* covering the hill sides below the line of water-level gave way, and slid atter the retreating waters, until stayed with greater or lesser abruptness, according to the angle of slope and the volume of the mass, on the discharge ceasing and the waters coming to rest. The shelves so formed, modified slightly by subsequent subaërial action, constitute the "roads."

5th. The moraine detritus in places where the glaciers clashed, and where their progress consequently became checked or delayed, tended also to accumulate or heap up, and in this way in the Lochaber glens added to the strength and permanence of the ice-barriers.

6th. While the moraine detritus was irregularly distributed under the ice, or massed in particular places, the *débris* projected on the surface of the ice-stream, and contained in its body, was either left *in situ* on its liquefaction or else was—as the result of the great floods consequent upon the bursting of lake barriers—

carried successively to lower levels, leaving here and there banks of sand and gravel at various heights on the hill sides. These destructive floods, combined with the incessant river inundations due to the same general thaw of the great ice-sheet, carricd down and spread out in the valleys and plains the great beds of gravel and sand which—with the modifications since brought about by long-continued fluviatile action—have given rise to various forms of escars, terraces, and other less-defined accumulations of these detrital materials.

May 15.—"Some Researches with Professor Hughes's new Instrument for the Measurement of Hearing—the Audiometer," by Benjamin Ward Richardson, M.D., LL.D., F.R.S.* The Audiometer consists of two Leclanché cells for the battery, a new and simple microphonic key connected with the cells and with two fixed primary coils, and a secondary or induction-coil the terminals of which are attached to a telephone. The induction-coil moves on a bar between the two fixed coils, and the bar is graduated into 200 parts, by which the readings of sound are taken. The graduated scale is divided into 20 centims., and each of these parts is subdivided into 10, so that the hearing may be tested from the maximum of 200 units to 0° — zero. The fixed coil on the right hand contains 6 metres of wire; the fixed coil on the left hand contains 100 metres. By this means a long scale from the left-hand coil is produced. The secondary coil contains 100 metres of wire. In using the instrument, one Leclanché cell has been found sufficient, as a general rule; but two have been used in instances where the hearing of the person under test has been very defective. The person whose hearing is being tested should sit in an easy position, and should not see the act of the observer in moving the microphone key. For good observation the room in which the experiment is made should be large, and all external causes of sound—such as the ticking of clocks, the vibrations of windows and doors, the moving of feet, and the singing from gas jets—should be silenced. The sitter should close the ear that is not applied to the telephone while he is listening for minute sounds, and should give his full and calm attention to the proceeding. The instrument may be considered to afford the most satisfactory means for testing the hearing power of all persons who can define a sound. The range of sound is sufficient at the maximum, 200°, for everyone who is not absolutely deaf 0°, or zero, is a point of positive silence from the instrument, or rather from the sound which it produces through the telephone. One of the first facts learned with the audiometer is the suddenness with which the sound is lost to those who are listening. The sound is abruptly lost within a range of 2°; that is, within one-hundredth part of the

* For a description of Prof. Hughes's induction-currents balance see p. 508.

entire scale. This is the case with those who are very deaf, as well as with those who hear readily. The effect of filling the chest and holding the breath makes a difference in listeners. The capacity for hearing is for a few seconds increased by holding the breath. Holding the breath with the chest not full fails to produce the same result. As a rule, the hearing of persons who are right-handed is most refined in the right ear, and as most persons are right-handed it is found that the right ear is the best ear. This rule is, however, attended with many exceptions, since, for various reasons, some persons who use the right hand exclusively, practise for some particular purpose the use of the left ear, upon which that ear becomes more acute. Connected with the last-named fact is another, namely, that by this instrument the deaf are found to fail in capacity of hearing not only by reason of physical defect, but also by failure of memory of sounds. Thus in a youth who had suffered serious defect of hearing for seven years, owing to partial destruction of the tympanum, and who in the right ear could only detect sound at 107°, there was an inability to catch all the sound lying between 130° and 107°, until he could remember what he had to listen for. By practising him then to detect the lowest sound that he was physically capable of receiving, the author got him to detect this one sound more readily than those which came higher up. By further practice all the intervening sounds became audible with equal facility. These facts, which have been confirmed by another observation on a different person, seem to indicate that deafness from imperfection of the tympanum or other parts of the organ of hearing may be increased, beyond the mere physical failure, either from some lost power of automatic adjustment in the auditory apparatus or from failure of receptive power in the cerebrum itself, so that the memory rendered imperfect is slow to assist the listener until by exercise of function its readiness is restored. By use of the audiometer the influence of atmospheric pressure on hearing is detectable. In the author's own case, when the barometer is at 30° he can hear on both sides close down to zero, but below 30° he fails by 2° on the left side to reach zero. Dr. Richardson has tried to determine in some of the lower animals whether there is the same sense of hearing as in man. In most animals it is difficult to obtain sufficient quietude to enable the observer to gather from expression or movement of the animal the information sought for. In two dogs—one a terrier, the other a field spaniel—he succeeded in making some good observations, and in them the range of hearing power seemed to be distinctly lower than it is in the human subject who has perfect hearing. In both these animals, which were healthy and in the prime of life, the first indication of the detection of sound commenced at 10° on the scale. The detection was evidenced by the sudden expression of listening, by a slight change of position, and a slight dilatation of the

pupils. This detection was clearly made on the instant, as it the sharp line of hearing were the same in them as it is in the higher animal.

The following practical memoranda are given by the author:—

"1. The audiometer will, I think, be an essential in all physical examinations of men who are undergoing examination as to their fitness for special services requiring perfect hearing, such as soldiers, sentries, railway officials, and the like.

"2. The instrument will be of great use to the physician in determining the value of hearing in those who are deaf, and in determining the relative values of the two organs of hearing. In one instance, already, I have been able by its means to detect in a person who was supposed to be equally deaf on both sides, that on one side the hearing is perfect close up to zero, while on the other side nine-tenths of the hearing is lost.

"3. In other forms of diagnosis I have found the instrument useful. In a young person suffering from acute anæmia the hearing was so defective that on the right side it failed to detect sound at 18°, and on the left side at 15°. In ten days, during which, under a new regimen, great improvement took place in strength and general condition, the power of hearing had so much improved that the right ear was good down to 12°, and the left to 3°,—an improvement of 6° on the right, and of 12° on the left side. In another person, who was subject to repeated vertigo, the giddiness occurring three or four times a-day, the hearing was so defective that although the external ear on each side was clear and the tympanum natural, no sound could be heard below 30°. Under complete rest and attention to diet the vertiginous attacks were in a few days removed altogether, and with that removal there was gain of hearing, on both sides equally, up to 5° on the audiometric scale.

"4. The instrument may be used to differentiate between deafness through the external ear and deafness from closure of the Eustachian tube—throat deafness. In my own case I fail to detect sound by the mouth at 170°, and this I find is a fair average in those who are healthy. It represents the comparative value of communication by sound through the Eustachian canal and the external ear.

"5. The instrument promises to be very useful in detecting the effects in the body of those agents which quicken or excite the circulation, such as alcohol and other similar chemical substances.

"6. The instrument promises to be of great service in determining the value of artificial tympanums in instances of deafness due to imperfection or destruction of the natural tympanum. The cotton artificial tympanums introduced originally by the late Dr. Yearsley, and the membranous tympanums introduced by the late Mr. Toynbee, F.R.S., have proved of much service; and by means of the audiometer I have been able very accurately to test their respective merits, and to compare both with tympanums

made of other material. The inquiry has led me to test different metals for this purpose, and to find in fine gold the substance for making the most useful and effective artificial drum."

"Note on the Invention of a Method for making the Movements of the Pulse audible by the Telephone: the Sphygmophone," by Benjamin Ward Richardson, M.D., LL.D., F.R.S. While experimenting with the audiometer it occurred to me that I might get a secondary or telephonic sound from the movements of the pulse at the wrist. I have effected this in a very simple manner, by adding a microphone to a Pond's sphygmograph. I mount on a slip of talc a thin plate of platinum. I place the talc slip in the sphygmograph as if about to take a tracing of the pulse. I connect one terminal from a Leclanché cell to the slip of platinum on the talc, and the second terminal from the cell to a terminal of the telephone. Then I connect the other terminal of the telephone with the metal rod of the sphygmograph which supports the talc. The instrument is now ready for use. It is placed on the pulse, in the ordinary way, and is adjusted, with the writing needle thrown back, until a good pulsating movement of the needle is secured. When the movement is in full action the needle is thrown over to touch the platinum plate, which it traverses with each pulse-movement, and completes the connexion between the telephone and the battery. The needle, in passing over the metallic plate, causes a distinct series of sounds from the telephone, which correspond with the movements of the pulse. When all is neatly adjusted the sounds heard are three in number, one long sound and two short, corresponding to the systolic push, the arterial recoil, and the valvular check. The sounds are singular, as resembling the two words "bother it." The sounds can be made very loud by increasing the battery power. This little instrument is not so good a recorder of the pulse as the sphygmograph, but it may be made very useful in class, for illustrating to a large number of students, at one time, the movements of the natural pulse and the variations which occur in disease. I call the invention the Sphygmophone.

"Note on a recent Communication by Messrs. Liveing and Dewar," by J. Norman Lockyer, F.R.S. In a paper of last December the author called attention to the importance of discussing Young's observations of the chromospheric lines in connexion with the spectra of the metallic elements. Since his paper was read Messrs. Liveing and Dewar have given a table which professes to state the number of times various lines in certain metals were seen by Young in connexion with certain reversal phenomena observed by themselves. The statements, however, made in this table with regard to the visibility of certain lines in the chromosphere do not appear to the author to be in accordance with Young's published tables, and he considers that a higher degree of accuracy than that employed by Messrs. Liveing and Dewar is necessary to determine such coincidences.

The author's eleven years' work on this special branch has led him to the conclusion that all statements of coincidences between metallic and solar lines with a lower degree of accuracy than that employed by Thalén and Young are to be avoided when possible, as they may be worse than useless—they may mislead. Although the map on which he was working was on twelve times the scale of Angström's, it would have been better if it were larger; and in saying this he adds his tribute of admiration of the accuracy of the work of those who have preceded him, notably Angström, Thalén, Cornu, and Young, with whose work he is more familiar, as it is expressed in wave-lengths.

May 24.—"Note on the Spectrum of Sodium," by J. N. Lockyer, F.R.S. I have lately been engaged in studying the spectrum of sodium under new experimental conditions. In anticipation of a detailed communication I take leave to state that the vapour given off from the metal after slow distillation in a vacuum for some time shows the red and green lines without any trace whatever of the yellow one. Hydrogen is given off in large quantities, and at times the C line and the red "structure" are seen alone. After this treatment the metal, even when red-hot, volatilises with great difficulty.

May 29.—"Researches on Explosives. No. II. (Fired Gunpowder)," by Captain Noble, late R.A., F.R.S., F.R.A.S., F.C.S., and F. A. Abel, C.B., F.R.S., V.-P.C.S. The authors preface this memoir by two tables: one of these gives the results of some analyses of products of explosion of the three services powders of Waltham Abbey manufacture—pebble, R.L.G., and F.G.—which are required to complete the series of results obtained by firing the charge in different spaces, which were given in their first memoir on this subject, and a statement of the mean percentage composition by volume of the gases and by weight of the solid products furnished by those three powders, together with the highest and lowest proportions in which each product has been obtained with the three descriptions of powder. This table includes the results of examination of the products of explosion of four descriptions of powder differing in many respects from the powders chiefly employed in their researches. The other table gives the complete results of the whole series of analyses made, showing the proportion by weight of each solid and gaseous product, and including the amount of water pre-existent in the various specimens operated on. A portion of this memoir is devoted to a discussion of a few points raised in reference to the first memoir of the authors,* on fired gunpowder, by General Morin and M. Berthelot, who were appointed by the Académie des Sciences as a Commission to report on

* Phil. Trans., 1875, Part 1.

that memoir. In discussing M. Berthelot's views respecting the objections which the authors raise against the acceptance of any chemical equation as giving even a general idea of the metamorphosis which a gunpowder of average composition may be considered to undergo when exploded in a confined space, they disclaim having had any intention to convey the impression, which indeed they consider that their expressions were not calculated to convey, that it was impossible to put into *some* form of equation a representation of a variety of reactions which, if assumed to take place simultaneously, among different proportions of the powder-constituents, might give approximate expression to the results obtained in any one particular experiment, and might, thus far, afford some approach to a theoretical representation of the metamorphosis of gunpowder. What they desired to point out and lay stress upon was the conclusive proof, which is afforded by the very great variations in composition, of the solid portions more particularly, of the products of explosion of samples of gunpowder presenting only small differences of composition (and even of the products furnished at different times by one and the same sample), that the reactions which occur among the powder-constituents are susceptible of very considerable variations, regarding the causes of which it appears only possible to form conjectures, and that, consequently, "no value whatever can be attached to any attempt to give a general chemical expression to the metamorphosis of gunpowder of normal composition." An examination of the results of the author's experiments on the heat generated by the combustion of gunpowder shows, first, that the heat thus generated is subject to very wide variarions, dependent upon the particular nature of the powder employed (the Spanish powder, for example, generates just 50 per cent more heat than the mining powder); and, secondly, that the heat evolved by the same description of powder varies in different experiments to a greater extent than is to be accounted for by errors of observation. The authors' views on this head are confirmed by calorimetric determinations in their researches on gun-cotton. In these determinations, which have been carried on with precisely the same apparatus, no appreciable difference was found in the heat evolved in the various experiments. It is of high importance to observe that the volume of the permanent gases generated is in every case in inverse ratio to the units of heat evolved, as is shown by the following table.—

Nature of Powder.	Units of Heat per Gramme exploded.	Cub. centims. of Gas per Gramme exploded.
Spanish pellet powder ...	767·3	234·2
Curtis and Harvey's No. 6	764·4	241·0
W.A.F.G.	738·3	263·1
W.A.R.L.G.	725·7	274·2
W.A. Pebble	721·4	278·3
Mining	516·8	360·3

The results given in this table are very striking. Taking the two natures of powder which commence and close the list, the heat generated by the Spanish powder is about 50 per cent higher than that generated by the mining powder, while the quantity of permanent gases evolved by the latter is about 50 per cent greater than that given off by the former. Thus it appears that the great inferiority of heat developed by the mining powder, as compared with the Spanish powder, is compensated, or at least approximately so, by the great superiority in volume of permanent gases produced. A similar relation is observed in respect to the other powders, and it would indeed appear that the pressures at any given density and the capacity for performing work of the various powders are not very materially different. This fact has been entirely verified for the whole of the Waltham Abbey powders, and in a less degree for the three other powders also. The peculiarities shown by the mining powder are so interesting that it appeared important to determine its tension when fired under a high gravimetric density. 11,560 grs. (749 grms.) of this powder have therefore been fired under a gravimetric density of unity. The pressure developed by two very accordant observations was, when corrected, 44 tons on the square inch (6706 atmospheres). The pressure obtained under similar circumstances from Waltham Abbey powder was 43 tons on the square inch (6554 atmospheres), The capacity for performing work of the various descriptions of powder was also found to be not very different, a similarity of result the more remarkable seeing that with, at all events, three of the powders there are striking differences both in their composition and in the decomposition they experience, and when in consequence material variations both in pressures at different densities and in potential energy might have been expected. With respect to the great difference in heat evolved by the Spanish and mining powders, it appears difficult to resist the conclusion that the small number of units of heat evolved by the latter is in great measure due to the quantity of heat absorbed in placing the very much larger proportion of the products of combustion in the form of permanent gases. This suggestion would also appear fully to explain the fact alluded to in the authors' first memoir, and to which they had been led purely by experiment, namely, "that the variations observed in the decomposition of gunpowder do not, even when very considerable, materially affect either its tension or capacity for performing work." A comparison between different gunpowders, or a comparison between gunpowder and other explosive agents, cannot therefore, as has been proposed, be determined by a simple measurement of the corresponding units of heat they evolve. As regards the *actual* temperature of explosion, the results of the further experiments detailed in this paper leave little doubt that the temperature named in the authors' first memoir, viz., 2200° C., is not far

removed from the truth for the principal powders with which they then experimented. The authors then discuss the constants in the equation, expressing the relation between the tension of the products of explosion and the volume these products occupy, as stated by them in their first memoir, and give values of those constants, corrected from the analyses and experiments made since the publication of that memoir, concluding their remarks upon this part of their subject with a table which gives in terms of the mean density of the powder products the tensions which would exist in the bores of guns were perfectly dry powder of normal composition suffered to expand, with or without production of work. The tensions are expressed in kilogrammes per square centimetre, tons per square inch, and atmospheres. The authors call attention to the great utility of a table (XI.) showing the theoretic work which a charge of gunpowder is capable of effecting in expanding to any value, v. The table given by them exhibits the theoretic work for all necessary volumes of v, from $v=1$ to $v=50$. The authors then discuss the causes which in the bore of a gun affect the energy realised by gunpowder, and point out that this energy varies very much with the powders employed, being in this respect dependent upon circumstances, such as the density of the powder, its size of grain, the amount of moisture, chemical composition, nature of charcoal used, &c., but that the energy may also vary considerably, even with the same powder, if the charges be not fired under precisely the same circumstances. For example, especially with slow burning powders, the weight of the shot fired exerts a very material influence upon the factor of effect, and the reason is obvious—the slower the shot moves at first, the earlier in its passage up the bore is the charge entirely consumed, and the higher is the energy realised. The same effect, unless modified by other circumstances, is produced when the charge is increased with the same weight of projectile. In this case the projectile has to traverse a greater length of bore before the same relief due to expansion is attained. The higher pressures which consequently rule react upon the rate of combustion of the powder, and again a somewhat higher energy is obtained. Both these increased effects, of course, correspond to an increased initial tension of the powder-gases, but, especially with the smaller guns, a very great difference in the realised energy may arise from other causes. The authors draw attention to the effect of retaining the shot in its seat for a greater or less time, thus giving rise in the former case to a more perfect and earlier combustion of the charge. They cite experiments to show that from this cause alone differences of energy in guns having a calibre of 12 centims. of 13 to 14 per cent have been obtained. It is pointed out that from the principles laid down it is possible, if the maximum chamber-pressure be known, to fix, very approximately, the position of the shot in the bore when the combustion of the charge

may practically be considered to be effected. The authors have little doubt that the main theories upon which they insist, confirmed as they are by experiments made or facts obtained, under very great variety of circumstances, may be accepted as, at any rate, close approximations to the truth. It is satisfactory to find that the laws which rule the tensions and temperatures of gases under ordinary circumstances do not lose their physical significance, but are still approximately applicable, at the high temperatures and pressures they have been considering. At all events, it appears certain that the rules and tables they have laid down, as based on their analyses, experiments, and calculations, may for all practical purposes be accepted as correct, and may, bearing in mind the restrictions to which they refer in this memoir, be applied to nearly every question of Internal Ballistics.

June 19.—" On the Production of Coloured Spectra by Light," by Capt. Abney, R.E., F.R.S. Last year I incidentally mentioned in a note to the Royal Society (" Proceedings," vol. xxviii., p. 291) that the production of natural colours by the agency of light, examples of which were shown by Becquerel, was probably caused by the oxidation of silver compounds employed. I have ventured to return to the subject, in order to show that the colours are so produced and are not due to interference. I have sent, for the Society's inspection, pictures of the solar spectrum on silver plates, and also on compounds of silver held *in situ* by collodion. It will be observed that the spectrum has imprinted itself in approximately its natural colours; that on the silver plates it is more brilliant than on the collodion film, but that in the latter the colours are seen by transmitted as well as by reflected light. I reserve for the present the exact details of the production of these pictures, but may say that they are produced by oxidation of silver compounds when placed in the spectrum, an exposure of two minutes being amply sufficient with a wide slit to impress the colours. The colouring-matter seems to be due to a mixture of two different sizes of molecules of the same chemical composition, one of which absorbs at the blue end and the other at the red end of the spectrum, and the sizes of these molecules are unalterable whilst exposed to the same wavelengths as those by which they were produced. I believe it possible and probable that the colours may be preserved unchanged when exposed to ordinary daylight.

METEOROLOGICAL SOCIETY, *June* 18.—Mr. C. Greaves, F.G.S., President, in the chair.

" Thermometer Exposure—Wall *versus* Stevenson's Screens," by William Marriott, F.M.S. It being the practice of some observers to expose their thermometers on walls facing north, it seemed a suitable object of inquiry whether instruments so

placed gave results comparable with those obtained from thermometers in a Stevenson stand in the open. A pair of Meteorological Office wall-screens were fixed to the brick wall of an outhouse with a northern aspect, so that the screens were in the shade except in the morning and afternoon of the summer months. The Stevenson screen was on a grass plot 17 feet square, and about 50 feet north of the wall-screen. The Paper contains the results of the comparison of the maximum and minimum temperatures in the wall-screen with those in the Stevenson stand for the twelve months ending 31st March, 1879. The figures show that the mean daily maximum temperature on the wall is below that in the open, the monthly differences varying from 0·0° to −2·1°, that for the twelve months being −1·0.° The minimum temperature on the wall was mostly higher than in the Stevenson stand, the differences varying from −0·1° to +1·3, the mean for the year being +0·5°. The individual differences, however, are sometimes much greater, the maximum temperature on the wall being considerably lower than that in the stand. For instance, the difference exceeded 4·0 five times in September and four times in March, the greatest being 6·7°: these extremes occurred on fine calm days. The minimum temperature on the wall was more than 2·0° higher than that in the Stevenson stand on five occasions in June, seven in July. and four in September. The mean daily range of temperature on the wall for the twelve months was 1·4° less than in the stand in the open. The greatest difference was on March 9th, when the range on the wall was 8·5 less than on the stand. These results seem to show that, although the mean temperature may be roughly ascertained from thermometers shaded by a wall with a northern aspect, this method of exposure affords less sensitive indications than those obtained from instruments in a properly exposed Stevenson stand.

Physical Society, *May* 24, 1879.—Prof. W. G. Adams, President, in the chair.

Mr. W. J. Wilson exhibited a new harmonograph and figures drawn by it. The figures drawn by prior harmonographs are all more or less imperfect, owing to loss of motion in the pendulums actuating the marking pen; and Mr. Wilson therefore designed a new instrument, which not only gives perfect figures, but admits of the phase of either of the two compounded motions being decreased by a known amount. In this instrument toothed wheels take the place of pendulums, the ratio of the teeth giving the ratio of the periods of the motions. By employing the device of an intermediate wheel gearing with two others, and arranging two or more wheels on the intermediate axle, a great variety of phase can be obtained for each motion. An ingenious adjustment by means of a movable nut allows the phase of either or both motions to be altered to a known extent, and thus an

endless variety of figures can be obtained. As in other harmonographs, a writing table, on which the paper is placed, and an aniline glass pen, are used. Several of the figures done also on glass were thrown on the screen, the stereoscopic effect being very apparent. In reply to a query, Mr. Wilson said that he had adapted some of the figures to the stereoscope.

Prof. Hughes explained his new Induction Balance, and showed some of its principal effects. It is well known that on an electric current passing along a wire adjacent to another wire, an induced current is set up in the second wire in an opposite direction to the first or primary current. In the induction balance two primary circuits or coils are taken, with the same current (interrupted by a microphone acted upon by the ticking of a clock) running through both; and between these is placed a secondary circuit or coil in connection with a telephone. The primary coils are so wound as to oppose each other's induction on the intermediate secondary. There is a point, moreover, between where these opposed inductive influences exactly neutralise each other. If the secondary coil be placed there no induced ticking of the clock can be heard; but if the secondary be displaced to one or other side of this point the ticking can be heard in the telephone, increasing in loudness as the secondary approaches one or other of the primaries. If the distance between the primaries be graduated into a scale this contrivance becomes a sonometer, since it gives an absolute zero of sound and degrees of loudness. It is well adapted for measuring the hearing power of the ear, and when used for this purpose it is known as the audiometer. By splitting the secondary coil into two parts and placing each close to its proper primary, so that there are four coils in all arranged in two pairs, the extremes in one primary circuit and the means in one secondary, the two opposing parts of the balance can be separated from each other, so as not to disturb each other's action, and the balance made very sensitive by the closeness of the primaries and secondaries. Prof. Hughes finds that there is a line or zone of maximum induction midway between each primary and its secondary. If a conductor, such as a piece of metal, be put in this position it has a maximum distributing effect on the balance, due probably to the electric currents generated in it by the induction. The effect is apparently proportional to the conductivity of the metal. It requires an exactly similar piece of metal put between the other pair of coils to restore the equillibrium of the balance. A very slight difference of alloy or of weight between two like coins is at once observed from the imperfect restitution of the balance; base coins are thus also at once detected. Again, it is possible for a person to tell what particular coin or coins are in one part of the balance by trial of the same coins in his part. When plates of non-magnetic metals are held vertical in the balance their distributing effect is nil; iron, on the other hand, gives its maximum effect at this

position, because its maximum effect overbalances its electrical effect. Two pieces of iron may therefore neutralise each other as cores in an induction coil. Steel is easy to balance compared with soft iron. Zinc disturbs most when placed along the sides of each pair of coils ; iron when in centre. A certain length of metal laid along the outsides of the coils produces silence. The maximum line of inductive force is midway between the coils, and there is a line of minimum force at half the thickness of each coil ; a metal placed at these lines of minimum force has no disturbing effect on the balance. Pressure, torsion, heat, magnetism, strain, and in fact all imponderable forces, are indicated, and their effects may be measured.

Prof. W. G. Adams believed that one result of Prof. Hughes's experiments will be the determination of the way in which the law of electro-dynamic induction depends on density. He also imagined that the metal when in the maximum line between the coils gathered the lines of force to itself, whereas when on the minimum lines it could not thus divert them.

Prof. Ayrton cited the early experiment of Faraday with a sheet of copper oscillating to rest between two opposite magnetic poles. The copper took a long time to stop ; but a sheet of iron placed between two like poles was soon stopped, owing to its becoming imbued with an opposite polarity, and deflecting the lines of force. He also suggested that the divergence of the results for conductivity of metals got by the induction balance from those got by the Wheatstone balance might be due to that electric inertia suspected by Sir William Thomson.

Prof. Guthrie thought that the induction balance pointed to the conclusion that the disturbing effect of a conducting mass applied in this way is proportional to the quantity of electricity generated in it under certain conditions of temperature, &c. The determination of the conductivity of liquids would be a useful application of the balance.

Mr. Chandler Roberts gave some results which he had obtained from an examination of certain alloys by means of the induction balance. He had been able to detect a difference of 1 part in 1000 in the amount of silver present in two shillings of equal weight. He also pointed out that Matthiessen divided alloys into three classes :—(1) Solidified solutions of one metal in another ; (2) Solidified solutions of one metal in an allotropic modification of another metal ; (3) Solidified solutions of allotropic modifications of both metals. For the first class the curve of electric conductivity is a straight line ; for the second, a parabolic curve ; for the third, a bent line. Mr. Roberts found that the balance gave the characteristic curve for the first class with an alloy of lead and tin, and for the second with an alloy of gold and silver. With a copper-tin alloy, which is a good example of the third class, he found the curve given by the balance to be intermediate between Alfred Rich's curve of density and Matthiessen's curve of conduc-

tivity, and considers that the balance is influenced by the density as well as by the conductivity of the metal interposed.

Prof. Hughes said that as the working of metals appeared to effect their conductivity, he was inclined to rely more on the conductivity measurements of the balance than on those of the Wheatstone bridge. By the balance plain pieces of metal were taken, whereas by the bridge wires were mostly taken. He would rather not give any theories yet as to the results obtained from the balance.

Dr. Erck then exhibited his novel pump for lifting solutions out of batteries. It consists of a closed vessel, funnel-like, the stem dipping into mercury, a column of which ascends the latter to a certain height. Two tubes emerge from the cover, one dipping into the liquid, the other opening to the air. By altering the pressure inside the vessel the solution rises to the latter, and can escape from it by trickling through the mercury.

NOTES.

Biology.

Some interesting results on the hereditary transmission of artificial injuries have been obtained by M. Brown-Séquard. He concludes that the young of parents abnormally constituted inherit external lesions, but not the central anomaly which determines such lesions.

M. Geoffroy has recently discussed the views of Magnus on the evolution of the colour-sense. He agrees substantially with Mr. A. R. Wallace and Mr. Grant Allen that it was not the perception of colours which was wanting in Homer's days, but merely a precise nomenclature for shades.

M. Zolyet has laid before the Society of Natural Sciences, at Bordeaux, some researches on the respiration of fishes. He finds that of all animals their respiration is the least active; they never, however, absorb more carbonic acid than they give off oxygen. Between the temperatures of 2° and 30°, which may be taken as the extreme limits of the surrounding medium compatible with life, the amount of oxygen varies from 1 to 10.

The action of the sulphide of carbon upon the vine has been examined by M. Boiteau. He finds that all doses destructive to the *Phylloxera*, from 6 or 7 grms. up to 10 grms. per hole,—*i.e.*, 12 to 24 grms. per square metre,—are injurious to the roots within a certain radius.

M. G. Pouchet states that Averrhoes is the first writer who gives an approximately true account of the sensation caused by the touch of electrical fishes. He compares it to magnetism, whilst Galen and others had considered it analogous to cold.

It is asserted by some bird-fanciers that the feathers of canaries may be turned red by feeding them, before and during the moulting season, with a mixture of hard-boiled egg and bread, dusted over with the best quality of capsicum in fine powder.

The "Revue Industrielle," after stating the conflicting views of Vegetarians and of the friends of a mixed diet, thinks that sufficient attention is not paid in this controversy to differences of race, temperament, climate, and employment.

M. Richet has investigated the effects of heat on the functions of the nerve-centres of the crayfish. Nervous voluntary action is weakened at 23° to 24°, and disappears completely at 26°. Reflex nerve action disappears at 27° to 29°, at 30° the respiratory flabellum ceases to act, and from 32° to 34° the motor nerves become inactive. It is remarked that amongst the Vertebrates

the different functions of the nervous system are affected in a progression very similar to that traced among the Crustaceans.

According to "Le Journal d'Hygiène" the heron has on its breast large greasy tufts, which secrete a whitish unctuous matter of a disgusting odour, but which has a remarkable power of attracting trout, and probably other fishes. M. Noury, on placing the breast of a heron in a net, has invariably found it filled with trout.

With regard to the chemical structure of bones, Dr. Carl Aeby points out that the hygroscopic properties and the relations of density and volume, both among the different bones of a skeleton and among the strata of one and the same bone, show greater discrepancies among themselves than is often the case with the bones of entire genera. He considers these differences as points of departure for further research, the significance of which extends far beyond the sphere of physiology.

The larva of a Dipterous insect, probably belonging to the genus *Syrphus*, has been observed to devour the *Phylloxera* with great eagerness.

M. Ch. Chamberland shows that the germs or spores of *Bacillus subtilis* in neutral liquids resist a temperature of 100° C. for several hours, and multiply rapidly at 50°.

The idea prevails in various parts of France that it is impracticable to strip the bark of trees if a flock of sheep is passing to the windward.

According to M. J. G. Lemmon the age of the *Sequoias* of California has been greatly over-rated, none of them, according to the number of their annual rings, exceeding fifteen hundred years.

Prof. Gunning declares that recent experiments have much shaken his faith in the supposed destructive action of alcohol upon low forms of organic life. There is further no *à priori* reason to be urged in favour of the assumption that alcohol is fatal to bacteria and their germs. Its dehydrating power cannot have such an effect, since such low organisms are merely thrown into a latent condition when temporarily deprived of water.

M. Coursserrant, in a communication made to the Academy of Sciences, recommends the systematic use of coloured glasses in cases of colour-blindness. In experiments which he has undertaken he finds that on looking through a green glass at a sheet of white paper, upon which was pasted a narrow stripe of cherry-red paper, the latter not merely appeared more brilliant, but a rose tint was spread over the whole white surface. According to certain experiments made upon himself, the luminous sensation is still further heightened if a feeble constant current —say of 2 or 3 Gaiffe's elements—is passed through the retina at the moment of the production of the phenomenon.

The Report of the Inspector under the Vivisection Act for the year 1878 has made its appearance. The greatest number of licenses in force at any one time during the year was 45: of these 18 remained a dead letter. Under the remaining 27 about 481 experiments were performed, of which not more than 40 involved the infliction of serious pain. In Ireland 10 licenses have been issued, only 5 of which have been acted on, and no pain has been inflicted even in one of the 24 experiments performed. These statistics are surely enough to show the utterly farcical character of the anti-vivisection agitation which gave birth to the Act in question. Even if vivisection were a demonstrated evil, biologists might have been protected by the maxim "De minimis non curat lex."

Writing on the physical conditions of consciousness, Prof. A. Herzen says that a psychical act considered objectively is a molecular movement, *sui generis*, which an external impression conveyed from the afferent nerves to a reflex sensation induces in the central nervous elements; it is not yet psychic until the vibrations have reached a cellule of the grey substance, and it is no longer psychical from the moment when the vibrations cease or leave the central cell to communicate themselves to the afferent nerves to be expended in the form of muscular motion. The phenomenon, taken as a whole, presents two phases: in the first there is decomposition of the substance of the nervous elements, and a release of the latent energy which has been shut up; in the second there is a re-composition of their substance, and a storing up of latent energy, destined to serve for future expenditure. The author calls the first phase nervopsychic disintegration, and the second nervopsychic redintegration. He then proposes the following physical law of consciousness:—Consciousness never accompanies the integration or redintegration of the nervous elements; it accompanies merely their functional disintegration. Its intensity is simultaneously in direct proportion to the intensity of the disintegration, and in inverse proportion to the ease and the rapidity with which the internal work of each nervous element is discharged upon another sensitive or motor element, whether central or peripheric.

M. E. Heckel has presented to the Academy of Sciences an account of the action of the salts of strychnin upon the Gasteropod mollusks. Noting the fact that the alkaloids occur more frequently and possess a more powerful physiological activity the higher we ascend in the scale of vegetable life, he asks whether their special task may not be to defend the plant against animal enemies, especially as the more important an organ the more generally it is possessed of poisonous properties. He therefore finds it important to ascertain the action of the best known alkaloids upon certain selected terms in the animal series. For this purpose he has experimented with strychnin sulphate and oxalate upon *Helix pomatia* and *aspersa*, and *Zonites algirus*.

He observed that whilst *Helix aspersa*, of the mean weight of 6 to 6·70 grms., succumbed to a dose of strychnin sulphate or 0·025 grm., *Zonites algirus* of 8 grms. and *Helix pomatia* of 9·70 grms. perfectly resisted doses of 0·045 grm. He concludes that the Gasteropods enjoy a remarkable immunity from the effects of strychnin: that with them, as in case of the Vertebrata, the degree of hurtfulness of the poison is inversely as the weight of the animal, and that upon them as upon higher animals strychnin is tetanising.

Chemistry and Technology.

A writer in "Reimann's Färber Zeitung" points out that tartar emetic, as used in cotton-dyeing, serves not to fix the aniline colours themselves, but merely to fasten the tannin, thus playing the part of an indirect mordant. Several experiments have been instituted at the Färber Akademie in order to ascertain whether this application of antimony can be pronounced injurious to public health. It was found that water, in which cotton yarns dyed with aniline colours on a mordant of tannin and tartar emetic had been steeped, or especially boiled, gave distinct indications of antimony when tested in ordinary manners. The quantity of the metallic compound fixed on the fibre seems, indeed, far too trifling to have any effect upon human health. Still, in view of the panic which has seized upon the public mind, and of the tendency of the literary and political press to attribute all mysterious attacks of sickness to the influence of poisonous dyes, &c., Dr. Reimann—in our opinion very judiciously—advises dyers to dispense with the use of antimony, which will doubtless be found by no means necessary.

The effects of inhaling oil of turpentine have been described to the Academy of Sciences by M. Poincaré. He has examined 282 workmen who use this oil in their trades, and has kept animals for several months in a medium strongly charged with its vapour. Among the workmen the symptoms were headache, dizziness, watery eyes, weakness of sight, especially in artificial light, cough, and troubled digestion. In most cases the constitution became habituated to this agent, but sometimes a change of employment was necessary. The animals experimented upon remained in a normal condition if good ventilation was maintained. In confined air death ensued in consequence of congestion of the brain, lungs, &c.

Physics.

Dr. Henry Draper, the eminent American physicist who is now on a visit to this country, has addressed recent meetings of the Astronomical and Physical Societies on his alleged discovery of oxygen in the sun by bright lines in the solar spectrum. He said that hitherto he had not been able to find these lines projecting from the limb of the sun like hydrogen, and his impression

is that oxygen resides lower than the reversing layer. He had lately been extending the dispersion of the spectrum of terrestrial oxygen, and from a light of maximum intensity of one-candle power had now got a dispersion of 80 inches from A to O. He exhibited two of the original negatives of the solar spectrum, showing the bright lines. "The Times" gives some interesting particulars respecting the time and labour involved in this research. The bobbin of the Gramme machine revolves once for each spark used in obtaining the photographs of the air spectrum. Each photograph requires 30,000 sparks, and the photographs obtained in the last three years have required no less than 20,000,000 sparks, so that the bobbin of the Gramme machine has revolved 20,000,000 times. Although the petroleum engine used for driving the machine consumes only a couple of drops of oil at each stroke, 150 gallons have been used up in three years.

At the recent Soirée of the Royal Society several novelties of great scientific interest were exhibited. In the first room were exhibited Mr. Crookes's Exhausted Tubes and Apparatus, illustrating various phenomena connected with Molecular Physics in High Vacua. A description of these experiments will be found in the "Monthly Journal of Science" for June. In another room Prof. Guthrie, F.R.S., exhibited Broken Glass in Frames, illustrating the Fracture of Colloids. The working of the Writing Telegraph was shown by Mr. E. A. Cowper. The object attained by this instrument is that it enables the operator to write at a distant station many miles away, just as though he were present there himself, without requiring the use of any special signals, codes, or signs (to spell each letter as is now the practice), and without the assistance of any person to translate the signals as received. The instrument acts upon the simple principle of communicating at all times to the writing pen at the receiving end of the line, the exact position of the pencil of the operator at the sending station through two line wires, or, so to speak, giving the latitude and longitude of the pencil continually, the position of the pencil vertically being communicated by one wire, and the position horizontally being communicated by the other wire. The pencil of the operator has two light "contact rods" jointed to it, and one of these slides over the edges of a series of "contact plates," having various resistances interposed between them and the line wire, while the other rod slides over a second set of such plates connected to the other line wire; at the receiving end of the line each of these wires actuates its own needle. The two needles (which are placed at right angles to each other, and are provided with light springs) actuate one writing pen, so that the pen moves up or down, and backwards or forwards, in exact obedience to the motions of the pencil in the hand of the operator at the distant station. Both the paper written upon in pencil by the operator at the sending station and that written upon in ink by him at the receiving station move

along as the writing proceeds, and the messages have only to be cut off from time to time, wound round a piece of card, and sent out to their destination, or put into an envelope and dispatched. Edison's Loud-speaking Telephone was exhibited by Mr. Arnold White and Mr. C. P. Edison. This instrument far surpasses every other form of telephone at present invented. It is thus described by Mr. Conrad Cooke. A cylinder composed of precipitated chalk, to which a small proportion of acetate of mercury is added, the whole being moistened into a saturated solution of caustic potash and moulded into a cylindrical form by being subjected to hydraulic pressure, is mounted upon a horizontal axis, and is made by suitable mechanism to revolve beneath a metallic strip, which is maintained with a uniform pressure against the surface of the chalk. At the point where the strip rests upon the cylinder a small plate of platinum is fastened, and the opposite end of the strip is attached to the centre of a diaphragm of mica, four inches in diameter, firmly fixed to the framing of the instrument by its circumference. By connecting the strip to the zinc element of a voltaic battery, and the chalk cylinder to the copper pole, and rotating the cylinder at a uniform speed away from the diaphragm, it will be found that, when no current is passing, the friction between the moistened surface of the chalk and the platinum strip is sufficient to drag the centre of the diaphragm inwards, and it will take up a fixed position of equilibrium when the frictional pull in the centre of the diaphragm is equal to the elastic tension of the strained diaphragm. The moment, however, that an electric current is allowed to pass between the strip and the cylinder, electro-chemical decomposition takes place, the friction between them is reduced, and the diaphragm, finding its elastic tension unopposed, flies back to a second position of equilibrium dependent upon similar conditions ; and, if a variable or undulatory current of electricity be transmitted through the instrument, the diaphragm will be kept in continual motion by the constantly varying friction existing between the chalk and the platinum dragging the diaphragm in opposition to its own constant elastic tension. So marvellously sensitive is this simple mechanical arrangement to the smallest as well as to the most rapid and complicated variations in electrical intensity taking place in the transmitted current, that all the complex sonorous undulations propagated by human articulation instantly produce their corresponding variations of frictional resistance, and the diaphragm reproduces, in a loud voice, the words which are being uttered into the telephone at the distant station. The cause of the superiority in power of this instrument over others must be looked for in the fact that the vibration of the diaphragm is produced, not as in other telephones by an electric current, but by local mechanical means, that is to say, by the rotation of the cylinder by hand, by clock-work, or by any other motive power. The function of the electric current is purely a controlling and regulating one ; it determines how much or how

little of the force exerted on the spindle is to be communicated to the diaphragm, and the time when that force is to be brought into play, and it may, mechanically speaking, be compared to a frictional clutch, coupling a machine to a steam-engine, and which, at any moment, may be made to transmit the full power of the motor to the machine, or, by reducing the friction, to transmit only a portion of that power.

The Parliamentary Committee on Electric Lighting have concluded their labours and issued their report. We shall refer to the evidence in a future number of this journal. In their report the Committee state that the general nature of the electric light has been well explained in the evidence of Prof. Tyndall, Sir William Thomson, Dr. Siemens, Dr. Hopkinson, and others. It is an evolution of scientific discovery which has been in active progress during the whole of this century. Essentially the electric light is produced by the transformation of energy either through chemical or mechanical means. The energy may be derived from a natural force, as, for instance, a waterfall, or through combustion of a material in the cells of a voltaic battery, or of fuel in a furnace. The energy being converted into an electric current, may be used to manifest electric light by passing between carbon points, or by rendering incandescent solid bodies, such as iridium. A remarkable feature of the electric light is, that it produces a transformation of energy in a singularly complete manner. Thus the energy of one-horse power may be converted into gaslight, and yields a luminosity equal to 12-candle power. But the same amount of energy transformed into electric light produces 1600-candle power. It is not therefore surprising that while many practical witnesses see serious difficulties in the speedy adaptation of the electric light to useful purposes of illumination, the scientific witnesses see in this economy of force the means of great industrial development, and believe that in the future it is destined to take a leading part in public and private illumination. There is one point on which all witnesses concurred, that its use would produce little of that vitiated air which is largely formed by the products of combustion of ordinary illuminants. Scientific witnesses also considered that in the future the electric current might be extensively used to transmit power as well as light to considerable distances, so that the power applied to mechanical purposes during the day might be made available for light during the night. So far as the practical application of the electric light has already gone there seems to be no reason to doubt that it has established itself for lighthouse illumination, and is fitted to illumine large symmetrical places, such as squares, public halls, railway stations, and workshops. It is used in Paris for lighting shops which require a light by which different colours may be distinguished, and has recently been used in England for the same purpose with satisfactory results.

THE MONTHLY

JOURNAL OF SCIENCE.

AUGUST, 1879.

I. ENGLAND'S INTELLECTUAL POSITION.

The Literary and Philosophical Society of one of the most important towns in England recently memorialised the Privy Council against the recognition of Owens College as a University. The petitioners professed to be alarmed at "the serious risks to which an undue increase in the number of degree-giving universities would expose the whole system of English education."

This plea surely insinuates that the English system of education, if not absolutely perfect, is to be regarded as superior to that of other nations, and that it could scarcely be modified except for the worse. Reflecting on the opinion thus conveyed, it seemed to us not useless to inquire what are the peculiarities of English education as compared with the systems established or aimed at amongst our neighbours and rivals, and what are the fruits of such distinctive peculiarites as manifested by England's share in the advance of culture. The foremost characteristic of English higher education—for that alone can be meant—is its monastic character. In monarchic Germany, as in republican America, a college consists merely of class-rooms, lecture-halls, libraries, laboratories, and other localities needful for research and study. The students live in lodgings in the town according to their tastes and means. The professors reside in private houses, and, so far from being forbidden to engage in any literary work without special permission, the more they create a reputation for themselves by the publication of their observations and discoveries the more honour they are con-

sidered to reflect upon the institution. With us all this is different; in a college of the normal English type the professors and students are obliged to "live in," like the hands of a modern gigantic drapery warehouse. Celibacy is the rule, by way we suppose of inducing men of high reputation to feel contented, and over the whole convent rules a Father Abbot, empowered, of his own motion, to appoint and dismiss officials irrespective of their standing, and to interfere with the details of all departments, however slight may be his qualifications to do so with benefit.

Worse still remains: the most eminent professor, instead of devoting his leisure to research, is expected to undertake the degraded and degrading task of maintaining "good order and discipline among the students." In other words, he is called upon, figuratively speaking, to assume the cocked hat and red waistcoat of Bumble! Those who are so clamorous for the maintenance of "discipline" forget that the necessity is entirely of their own creation. Enforce "residence;" shut up a number of young men together in flat violation of the order of Nature; compel the thoughtful and the studious to associate, will they nil they, with the idle, the reckless, and the profligate; give the bully a free choice of victims—and the duties of the beadle must devolve upon some one. But disperse students through the town or neighbourhood, and they will no more require any special officers for the enforcement of "good order" than would an equal number of bank clerks or young men engaged in the Civil Service.

Such, then, is the foremost peculiarity of the "system of English education"—a distinct "survival" from the days of ignorance which surely deserves exposure to something more than a mere "serious risk" of overthrow.

The second characteristic of our English colleges, as compared with their foreign rivals, is more deeply seated, and will prove much less easy to reform. It lies in the very character of the teaching given. The German professor, both by precept and example, seeks to qualify his students to add something to the existing total of human knowledge. He trains them for research, for discovery; he judges of their proficiency by the power they display of dealing with unsolved questions,—with those problems in which every branch of Science abounds. He makes them the partakers of his own investigations. The highest honours are earned *rebus gestis*.

According to our English system all this is well-nigh reversed. Men study, as Prof. Huxley well expressed it,

not to "know" but to "pass." He who can make himself most fully acquainted with the works of others bears the bell, whether he is capable of conceiving an original thought and of carrying out a train of research or not. We are consequently rich in "all-round" men, who can talk with considerable fluency on every known Science, but who are neither able nor willing to extend the boundaries of any.

If we point out this sad defect in the working of our educational system we are often told, by way of excuse or justification, that English Universities are not investigational but "tuitional,"—mistuitional rather,—and that if we want seats of research we may found and endow them ourselves. But suppose we act on this kind permission, and that we attempt to secure for the institutions we have created the power of testing and recognising merit, then there are found men not ashamed to petition Government to withhold from us the power of granting degrees. We have sometimes advocated the endowment of research. Alas! we should feel but too happy if no positive hindrances were placed in its way—if there were within the four seas of Britain even one college where original work was the sole passport to distinction.

Let us not, however, condemn this wonderful "English system of education" on mere theoretical grounds, but let us examine its practical working. Are we holding our own in comparison with rival nations? Are we giving or mainly receiving light? The answer is not hard to find. Look, for instance, at chemistry. According to a summary compiled by Prof. Frankland, of 1273 memoirs embodying the results of original research, published in 1866, the United Kingdom contributed only 127, or about one-tenth; Germany producing 777, or more than half of the grand total. Worse still remains: of the papers with which Britain is here credited no inconsiderable portion is due to Germans resident in this country. We have no reason to believe that in the dozen years that have sped since this return was made the relative position of Great Britain has much improved.

But let us turn to another sphere in which we have far greater opportunities than any other nation—perhaps than all other nations combined. We refer to the exploration—geological, zoological, and botanical—of the less-known regions of the globe. It is self-evident that, with our vast colonial Empire and our ubiquitous commerce, such countries ought to be first and foremost examined by Englishmen,—that their description should be given through English pub-

lications,—and that their productions should be displayed in our museums and botanical gardens. Have we, then, even in this department, such a vast superiority as might reasonably be expected? Without the slightest wish to overlook the splendid services rendered to Science by Darwin, Wallace, Bates, and Belt, we fear we cannot reply in the affirmative. Let us take the case of New Guinea, an island of whose magnificent fauna Mr. Wallace gave us, sixteen years ago, so tempting a vision. Let us consider that it is merely separated by an arm of the sea from important thriving and energetic provinces of the British Empire, and we should naturally expect that English speaking travellers would take up and complete the task. Not so; Dr. Maclay, the Russian, and Prof. D'Albertus, the Italian, have stepped in before us. Turn even to the Fiji Islands; we have annexed them, but their thorough scientific examination has been allowed to fall into the hands of foreigners, who make better use of their scanty facilities than we do of our incomparable opportunities. In Africa the case is but too similar. Travellers from Germany,—we need only mention Dr. Schweinfurth,—from Belgium, France, Italy, Portugal, are at work in all directions, and are gathering rich spoils of discovery. We are, meantime, piously hoping to do a large business in the interior when the Zulus are disposed of. Let us beware lest we are merely conquering a market for others.

There are yet other tests to which our educational system can be submitted. Do our colleges attract students from foreign countries? With the exception, mainly, of a few Japanese our seats of learning are not frequented by strangers. Numbers of American young men go abroad to complete their studies; but they repair not to England, but to Germany. The same rule holds good with various other nations. What they require is not to be found amongst us.

Again, it may be asked whether we send out professors of the sciences to foreign nations, civilised or semi-barbarous? Are the universities, the libraries, the museums of the world to any marked extent in the hands of Englishmen? He who cannot at once answer this question with a decided negative must have spent his days in dream-land. We believe that over the whole European continent, as well as in the United States, there is not a single professorial chair occupied by an Englishman. Such posts, if not filled by natives of their respective countries, seem to fall, as a matter of course, to Germans. Science in Russia, for instance,

has undergone a development which thirty years ago would have been pronounced simply impossible. But it is purely of German creation. Germans have trained and fostered speculative research in the Universities of Kazan and Perm, just as they have guided applied science in the print-works of Moscow and the foundries of Tula. So successfully has this been effected that Kazan and Perm contribute more original investigations than any British University, and that at the late Paris Exhibition Moscow outstripped Manchester and Glasgow in the purity and brightness of its alizarin-red grounds.

Turning from Russia to a country of far older civilisation, where intellectual pursuits of all kinds have undergone a striking revival, we find professorships at Turin and Rome held by distinguished Germans. In the United States, though there exists a great and increasing body of scientific men of American origin, Germany is playing her part of general instruction. Even in the quasi-civilised States of South America the same facts are to be recognised. Whatever scientific activity exists is due to Germans, and is placed under their guidance.

We see thus that over a very great part of the civilised world there is a double movement in progress; students are flocking from all ends of the earth to German seats of learning, and professors are going forth to all ends of the earth as apostles of culture. On the contrary, England is the focus of no such centrifugal and centripetal movement. As a natural consequence German literature, German ideas, German habits of thought are spreading everywhere. Nor is this, as might perhaps seem at first sight, a matter of barren honour. The German professor—settled, say, in South America or in Japan—naturally uses his influence on behalf of the German manufacturer and the German merchant. If a new branch of industry is to be introduced German engineers, or chemists, and German workmen are sent for, as is now the case with a woollen manufactory in China, which is being established on European principles. Nay, it may even be that our political weight is suffering from what foreigners cannot but regard as our inferiority and ignorance.

Such, then, is the state of affairs outside the boundaries of the British Empire. How is it within? To a very great extent, both in the home kingdoms and the colonies, we find ourselves compelled to import that intellectual eminence which we refuse to cultivate in our midst. Foreigners oc-

cupy professorial chairs in our colleges; they fill the posts of botanists and geologists to our colonial governments; they hold high positions in the respective staffs of the British Museum, of the Geological Survey of India, and of our exploring expeditions. If we pass to the regions of Applied Science we find aliens in increasing numbers managing our most important manufacturing establishments. In the year 1858 there was one foreign chemist employed in the great alkali district of south-western Lancashire. How many are there now? Nay, Dr. Reimann does not scruple to boast, in the "Faerber-Zeitung," that English manufacturers owe whatever eminence they may claim not to Englishmen, but to Germans. Making all due allowance for exaggeration, there is too much reason to own that we as a nation are sinking into the position of mere "hewers of wood and drawers of water," whilst every function requiring disciplined intelligence—save and except the miserable task of speech-making—is passing into the hands of strangers.

Now we submit that it is not in virtue of any inbred deficiency if the countrymen of Newton, of Faraday, and of a few living biologists and physicists, are outstripped in the pursuits of Science by any nation upon earth. If we fall short, as the picture we have just hastily sketched so plainly shows, it is due to that deplorable "English system of education" to which we cling with an attachment so fanatical. Are we still to go on passing examinations whilst other nations are effecting organic syntheses? Are we to accept our senior wranglers and double first-men as an equivalent for their discoverers and inventors? If so, all other reforms and "movements" notwithstanding, our abdication of the rank we once held is sealed and signed.

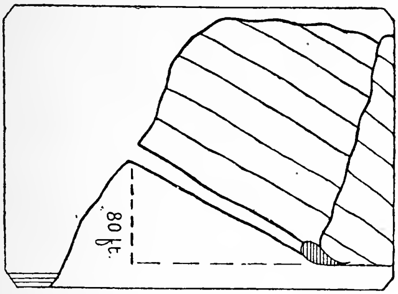
II. PARADOXICAL PHENOMENA IN ICE-CAVES.*

By N. M. Lowe.

In the "Scientific American" for March 29th last there appeared a letter from Mansfield, Ohio, inquiring as to the cause of the phenomena in an ice cave which is to be found in Decorah, Iowa, and for which there appears

* Read before the Boston Scientific Society, April 9, 1879.

to have been as yet no cause assigned. A description of this cave is given in the same letter, of which description, so nearly as is possible, the accompanying illustration is a fair representation, as regards the main features of the case.



There may be a few differences as regards the details of the cave, but so nearly as can be judged from the written description the drawing presents the elements necessary to the peculiarities of the cave. In the figure the cave will be seen represented as at the bottom of an inclined passage, the inclination being that noted in the description, and the dimensions and other particulars being as nearly as possible to the proper scale. The crevice mentioned in the description may be imagined as a fault, which extends from the top of the cave to the top of the bluff, through which crevice mingled air and water finds its way to the cave.

In regard to the mingling of air with a stream of descending water, a quotation from the pamphlet of Mr. Frizzel, on the subject of the compression of air by such streams, would not be entirely out of order. On this subject he says—" It is a matter of common observation that bubbles of air rise in still water with a very moderate velocity. The velocity depends somewhat on the size of the bubbles. Bubbles, such as issue from an orifice one-eighth or one-tenth of an inch in diameter, rise from a depth of 50 feet in about fifty seconds, moving rather less than 1 foot per second near the bottom, and rather more than that near the surface. It is plain that a bubble of air drawn into water that has a downward motion of more than 1 foot per second will be carried down and subjected, in its descent, to a continually increasing pressure."

Considering, then, the description and the facts above quoted, it would not be unfair to assume that there would be a possible compression of air contained in the water, on its liberation in the cave, of about 80 lbs. to the square inch.

This assumption is supported by the fact that, from the description, the mouth of the cave would be at least 80 feet above the level of the river, and it may be inferred that as no special mention is made of the position of the entrance, save that it is in the side of a bluff, the hill may be considered as extending above the mouth of the cave to at least the distance of the latter from the river.

The phenomenon, then, of ice being found there in the summer can be referred, I think, to the theory of the liberation of compressed air brought down from a considerable height by a stream of water falling or flowing through a natural conduit or fissure in the rock, embodying the principle of the ancient and well-known *tromp* used in the Catiline forge, and still in use in Corsica, Sardinia, Savoy, and many other places.

It is only necessary to imagine such imperfection in the conduit or fissure at the initial point, which is supposed to be on the top of the bluff, or far up the mountain's side, as would admit air to come in contact with the water after it had attained a velocity of more than 1 foot a second. When the air has reached the bottom, and is liberated in the cave, it will be from a pressure equal to the height of the column of water, and it will have lost by *convection*, in the mass through which the conduit passes, the heat *due to its compression;* and on being liberated it will immediately absorb, from the air and the water in the cave, the heat which it has lost in its downward passage.

"The most remarkable fact," that the cave freezes only in summer, and as the cold of actual winter comes on the ice in the cave gradually melts and disappears, is caused, I will venture to state as an opinion, by the gradual freezing of the surface at the top of the bluff, or the source of the air, to a considerable depth, thus sealing up the aperture through which the air entered the conduit.

Sir Roderick Murchison described a similar ice cave at Lletski, Russia, but gave no explanation as to the phenomena.

Ice wells are to be found at the foot of Mount Mansfield, in Vermont, and are really incipient caves, without depth enough to be clear of ice in winter, from the fact that the external winter temperature reaches the bottom or source of the summer ice.—*Science Observer.*

III. PAIN AND THE WEATHER.

IN his Paper on the "Relation of Neuralgic Pain to Storms and the Earth's Magnetism," read before the National Academy of Sciences, Prof. S. Weir Mitchell reported the following observations :—

Capt. Catlin, U.S. Army, lost a leg during the war, and since that time has suffered from traumatic neuralgia, sometimes in the heel, but more frequently in the toes, of the foot. He has carefully noted the effects produced on himself by changes of the weather. Dr. Mitchell's own studies in this case, as he says, "would never have proved successful had it not been for the unusual ability, interest in the task, and perseverance of the accomplished gentleman who has obliged me by making his own torments useful in the solution of the question of how far weather effects the production of certain kinds of pain." The hourly observations cover a period of five years. For the first quarters of these five years there were 2471 hours of pain ; for the second quarters, 2102 hours ; for the third quarters, 2056 hours ; and for the last quarters, 2221 hours. The best yield of pain is in January, February, and March, and the poorest in the third quarters—July, August, and September. During these five years, while the sun was south of the equator, there were 4692 hours of pain, against 4158 hours while it was north of the equator ; and the greatest amount of pain was in the quarters beginning with the winter solstice, and the least was in those beginning with the summer solstice. The average duration of the attacks for the first quarters was 22 hours, and for the third quarters only 17·9 hours.

By taking the four years ending January 1, 1879, it is found that of the 537 storms charted by the Signal Bureau, 298 belong to the two winter quarters, against 239 for the summer quarters. Hence we have the ratio of the number of storms of the winter quarters and summer quarters corresponding to the ratio of the amounts of neuralgia for these respective periods, and the ratio of average duration of each attack for the same time corresponds closely with the ratio of the respective total amounts of neuralgia for the same periods. The average distance of the storm centre at the beginning of the neuralgia attacks was 680 miles. Storms coming from the Pacific Coast are felt farthest off, "very soon after, or as they are crossing the Rocky Mountains," while storms along the Atlantic Coast are associated

with milder forms of neuralgia, and are not felt until the storm centre is nearer. Rain is not essential in the production of neuralgia.

It was found that the severest neuralgic attacks of the year were those accompanying the first snows of November and December. One of the most interesting and valuable results of this series of observations is thus stated :—"Every storm, as it sweeps across the continent, consists of a vast rain area, at the centre of which is a moving space of greatest barometric depression, known as the storm centre, along which the storm moves like a bead on a thread. The rain usually precedes this by 550 to 600 miles, but before and around the rain lies a belt which may be called the neuralgic margin of the storm, and which precedes the rain about 150 miles. This fact is very deceptive, because the sufferer may be on the far edge of the storm basin of barometric depression, and see nothing of the rain, yet have pain due to the storm.—*Kansas City Review of Science and Industry.*

IV. A METHOD OF PREVENTING THE TOO RAPID COMBUSTION OF THE CARBONS IN THE ELECTRIC LAMP.

By H. W. Wiley.

In using the electric light for projections two chief points are to be considered, viz., 1st, brilliancy of illumination, and 2nd, steadiness of the light. When the source of electricity is sufficient the first of these ends is easily obtained. The second, however, is not so easy of accomplishment. The chief difficulty in the way of securing steadiness is found in the carbons themselves. Some carbons, and I find these to be the most common, burn away so rapidly that, where no mechanism is present to produce alternating currents, the electric arc is constantly passing out of the focus. Often, too, I have found that when the current is quite strong with the softer carbons, the arc would extend itself momentarily between points as far as a

centimetre from the end of the carbons. At other times it would revolve about the electrodes something like a spiral flame in a pyrotechnic display. This leaping and dancing of the arc is, of course, fatal to its employment for projection.

In order, if possible to remedy these defects in a lantern which I have in almost daily use, I made the following experiments:—I first took the specific gravity of three specimens of carbon, obtained from different dealers, one in France and two in America. The specific gravity of the French carbon was 1·85; of No. 1, American, 1·53; of No. 2, American, 1·55. The French carbon is hard, of a grayish black colour. The American carbon is soft, easily broken up, and has no sign of a metallic lustre. The light from the French carbon is quite steady, and displays very little of that tendency to flicker so troublesome in the American varieties.

A positive French carbon, which had been used for several hours, until consumed nearly to the lamp, burned away at the point, but otherwise retained its original shape. This carbon was used without any previous preparation.

A soft carbon, however, of the same size as the preceding, became red-hot to a distance of 4 to 6 centimetres from the end, and rapidly wasted away; after being in use for half an hour it was reduced to a slender, tapering form.

I first tried the plan so well known in France, but so seldom tried here, of coating the carbons with a film of copper. The precipitation of the copper should take place slowly, and with a current so regulated in quantity and intensity as to produce no spongy deposits. When the soft carbons were thus prepared they worked beautifully for a short time. The light was brilliant and steady, while any green tint imparted to it by the volatilised copper produced no effect whatever prejudicial to the purpose in view. But as the carbons, little by little, became heated, the copper film oxidised, and after half an hour the carbon was again reduced to the slender form above described.

I next tried the expedient of setting a copper wire, 0·4 m.m. in diameter, into the centre of the carbons. With a thin saw I cut a longitudinal groove to the centre of the carbon, and after inserting the wire fixed it in place by filling the groove with plaster. The upper end of the wire was left projecting so that it could be brought into actual contact with the clamp. I hoped from this contrivance to hold the origin of the arc steadily at the end of the carbon, and at the same time, by increasing the conducting power, to pre-

vent the heating and consequent rapid consumption of the electrode. In placing the carbons in the lamp the grooved sides were turned from the lens. This device proved very successful in securing a steady light, but the carbons were heated to their points of insertion in the holders, and wasted rapidly away. The point, it is true, remained blunt, but the stem of the carbon burned away so rapidly that the experiment must be regarded as unsuccessful.

It was evident from my first experiments with copper-plated carbons, that, if any way could be devised of protecting the copper from oxidation, the copper would prevent the carbon from heating by increasing its conductivity and diminishing the resistance. I accordingly covered the carbons, after copper-plating, with a film of plaster of Paris, about 1 millimetre in thickness. After this had set, the edge of the carbon, which was to be turned towards the condenser, was carefully denuded of its plaster covering, which was also cut away until quite thin on the adjacent surfaces. These precautions were taken so that the plaster might not interfere with the radiation of light in the direction of the condenser. Before use the plaster must be thoroughly and slowly dried. The copper surface at the end of the carbon being uncovered, it is fastened in the holder in the usual way. The carbons prepared in the way described left nothing to be desired. The light was steady, and the carbons burned slowly away. The film of plaster melted gradually as the point was consumed, and thus never interfered with the illumination. The points of both positive and negative carbons remained blunt, and there was no wasting away of the stem. A carbon prepared in this way will last at least ten times as long as one used in the ordinary way. But the chief advantage is found in the comparative steadiness of the light thus secured.

Carbons of the above description work best when well plated. The following numbers give what I regard as a minimum amount of copper to secure satisfactory results. In all the experiments I have tried the carbon has been of the soft American variety, with an average specific gravity of 1·55.

Length of carbon	17·5 c.m.
Each side	1 ,,
Number $\overline{\text{c.m.}}^2$ of surface, including ends	72 $\overline{\text{c.m.}}^2$
Weight before coppering	21·1615 g.
,, after coppering	24·0410 ,,
,, of copper	2·8795 ,,
,, ,, to each $\overline{\text{c.m.}}$	0·0397 ,,

In order to dispense with the use of a reflector I arrange the carbons + above, as described in the "Journal of the Franklin Institute" for May and June, 1878.

The peculiar cup-shaped appearance of the positive carbon helps to concentrate the light on the condenser. It is understood that any lamp in which the carbons are arranged end to end can be used with electrodes prepared as above. Such a lamp can be easily substituted in a lantern made for use with oxy-hydro-lime light. I use constantly such a lamp with one of Edgerton's physical lanterns originally made for the lime light. I am inclined to think a kaolin paste would be better than plaster for coating the carbons. The electric force used in all the experiments has been furnished by the Gramme machine, described in the "Journal of the Franklin Institute" already cited.

The use of projections for illustration in lectures on Chemistry and Physics has become so general that I hope the suggestions in this Paper may prove of some benefit.—*American Journal of Science and Arts.*

V. SEED-BREEDING.

By E. Lewis Sturtevant, M.D., So. Framingham, Mass.

We sow wheat in order to reap wheat, and rye in order to reap rye, and corn in order to harvest corn ; and were the wheat to turn out rye, and the corn-field to yield barley, we should suffer even a greater bewilderment than did the heroes of ancient fable when they became intimate with those enchanters and sorcerers who caused fishes to indulge in conversation and mankind to become changed into beast-like forms. For the world is so subject to law that its recognition on a large scale is part of each man's daily experience ; and living amid the triumphs of modern science, even the every-day labourer is educated unconsciously within the sequences of a more and more correct

philosophy, so that in general terms he recognises that as he sows he must reap, and that thorns do not bear figs, nor thistles grapes. In this aspect of the case it would seem to be unreasonable to talk about heredity in plants, because the statement of the fact in general is uncalled for, being universally known and universally recognised as known. Yet we purpose to give a few thoughts on the subject, despite the apparent clearness of the claim that seeds have a heredity inherent in their nature and structure, because we recognise that the *application* of the well-known fact of heredity in these cases has not received a proper attention from the public, and that while the fact may be well apprehended, yet too few are willing to carry reasoning based on the correct premise to a logical and practical conclusion.

We certainly have no reason to believe that there is one law for vegetable and another for animal life; rather, we have firm grounds for asserting that all life is subject to the same laws of Nature, and that apparent differences are to be ascribed but to the individual under observation, being subject to forces under a greater or less antagonism, or from the simplicity or complexity of its structure and functions which render it amenable to one law rather than to another. Thus, to illustrate our proposition, we can say that a plant and an animal are subject to the action of the great natural forces which affect life and its development, such as gravity, heat, light, &c.; yet the application of a stimulus which will produce no apparent movement in the one may excite muscular action in the other; or, to make a more mechanical suggestion, a falling stream of water may serve to move a saw which shall convert a log into boards, or shall act on a more complicated series of machinery to turn a lathe which shall convert the same log into spools, or the irregular gun-stock. In either case the same moving force has acted, but the work accomplished has been modified into different results through the action of another force, the mind of man, which has intervened to produce oppositions, and through the reaction of these varied oppositions has contributed to the concrete effect of the force derived from gravity upon the shaping of the log. Thus, in Nature, we have the *life*, distinguished from mineral matter by the possession of motion, and the power of adding to itself, through the force which this motion implies, from the surrounding world, of matter suited to its own structure and function, and imparting to it of its motion. Once stop this motion, destroy this mysterious force, and the life becomes subject to other conditions, and dissolves its structure, the function being

lost as well, into the matter which is incapable of acting on itself to produce this added power which we call life and organism. We, however, may say that mineral is one phase of matter which is subject to one set of laws and conditions, and that life is this same matter, with added force which renders it subject to an additional set of laws and forces, which are more or less greater or less powerful, according to the complexity of the life upon which they can act. The true view of Nature is, then, according to our view, that laws are continually in action and are universal, and that it is the province of the existent life or matter to use these laws according as the structure and function affect their relations; and that hence the mind of man, which recognises and adapts existing relations to his own benefit, by modifying through law, structure, and function, can arrange relations to other laws, and produce diversity of result.

The law of inertia, which is defined as being that condition of matter by which its existing state is retained, if at rest, rest, if in motion, the continuance of that motion until other forces in action shall produce a change, serves as well for the consideration of the problem of life. A perfect likeness between parent and offspring would presuppose the parents being from all time in a state of *inertia*,—a supposition which nullifies the proposition, as the act of having offspring would be a departure from that state. Yet when we define the science of breeding, and state that "like produces like," we recognise the fallacy of the proposition as applied with exactness to individuals, and modify it immediately by limitations, which if they serve to weaken the statement, yet serve to make it not contradictory to our experience. Rather the law of variation should be the keyword for breeding, although the proposition that "like produces like" as a mental conception is as true and as applicable as is the proposition that *inertia* is a definition of a law of Nature. Like does produce like when every condition is removed which would produce difference, which only can be done as a mental act: a body is considered as continuing in its present state when every condition involving a change is removed by a mental act. Hence the "law of likeness" is of the most importance to that breeder who desires to so modify surroundings as to restrain his animals from being improved in their progeny; and the "law of variation" is deserving of the more attention from that breeder who would desire to improve his animals in their progeny.

We might amplify this proposition to almost any extent

did we deem it desirable to encumber our essay with metaphysics, but we think that the careful reader or hearer will understand us in our statement that progress comes through variation, and not through likeness; that whether we breed the vegetable or the animal, it is the laws of variation which we must study and apply in order to produce progressive results: and that the law of likeness, the "like produces like" of our books, however true as an expression which must be and is followed by numberless limitations, is but of secondary importance to the practical dabbler or expert in the art of breeding, to that much more important law which embraces the variations and fixing of structures and functions which have been obtained. The pendulum swings to the right hand and to the left with a uniform motion. Any pendulum of the same length of rod and same weight of bulb, under given conditions, vibrates through the same arc. This statement may illustrate the breeder's proposition that "like produces like." Yet let any two of us make a pendulum of equal length of rod and weight of bulb at our own homes, and we would find that practically they would not move over the same arc, because we would use rods of different character or diversity, or bulbs of different centres of gravity, or would hang them differently, or would calculate the arc traversed under temperatures which were different. It is only as we understood these causes of variation that it would be possible to produce two pendulums whose oscillations would compare. This may liken the "law of variation" which is the "application law" for breeders, as distinct from the "like produces like," which is a true, yet a "theoretical law," which does not admit of practical application without the use of the "application law" as here defined. Heretofore, the tail has wagged the dog; we do not propose to stop the wag, but to have the dog wag the tail. Unless this point be understood breeding as a science will cease to progress, and, indeed, the triumphs of the art have come from the unconscious application of these facts rather than from the course which theorists have laid down:—"Variation and likeness," not "Likeness and variation." This is the new and correct method for the study of the laws of breeding as practised. Just as cultivated vegetables have in a large degree been removed from the wild vegetation, and their different status been apprehended, must their breeding science leave wild nature, and come to a cultivated application.

These remarks must be understood to have a close and practical bearing on the breeding of seed, for a plant, as

being individually confined to one locality, has a limit placed by its structure and position in respect to its environment which the animal does not have, and is subject to a different order of variation. The animal progeny comes from a distinct act of the father, and the offspring following this act have characteristics determined by this and other limited systems of parentage; but the ovules of the plant receive fertilisation in general from a large number of parentages, and the seed represents, as in animals, both maternal and paternal influences. The litter of a sow are brothers and sisters; the kernels of the corn plant are not necessarily brothers and sisters, but bear the same maternal influences and different paternal influences, and hence have a variation in their growth and seeding which is dependent on a different species of relationship than in the animal. To illustrate these effects we may mention that while in carefully bred cattle the variations in the progeny are seldom, if ever, sufficient to constitute a new variety, yet Colonel Le Conteur relates that in a field of his own wheat, which he considered at least as pure as that of any of his neighbours, Professor La Gasca, found twenty-three sorts; and he goes on to state the variations in the kernels which occupy each head, and which implied such variation in growth as to necessitate the selection by single kernels in order to perpetuate a variety. In our own cornfield, with certainly no other field cultivated with a different variety nearer than a mile, on the same ear corn-grains of different colour and structure occasionally appear, and there is always a certain percentage of ears of ten and twelve rows amongst the eight-rowed normal ears of the variety. We have in our collection ears of corn with kernels of white corn, yellow corn, and sweet corn in juxtaposition, the male element so overpowering the ovule as to give to it this diverse development, and, as we must assume, chemically unlike as well.

We may next remark how much more quickly the influence of moisture, of dryness, and of heat, avail to change the aspect of the growth and the produce of vegetable life over animal life. We do know that the character of soil has an influence on the cattle grazed thereon, but this influence is seldom of a character which admits of ready definition; on the contrary, the size and shape of the plant, and even the nature of the product, may be completely changed. Thus the hemp plant in India furnishes a resin known as churras, which is stimulant and intoxicating, but does not produce it when the plant is grown in a temperate clime. The ragweed of our fields will mature its seeds and

show a perfect growth on a gravel-bank, the plant being but 2 or 3 inches tall; and the same plant in fertile soil will attain the height, accompanied by a corresponding degree of development otherwise, of as many feet.

Thus we might go on and illustrate from facts which come under the observation of every one, the extreme variability of the plant nature, a variability which is to our advantage, as rendering the plant available to man according as art is exercised by him in developing these variations according to the laws of their structure, in the line of his desires. Every plant with which we are acquainted possesses this power of variation in a marked degree, and the amount of this variation can be artificially determined for it through the processes of hybridisation and of culture as applied to the plant which furnishes the seed from which the individual is derived; and it is this fact which gives to man much power. Each seed possesses its own attributes and its own powers, and its own individuality, so that the conditions for germination and growth being uniform for all, a dozen kernels of corn planted will develop a different germinative power, a different rate of growth, and a different amount of product for each.

Thus in an experiment of ours, kernels of corn were selected of the same size and from the same ear, and were planted, on land made fertile in excess, at the same time and the same depth, and on one plot no cultivation at all, but the weeds pulled by hand; on another plot, the same conditions, but the land kept clean by the hoe. It was observed that there was a different rate of germination for each kernel in the hill, and between the kernels of the adjoining hills; there was a difference in the rate of daily growth between the plants from kernels in the same hill, and that on some day the rate of growth would be reversed as between these hills—one day one plant growing in excess of the others, and other days being the most backward, &c. When the crop was harvested, we found the following for the maximum and minimum product from hills of three kernels planted:—

	Plot 1, Unhoed.		Plot 2, Hoed.	
	Corn (in ear). Lbs.	Stover. Lbs.	Corn (in ear). Lbs.	Stover. Lbs.
Greatest yield per hill	3	5½	3½	6¾
Least yield per hill ...	¾	6¾	1¼	5¼
Average	1·8	4½	2·3	5
No. of ears, all qualities:—				
Largest number ...	9 ears		9 ears	
Least number ...	2 ,,		3 ,,	

In experiments with single hills under different treatment, in another plot, the ears varied from three to ten in number, the weight of corn from 1 lb. to $3\frac{1}{4}$ lbs. in the ear, the weight of stover from $1\frac{1}{2}$ to $6\frac{1}{4}$ lbs., and the total weight of hills as harvested from 3 lbs. to $9\frac{1}{2}$ lbs. harvested.

In those plants which have been subjected to culture for the longest time we find variation, as a rule, more marked than in those in a state of nature. It is a curious and valuable reflection that those plants whose origin in their present form antedates the history of civilisation are at the furthest remove from the wild condition, and have become modified to such an extent as to be unable to hold their present forms without the assistance of man. We also find that wherever the part of the plant modified into use admits of a variety of forms as connected with its uses, there such exist. There is, as a rule, but slight variations between the growths of our most ancient cereals, as attention has been paid more to the grain produced than to the plant which produced it: in the cabbage family, however, where the plant rather than the seed is used, we find variation into cauliflowers, broccoli, borecoles, Brussels sprouts, savoys, cabbage, collards, or coleworts, &c., and these varieties all reproduce themselves by seed.

The art of breeding seeds is therefore to produce and select such variations as are found desirable, and then to establish these variations so that they shall be transmissible either in their present or in an improved condition, by seed. The means of breeding is through the act of selection carried on for successive generations under well-considered conditions of environment, by which the heredity of the seed in the desired direction shall be strengthened. This heredity of the seed brings us more closely to our subject.

There are many degrees of heredity, so far as the breeder is concerned. There are some plants in which the tendency of the seedling to grow into a plant but slightly varying from the parent is very strong, and such are usually wild plants which include in their ancestry countless generations of somewhat similar environment, and a uniform natural selection in action which has selected and continuously propagated the type of plant best fitted to exist amidst the dangers and difficulties of the surroundings. With such a plant the first effort of the breeder is to produce a variation, to overcome the tenacity which leads the seed to propagate in the manner of its ancestry. This tenacity is a heredity neither stronger nor weaker than that which is to be found

in plants of a more sportive nature, but is the outcome of a set of conditions which have been of a similar character for a long period. The same kind of heredity, neither necessarily stronger nor weaker, leads the cultivated plant to vary. Hence we may say that fixity or unfixity of type is no proof of heredity, and that it is but in the conditions which have availed in the past that the heredity which produces likeness is to be sought.

This tendency that exists throughout Nature for one life to produce in its offspring the characteristics of its past, as well as the modifications which have availed in its near ancestry, is the law of heredity. It is a true law, which cannot be said to be manifested by exact likeness of an offspring to its progenitor, because we have instances of alternate generation which shows this not to be so; but it is a genealogical expression which is to be interpreted that the life transmits itself in its modifications; and to know what these are, in order to use this law to our advantage, we must have the history of the preceding lives,—that is, a genealogical record. Now, in the case of a seed, we desire to act upon this knowledge of what the law of heredity as applied to breeding is, and not saying that the like seed produces a like seed, must realise that the total seed of a plant may produce some good, some bad seed,—some good plants, some poor plants,—some plants better, some worse than itself; and that it is heredity which produces these variations, and that we can through the process of selection modify the heredity of the future, so as to influence future seedings. It is this gradual gain, through the influencing the heredity of the seed, that concerns the seed-breeder. This would seem to imply that the tendency of the seed is to reproduce the plant from which it was grown. This is indeed so, because, speaking of the plant as a whole, it may be said to have in its structure a species of *inertia*,—that is, it tends to keep the form and condition it already has, except as diverted therefrom by cause. We cannot think of a change happening to a life without some cause to produce it; and in just so far, and in just that direction that causes have acted, and continue to act, must this *inertia* of the plant life be overcome, and the change which is adequate to the cause take place. So far from reasoning to prove that continuation of a form in a species does exist, we prefer to say that it must be so from the law of Nature, and challenge others to show the contrary, the burden of proof being with them. Hence, from our point of view, the variations we produce in a plant must be transmitted through

the seed, unless they are nullified by some opposing or counterbalancing variation which shall disguise their effect. Let us illustrate this fact:—Prof. Buckman, experimenting with the *Vicia sativa*, or vetch, by a series of five selections, so accumulated the variations that occurred, from a plant but a few inches in height, gained plants more than 3 feet in height, and increased the weight of the seeds from half a grain in the original to 1 grain, and then to $1\frac{1}{2}$ grains in weight. Mr. Hallett selected his original pedigree wheat in 1857, the ear being $4\frac{3}{8}$ inches long, and containing 47 grains; in 1861 his finest ears were $8\frac{3}{4}$ inches long, and contained 123 grains. The second year he obtained ten ears to a stool, and the fifth year fifty-two ears to a stool. This was brought about through the selection of variations, and the continued selection, whereby the effects accumulated in the heredity of the plant. In 1848 Prof. Buckman sowed seeds of the wild parsnep and wild carrot, and, by careful selection, in 1851 the plants of the parsnep presented the stems and foliage of cultivated examples, and approached them in the character of the roots. The change effected in the carrots was not nearly so great as that observed in the parsnep, the Professor observes; but still the progress was quite sufficient to show that it is within anyone's power to renew both of these plants in a cultivated form from wild specimens. It is probable that in the Concord grape, and in our fine varieties of cultivated strawberries, we have the action of selected variation to thank for the result, and none of us can fail to have noticed how soon the varieties of any fruit are offered to the public after a break in the wild species or cultivated species has occurred. Indeed, through this power of heredity, this breaking in upon the *inertia* of the wild plant by mankind—this falling back again upon a trained heredity by art—we have accomplished these wonderful results upon the nature of plants, so that now, once cause a wild plant to sport and the way is immediately opened for the breaking up of the original species into numberless varieties, which shall add to the comfort, to the necessities, or to the gratification of mankind.

If such is our power,—if heredity in life cannot be escaped from,—and if it is the causing of variation, and the fixing of the changes induced which concerns us as agriculturists, what practical lesson can we derive therefrom? We will take up the corn plant, and show what capabilities are offered to energetic and skilful effort to improve this plant through the heredity of the seed, because few species are subject to greater or more valuable variations, and

hence it is especially fitted to respond to any wise effort of ours.

Zea maize, Indian corn, or simply corn, occurs within a wide range of climate, and is modified accordingly in the growth of the plant and the structure of the grain. There is a vast range between the pop-corn, whose normal length of ear is $1\frac{1}{2}$ to 2 inches, and the field corn of from 9 to 12 inches, or even 15 inches; or in the number of rows of corn on the cob, which varies nominally from eight rows to twenty-four or thirty-six rows, as the "Canada 8-rowed" and the "Virginia white gourd" illustrate, and exceptionally even more or less. We find some varieties wherein the arrangement of the kernels on the cob is uniformly 8-rowed; other varieties where the rows may be either eight, or ten, or twelve; others still uniformly 12-rowed, or 16- to 22-rowed, or from 24- to 36-rowed. Some varieties bear the crop near the ground, from the lower nodes of the stalk; others, high up, on the upper nodes; and this feature of the plant is, so far as our observation extends, a true and uniform characteristic of varieties. In shape the ears may be blunt, or pointed after a manner, at the extremities; or may be of a long oval, tapering from a swollen centre towards the butt and tip ends; or may form a true taper from butt to tip; or may be cylindrical throughout. The kernel, again, may be arranged in lines wherein there is a distinct evidence of arrangement in pairs, or without any distinction or manifest separation into pairs of rows. The shape of the kernel may be nearly globular, or oval, or elongated; it may possess a flat point, or a dented extremity, or be furnished with a sharp tooth, either straight or recurved: it may be shaped like a horse's tooth, or be flattened; be longer than broad, or broader than long; may be smooth, or wrinkled, &c. In colour the kernel may be white, pale yellow, translucent, dark yellow, orange-yellow, reddish yellow, red, violet, purple, blue, slate, black, or variegated; in texture may be hard and brittle, softer and granular, and in some varieties almost gummy. Its specific gravity varies greatly—some 8-rowed, yellow, like the "Waushakum," weighing 64 lbs. to the struck bushel; others, like the Tuscarora, weighing seldom 56 lbs. to the bushel. The kernels differ in the structure of their contents: in the pop-corns the oils being distributed nearly universally throughout the kernels; in the varieties of the Canada corn the oily appearance being external to the chit and starchy portions, and forming the periphery from the hilum as a centre; in the dents, the apex being free from

this oily showing; in the true Tuscarora, no oil to be recognised, &c. This arrangement, which furnishes the basis for a practical classification of varieties, seems thus far to have been unused. In the cob we find also differences between varieties which are general for each variety—the large cob and small cob, the tapering cob, the cylindrical cob, the bulging cob, the white cob, the red cob, &c. In habit of ripening, one variety may mature in from 90 to 100 days, while another variety may require from 180 to 200 days. These variations we have mentioned are not in the nature of monstrosities, or exceptional in their nature, but are general, and peculiar to varieties. They are hence strongly inherited, and transmissible by the seed in all cases when other conditions than those which have availed in the past are not brought into interference.

Hence, if sweet-corn seed be planted, sweet-corn is harvested; if a pop-corn, then the pop-corn harvest is obtained; if a red corn, then red ears are gathered; if an 8-rowed corn, an 8-rowed crop, &c., throughout the whole list. As an undoubted fact—a fact which is the explanation of our general practice of planting the seed of the crop which we would gather—each seed has a heredity which causes it to develop the plant in the line of the past growth; and whenever, through natural or artificial agency, the character of the heredity has been determined for the seed, through any process by which like forces are accumulated and stored, we find the corresponding development in the offspring. That law of inheritance whereby we say "like produces like," meaning thereby the great law of continuance of development in the line in which forces compel, is hence so evident that as breeders of seed, or of animals, we can assume it as an axiom, and drop it from our discussion as too well known, too fully realised by the educated and trained mind, to require explanation or illustration. What is of practical concern is the defining of certain kinds of heredity, the accumulation in line of desired tendencies, knowing that the law of likeness is uniform and universal, and acting always within the lines of its application. To mark out these lines, to compel the action in the direction for our profit, is the true art for practical study, use, and endeavour.

If this position be the correct one, as we firmly believe, we may pass to the consideration of "variations," knowing full well that variations in all cases have a tendency, more or less strong, to be continued in the offspring, and that, if we can retain desirable variations for a sufficient number

of generations, that these will become established as permanent.

The production of variations in a plant is easily acccomplished. Any interference with the normal condition will cause them—a change of climate; a little more or less heat or moisture; the influence of crossing with the pollen of another variety; or an interference with the growth of the plant; or abundant or scant nutrition: all these, of the more evident causes, are effectual. A small eared, 8-rowed Canada corn, removed to a more genial clime than where it has been grown, increases in height of plant and in length of ear; a little greater change, and the tendency to increase of rows of grain on the cob becomes manifest. An 8-rowed flint-corn, removed and cultivated in Ohio, in one instance, became, from seven years' use, closely allied to gourd seed, being much dented, and the number of rows on the cob had increased to twelve and twenty. In Louisiana, the continued cultivation of soft gourd seed-corn from the West produced a hard flint-corn with a larger cob, in twelve years. This change, which is too well recognised to require further illustration, is also accompanied by a change in the habit of growth of the plant, which is not confined to the shape, and structure, and composition of the seed, but also to the habits of maturity,—the seed from northern localities, the first year after removal, ripening earlier than the varieties native to the region, but shortly approximating the periods common to the locality. It is this fact which enables the extension of culture into localities whose temperature will not admit of the immediate transference from southern sources, but which can be entered through the gradual cultivation through intermediate stations. Thus the potatoes, which require five months for their development in Germany, are cultivated, according to Leopold von Buch, in Lyngen, two degrees beyond the polar circle, and attaining their full development in little less than three months. The feature to be noted in these cases is, that these variations, brought about through means at the control of man, become fixed in the variety.

The effects of temperature and moisture are of great avail towards changing the habits of the plant, yet from the nature of the case cannot be well separated and defined. We have observed a difference in the transpiration of water from different plants, a variation which is of extreme importance as leading to the selection of that variety which requires the least water for the processes of growth, for planting in drouthy localities, and *vice versâ*. Heat alone,

moisture in sufficient quantity being present, tends to an elongation of the internode of the plant, while a diminution of temperature tends to shorten the nodes, and is suggestive of other changes in the character of the plant and its fruitage. As a general rule, for varieties of corn which are quite similar, elongation of the internodes means an influence on the ear; but there seems to be many exceptions, and the matter is worthy of further study.

The influence of hybridisation in corn is very peculiar, but always results in a change of character in the kernel. Thus we have before us an ear of a pale yellow field-variety which contains sweet-corn kernels, wrinkled, sweet, and of their proper shape. In this case the pollen has determined the development of the ovule of another variety so as to disguise completely the natural order of its development. We have another ear of a deep red corn, whereon the influence of the foreign pollen has been to modify the shape and the colour of the few kernels affected in such a manner that they seem intermediate in character between the two parentages, and in other kernels the influence of the pollination seems to be confined to distinct localities of the kernel. In these variations, these departures from the variety, we have the means at hand for further progress in an intermediate or diverse direction from either parent, because, as is too well known for argument, or even statement, the produce from these kernels would show effects resulting from this departure from the type of either parentage.

The influence of interference with the growth of the plant, in order to produce variation, seems not to have attracted the attention of authors, although of great interest. In some experiments of our own we have found that the obstruction to the flow of sap in the stalk, whether by ligature or by splitting the stalk and injuring the pith in the cross-section, has been in all cases followed by the elongation of the foot-stalk of the ear, and we have thus secured a branching corn-plant. This treatment increased the rate of growth of the plant in height, caused an earlier bloom, and increased the amount of produce over that of adjoining hills not thus treated. The removal of the upper nodes of the plant early in the season has caused a development of ears from the lower nodes, which in the variety experimented upon are universally barren as cultivated in the fields. When the leaves were cut from the ear-wrappings (not the leaf-stalks which form the husk, but the leaf extremities which are formed beyond the ear), the effect on the ear was manifested by irregularity of kernel develop-

ment; when the leaves were reduced on the plant during growth to a large extent, the growth was little vigorous, the plant was stunted, and, although there was an appearance of earing, there were no ears developed into crop, but all were of the class called pig-ears. When the stalks of the plant were daily twisted until a snap could be heard, the earing was very abundant, far more so than in other plants, but all did not develop into crop. When the lower roots were all removed from a plant, by digging up the seed soon after vegetation had commenced, and cutting off all the roots below the seed (removing the radicle), the plant showed a character of growth different from that of normal plants, throughout the season, and tillered largely; one hill which had the flowering-stalks of the plants broken down at the period of bloom gave a better lot of ears, and a plumper kernel, than did another hill whose plants had the flowering-stalk entirely removed; but the number of plants thus experimented on were too few to offer results to afford data for a reliable conclusion.

We now have indicated, in a general way, how variations may be produced, giving data but from experience. We might add, from theoretical grounds, the planting of seed from peloric ears or tassels, from using mutilated seeds, or seed from mutilated plants, from butt kernels or tip kernels, from ears in abnormal positions on the stalk, or from kernels in irregular positions on the cob.

Had we no other means of arriving at a conclusion of the antiquity of maize as a cultivated plant, this very fact of its pliability towards changed conditions, and the manner of its present sportiveness, would be ample. It is indeed far removed from a wild state, and has become domesticated, if we may use the term in this connection. Its present form, in any given case or variety, has been derived from the accumulation of tendencies which have been impressed on its varied generations by the needs of corn-eating man. In those features which have attracted the attention of its growers it shows this variability. In those other features which have been overlooked, and which, if influenced at all by man, have been through correlations, the plant shows a degree of stability which it is difficult to overcome. For instance, the "Waushakum" corn-plant bears its ears on the fifth node from the first, and developes embryo ears at the five nodes counting from the first which sends forth roots. Through no effort of ours have we been able as yet to change its nature in this respect. We have succeeded in causing the ear to be developed on the fourth, or third, or

second, or first, counting downward, but never on the sixth, the seventh, or the eighth. We have not been able, so far as we have observed, to change the plant from a nine-noded one or less, through manure or any other process we have as yet tried. This method of structure has remained constant, because there has been no effort in the past by any one to produce variation, as no variation in this line has promised advantage or been brought to the attention. Yet we have been enabled to vary the grain, the shape of the cob, and the habit of growth, the most readily in those directions in which a practical usefulness could be seen. Indeed the teleological argument is a fitting one as between the plant and man. In other varieties of corn we find a different arrangement of parts, and as strictly defined in the variety as in the case given. Thus a variety grown in Tennessee contains sixteen nodes in all, and the first ear appears on the fifth from the bottom, and on each intervening node up to and including the twelfth. Darling's early sweet corn produces its ears near the ground, as a variety characteristic, as does also the Narragansett, and some pop-corns we have grown. In the Southern white corn, as grown for fodder in New England, the ears develop high up on the stalk.

If we pass to intermediate characteristics, those which probably have been valued as a peculiarity of variety through many generations of culture, we find a fixity which is quite difficult to overcome except through successive efforts, if at all. Colour is perhaps the most prominent. Thus the red corn, occasionally cultivated here and there in New England, and which, although out of general culture, is tenaciously held on to by individuals, presents a fixity of type which overcomes in most instances the effect of hybridisation even in its colour. We know of instances where it is claimed for it that it does not receive hybridisation from other varieties of corn, because the colour continues, but a careful examination of those ears we have seen, and which have been grown among other corn, has shown an influence in the shape and quality of the kernel; and if in a few cases the colour has changed, or become modified, this fact but proves by its exceptional occurrence the fixity of the colour peculiarity. The red cob is another instance in point. We know that some growers place a value on this peculiarity, just as certain features of little real consequence have become valued in poultry by fanciers. We have known of instances where through hybridisation the colour of a kernel and the shape have become changed, while the cob

has retained, apparently, its variety characteristic of a red colour. In the arrangement of the kernels we have a feature wherein there has been little effort to fix the number, as the benefit of a fixity of type here has not been generally recognised. Hence we find in this respect a great variability, and there are but few varieties wherein a good percentage of other numbered rows than the normal number do not appear. Scarcely a field of eight-rowed corn but what will show ten- or twelve-rowed ears to the searcher after them. Scarcely a twelve-rowed corn wherein eight-, or fourteen-, or sixteen-rowed specimens do not exist.

The means in the power of the seed-grower to improve his seed is now indicated, and their efficacy has been shown. It is not heredity; it is not that the seed will produce a plant and a seed like its parent plant; it is no new and novel proposition which has been untried, but, as shown by all the facts of experience and reasoning, it is in the accumulation of desired qualities so as to determine the character inherited; for, let us repeat, even if charged with tautology, that heredity is general in the poor and the good seed alike, in the desirable as well as the undesirable qualities, and that it is but the character of the qualities which are transmitted which concern us most intimately in our efforts to improve. To accumulate good qualities to be transmitted in the stead of less desirable qualities,—to gain even an advance this year, and again another the next, and so on continuously, is the secret of breeding. It is not the plant like the parent plant that is the most profitable, but the better plant than the parent, and all that is to be done is to cause the individual plant to vary for the better, and then to hold on to the gain by each year, each generation to fix this good quality and to add on to it, so that it shall become a characteristic of the variety or the breed. The art of selection, the practice of selection, the continuous adding and subtracting of qualities from our plant is the secret, as well as the plain common-sense of breeding. So long as law prevails and forces act, we have in our plant, in our seed as representative of the plant, the reproduction of those qualities which have been impressed upon it from the past, and by changing these qualities in the present we can pass them along through the intervention of the seed, to future generations, and thus, through the continuous additions and strengthenings, can form the plant which shall devote itself to the carrying out of our desires, and which shall in a measure eventually overcome even what at first thought we would call the design of Nature.

Thus, through selection, winter and spring wheat, which botanically have been considered distinct species, have been transformed the one into the other. The beet-sugar plant, since its introduction into cultivation for sugar in France, has had its percentage of sugar almost exactly doubled through the agency of most careful selection and testing of roots gathered to be used for the growing of seed. By selection during a course of years the early maturity of peas in Great Britain has been hastened from ten to twenty-one days. Among florists' plants the Canterbury bell was doubled by four generations of selection. The wild cabbage plant has been developed into at least ten distinct varieties through cultivation; the crab has been transformed into the apple; the sloe into the plum; the wild grape into the concord grape; the wild strawberry into the large and fine cultivated species, and each year brings numerous varieties thus gained before the purchasing public. The whole secret of all these triumphs being the production of or the seeking for variation, and then putting into action a rigorous selection.

How rapidly changes may take place in a plant through yearly variation is shown by the results of Metzger's growing of corn in Germany from foreign seed. In the third generation, in one case, nearly all resemblance to the original and very distinct parent form was lost, and in the sixth generation this maize perfectly resembled a European variety. In another case, with a "white-tooth corn," the tooth nearly disappeared in the second generation. In our own experience we find that various features of the corn plant resist acts of selection more strongly than do others. It has seemed to us that three generations of selection have sufficed to change the size of the cob and the shape of the cob; that it has had a strong influence in changing the character of the grain, but has not yet fixed it; and that it has had a less effect on the earing habit of the plant, and yet an effect which is very noticeable and persistent.

Let us now consider the corn plant alone, and see what features we desire to obtain, and what must be the principle of selection which would seem the most efficient to bring us success.

As a rule, the one feature of the plant that concerns the farmer the most is that of prolificacy. There is less difference betweeen the values of different qualities of corn grain, from a sale point of view, than there is between the yield of different varieties. Seventy-five bushels per acre of any variety grown is usually more valuable than forty bushels of another variety, and yet these figures represent actual dif-

ferences which may prevail in the same neighbourhood. Hence how to increase the corn crop is an important question. This cannot be done by manuring alone, or by culture alone, but through the intervention of seed selection. If we plant a kernel of corn by itself, and dung it and care for it, we are sure to have an ear produced from it; and yet if this same kernel be planted in a field where it has to overcome disadvantages arising from the presence of other plants, and less of individual condition concentrated in itself, we are not as sure. In field culture we always find barren stalks in greater or less number, according to variety; we always find small ears—pig corn as we call them—no matter the amount of fertility; we always find perfectly formed and shapely ears scarce. This fact of non-productive plants can be proven by a calculation of the amount of grain which would be harvested from fields variously planted, *provided* each stalk furnished one first-class ear.

To plant an acre in hills $3\frac{1}{2} \times 3\frac{1}{2}$ feet distance, and four kernels in a hill, requires 14,224 kernels of corn. If each produced one stalk only, and one good ear shelling 7 ounces of corn,—not an impossible amount for a first-class ear,—the yield would be 111 bushels. If the acre be planted in drills, $3\frac{1}{2}$ feet apart, and hills every 28 inches, containing four kernels, the number of hills planted is 5335, the number of kernels 21,340, and the yield in the same supposition of 7 ounces would be 165 bushels. As a matter of fact it is not uncommon to harvest 3 lbs. of ears from a hill of three kernels, and 4 lbs. from a selected hill of four kernels, or 1 lb. of corn in the ear to a kernel planted. If we accept this as a maximum our yield would, on the suppositions above, be 355 bushels of ears (40 lbs.) and 533 bushels of ears per acre.

These calculations are not offered to show the crop that may be grown, but simply to illustrate the difference we find in practice between our ordinary yields and yields from the best hills of our field, in order to make evident the proposition that improvement in our seed-corn is needed, and that each farmer can find in his field the hint for the direction of this improvement. Whatever man can improve his seed so that every plant shall be the equal to the best of his planting has accomplished the difficult and scientific feat of the formation of a variety which is at present beyond aught that we have reached.

Now manure will not cause prolificacy to an equal extent in corn plants from various seed, as we have shown that under excessive manuring the variation in crop between

sixteen hills under like treatment has been as 1 to $3\frac{1}{4}$ by weight of corn on ear, and as $1\frac{1}{2}$ to $6\frac{3}{4}$ lbs. of stover per hill. Under ordinary manuring the difference has been nearly or quite as great. Under insufficient manuring the variations appear even greater. We desire to lay stress upon this fact, that manuring does not produce crop by itself, and that increase of manure is not necessarily followed by increase of crop. We know of a field which received thirty-five cords of manure per acre, and yet another field fertilised with six or seven bags of Stockbridge corn manure to the acre gave a larger and fairer yield of corn. On Waushakum farm, one rather poor (as we have considered it) piece of land, which has been heavily cropped by the aid of Stockbridge manures in past years, yielded 66 bushels of corn this year when manured with seven bags of Stockbridge fertiliser; while another field, considered our best land, manured with about $5\frac{1}{2}$ cords of clear cow-dung and six bags of Stockbridge fertiliser, yielded 66 bushels (or exactly $131\frac{1}{3}$, and 132 baskets of 41 lbs., respectively, per acre). Hence manure is but an element, an important one, but not sufficient in itself to furnish us large increase of crop.

Will cultivation do it? We can say also the like for our culture, that it is an important factor in the improvement of corn, and yet is not all-sufficient, being but a factor. In an experiment to determine this we took a suitable plot of ground, and manured it most excessively, so as to eliminate by the abundance of fertility the specific effect of the manure in producing differences between the yields (some forty-two cords of cow-dung per acre, besides abundant chemical fertilisation), and planting in hills 4×4 feet apart, three kernels to each hill, using selected corn and equivalent conditions of planting: we had three plots, each containing sixteen hills. Plot I. received no culture whatsoever, the weeds being pulled by hand. Plot II. was hoed thoroughly and frequently during the season. Plot III. was weeded by hand, as Plot I., but a knife was run vertically around each hill to the depth of 6 or 7 inches, at various times during growth. The following are the results of harvest;—

TABLE I.

	No. of hills.	Weight of corn in ear.	Weight of fodder.	Proportion of ear-corn to fodder.	No. of ears.	Length of ears.
Plot I.	16	$29\frac{1}{4}$	72	1 ; 2·46 lbs.	86	$643\frac{3}{4}$ ins.
,, II.	16	$37\frac{1}{4}$	81	1 : 2·17 ,,	92	752 ,,
,, III.	16	$33\frac{3}{4}$	$48\frac{3}{4}$	1 : 1·44 ,,	104	774 ,,

TABLE II.

	Total No. of ears.	No. of good ears.	No. of fair ears.	No. of poor ears.	Ears good as seed.	Merchantable ears.
Plot I.	86	38	6	42	2	44
,, II.	92	57	13	22	5	70
,, III.	104*	47	22	35	26	69

We can study this table with advantage, as there is shown an influence of cultivation upon the ear and upon the crop, which conforms to the general experience of farmers, and hence is undoubtedly a true and not an exceptional influence.

It is to the selection of the seed that we must look for the important factor, which manure and cultivation shall act upon to give the largest possible result. As a rule, seed has been selected with reference to the grain, and not with reference to the plant. We hence find many varieties of corn with grain of superior quality and ears of fair appearance, but the appearance of the ear gives us but little clue to the prolificacy of the plant. In our own experience we have had the results from seed corn from similar ears vary as 2 to 1, or as an actual result 55 bushels of grain per acre from the one, and 110 bushels from the other, under equivalent conditions of treatment. It is to the corn plant that we must look first in the improving of seed corn through selection, and we must bear in mind continuously the principles of breeding, remembering that by a law of nature the seed *must* transmit the tendencies it has itself inherited and acquired.

If there is one principle of breeding which is more fully and satisfactorily determined than another, it is that of the survival of the fittest. That animal or plant which is best fitted to overcome obstacles is the one which in the long run will succeed in establishing itself and its race. If we desire to modify individuals of either class, animals or plants, we by our interference furnish them advantages which aid them in overcoming, or prevent them from coming into conflict with the competition which they cannot endure. Thus our corn plant is protected during growth from competing for the possession of the ground with weeds; it is aided and encouraged through the furnishing of conditions suited to root-extension by means of the plough, the harrow, and the cultivator, &c. If we study the corn plant with reference to

* Three ears of large size too much affected by smut to count in the harvest. On Plot I. considerable smut, but none to destroy the ears. On Plot II. less smut, but no ears destroyed.

its parts, we find the same rule to apply. If we encourage an excess or produce a deficiency of leaf surface, the grain suffers from our interference. If we examine into the physiological structure of the stalk, we find that nature has provided the embryos of many ears, but that in the ordinary course of events, one ear, usually the upper one, develops, attracts to itself through its superior vitality the nutriment from the stalk, while the lower embryos are apparently unable to struggle against this prepotency of the one, and starve. We can state as facts that in a field where the average productive stalk yields but one ear, and this the upper one, its removal will cause the next lower to develop and form the crop; and thus by our interference we can cause the one to develop which we shall aid by removing its competitors in the struggle to secure the advantage.

What conclusion, then, can be logically derived from what we have stated?

1st. It seems to us clear that our first effort for the improvement of seed corn is to remove the changes which come through the hybridising of the corn by pollen from infertile stalks, as thus confining the heredity each year within the lines of productive parents, and thus bringing the heredity to act stronger and stronger in the way most agreeable to our profits. This is one species of selection, one influencing of variation, the selecting of prolific parents, and the causing thereby of variation in the direction desired. This has now been practised for two years with the "Waushakum thoroughbred corn," and the effects are strongly evidenced already, while the show for a still greater improvement is very promising. The inheritance or transmissal of qualities is always present in a seed, but we are securing the transmissal of good qualities, and eliminating the bad qualities—meaning good and bad in their relations to man—by this most influential and important method of selection.

2nd. The next direction for selection, as taught by these principles we have referred to, is of the ear. It is of advantage to select seed from plants which develop twin ears, but as the influence of cross-fertilisation as usually allowed by the grower, is to introduce a tendency to barrenness, this way of improving our yields of corn is slow in practice, and possesses an element of uncertainty which should be eliminated. In using seed of a strong tendency towards developing two ears to a stalk, we obtain perhaps the two ears, but the kernels on these two ears, perhaps, are fertilised by

pollen from a plant which does not develop any ear at all; the seed thus grown has a divided quality; it inherits from the mother plant a tendency towards fruitfulness, and from the father plant the tendency to barrenness, and according to the prepotency of either must the result be. While there is therefore a usefulness in the taking of seed corn from a twin-eared plant, this method of selection must be secondary to the first, so long as barren stalks abound in our fields and furnish pollen.

3rd. There is another method of selection which has a relationship to the 2nd, and which has not in practice, so far as we have observed, received the attention which its theoretical importance would seem to warrant. This is the influence of the position of the ear. The upper ear of the stalk, in many of our New England field varieties certainly, and doubtless in the majority, has a prepotency which enables it to develop normally at the expense of the lower embryo ears. Under moderate or poor manuring, this ear (if any) always, so far as we have critically observed, develops, and none others; under better manuring, a second ear, and always, so far as our observation extends, the one immediately below the upper one may develop, but it is probable will not be the equal to the first: under heavy manuring, a third, or even a fourth, may develop, but the quality is apt to deteriorate as we proceed from the top. This series of facts, if facts they are, indicate that the prepotency of the ears decreases as they occupy positions nearer to the ground. In the selection of seed-corn, therefore, we should endeavour to secure the lowest ear on the stock for seed purposes, as we thus are using seed which by overcoming the difficulties of position has proved itself to have great prepotency and vigour, and as the prepotency increases as we go upward, the selection of the lowest ear gives us the advantage of, as well as selecting, a prepotency of position which shall affect the development of all the ears which grow above it, or a plant prepotency, with reference to its seed bearing qualities, as well as the one prepotency we have selected for continuance.

We thus have indicated the three guiding selections which should be the careful concern of the seed-breeder. How long it shall take to secure the fixity of these qualities by continuous selection we cannot say with certainty for any given case, but for limits we have slight reasons for supposing seven and sixteen years. We know from experience that three years only are productive of recognised benefits.

The character of the grain and its arrangements on the cob, the length of cob, &c., are minor objects for the breeder's care, as requiring less time to fix, as being most readily apprehended, and as showing results so quickly as to encourage continued efforts. These minor characters can usually be fixed by three years of selection, if any variety of ordinary culture be worked on, and if the changes desired are not outside of the fixed characters of the variety. If we shall attempt to change a field corn into a sweet corn through other selections than those influenced by hybridisation between the two, or *vice versâ*, we shall have difficulty, perhaps shall fail, most probably shall not succeed. We can always place our efforts most successfully where the plant experimented on indicates by the nature of its variation the direction for us to act. Do we wish to change an 8-rowed corn into a 12-rowed corn? We shall do it more readily with a variety that presents frequent variations from the 8-rowed form, than with another where this variation seldom occurs. Do we wish to diminish the proportion of the cob to the grain? We can do this more readily with a variety where the proportion already existing is far from uniform, than with another variety where the proportion is quite uniform. Do we wish to lenghthen our ears? We can do this the more readily with a variety which varies greatly in the number of ovaries in line on the cob, than with another which presents a more uniform number, &c.

We believe that by the proper selection of varieties to be used as a beginning to work from, that we can so far offset the influences of climate as to grow a flint corn into a dent corn, or *vice versa*; a pop-corn into a field corn; a yellow corn into a white corn; a flat corn kernel into a rounded kernel; a deep-kerneled corn into a shallow-kerneled corn; an 8-rowed corn into a 10, 12, 16, or more, rowed corn; a large cob into a small cob; a short cob into a long cob; an ordinary variety of corn into a branching variety, &c. We believe that the corn ear can be removed, through selection, from the upper node to an intermediate one; can be borne on branches either terminal or axillary; can be transferred to the tassel, &c. In a word, that under the axiomatic law of inheritance, or the transmissal of forces, the process of selection will produce these changes we have indicated.

What kind of corn should the New England farmer seek to develop? First and last, one possessing prolificacy, as being the most important quality and the most difficult to be attained. Then smallness of cob, as being a correlative with the stalk (to be used and found exceedingly valuable as a

forage plant if small and thus easily cured) and as having a direct bearing on the storage of the crop. A small-cobbed corn can be binned in larger piles and earlier in the season than a coarse-cobbed corn, with less danger from mould, and is earlier to be husked and earlier to be binned, and earlier to be shelled and marketed, on account of the small cob drying out its moisture more readily and quickly. Then length of ear, as diminishing the labour of husking. Then depth of kernel and closeness of packing on the cob, a most evident economy. Then uniformity of kernel, as adapting the grain to more varied uses. Then colour, as having a relation to ease in marketing, because if one coloured corn sells more readily than another coloured corn, it is more desirable to be grown.

We would also call attention to the importance of securing adaptability to climate, and to the farm. This can be obtained by closely watching the corn of the farm, and using the evident changes produced by the locality to influence our selection. If there is a difficulty in keeping a 12-rowed variety from changing to an 8-rowed variety; that is to say, that if the 8-rowed ears are constantly appearing in the crop planted from a 12-rowed seed, then it is probable that your selection had better be towards the 8-rowed variation, rather than attempting to retain the 12-rowed.

The improvement of seed corn through systematic effort, guided by careful study of what facts we possess, has scarcely yet attracted the attention it deserves. Success, it seems to us, can be hoped for, and he who undertakes it understandingly must win some degree of success, sufficient at least to amply repay for the trouble and care, besides the satisfaction which shall attend the effort. It is for farmers to assist such an effort by demanding good seed, and by paying for good seed a price sufficient to remunerate the grower.—*From the Report of the Secretary of Conn. Board of Agriculture*, 1878.

NOTICES OF BOOKS.

Scientific Lectures. By Sir JOHN LUBBOCK, Bart. London: Macmillan and Co.

IN this volume the author gives a clear, though necessarily much condensed, view of the results of modern research on the mutual adaptation of plants and insects, and on the habits of ants—a subject which he has personally investigated with perseverance and success.

A particular interest attaches to the attempt made to explain not merely the general colouration, but even the pattern of caterpillars, by a reference to their necessity for concealment. The view taken by Dr. Weissman in his able monograph on the larvæ of the Sphingidæ, and, we believe independently, by the author, is that longitudinal lines prevail among such species as feed on grasses or other narrow-leaved plants, whilst such as browse upon broad-leaved plants exhibit diagonal bands. Thus *Smerinthus quercus*, *S. tiliæ*, *S. populi*, *and Sphinx convolvuli*—feeding respectively on the oak, the lime, the poplar, and the convolvulus—are all characterised by diagonal lines on their sides. On the other hand, *Anceryx pinastri*, which eats the narrow needle-like leaves of fir trees, has merely longitudinal lines. The chief difficulty in the way is that the larvæ of not a few species have more than one food-plant, and may be found both on broad leaved and narrow-leaved vegetables. Thus *Chœrocampa celerio*, *C. elpenor*, and *C. porcellus* feed upon comparatively narrow-leaved plants, such as different species of *Galium* and *Epilobium*. Certain entomologists, however,—such as Meigen,—declare that they feed also upon the vine, one of the broadest-leaved of European trees. We have ourselves taken *C. celerio* upon the vine, and can testify that *C. elpenor* and *C. porcellus* in captivity feed quite contentedly upon its leaves. Still it is probable that the bulk of these insects found in Europe have been fed upon *Galium*, and are descended from ancestors whose diet has been similar. Hence we need not pronounce it an exception to Dr. Weismann's law if *C. elpenor* and *C. porcellus* have only doubtful diagonal bands, and *C. celerio* none at all. The strongly-marked diagonal pattern of *Sphinx ligustri* may seem unsuited to a species living on so narrow-leaved a shrub as the privet, but the larva feeds also upon the lilac and the ash. The large blue eye-spots of *C. nerii* are explained as resembling the flowers of the periwinkle, on which it is said to feed in this country. On the coasts and in the islands of the Mediterranean, which may be considered its true home,

it feeds generally—some authorities say exclusively—upon the oleander (*Nerium*), from which it takes its name, and whose flowers are well known to be red.

The conclusion most readers will draw from this section is that the colouration of caterpillars is either protective—calculated to withdraw them from observation—or warning, those which are unsavoury, mal-odorous, or possibly poisonous, being strikingly conspicuous.

The author's account of ants, based on prolonged experiment and observation, is exceedingly valuable. Though he has not succeeded in verifying all the marvels told of "our six-footed rivals," he yet assigns them "a degree of prudence superior to that of some savages." If he cannot urge that "ants must be moral and accountable beings" it is because he has elsewhere attempted to show that, even with reference to man, the case is not by any means clear. His observations as to the effects of different coloured light upon ants seem to show that these insects have the power of distinguishing colour, that they are exceedingly sensitive to violet, and that their sensations of colour must be very different from those produced upon us. In this respect, if we may trust the conclusions drawn in the first of these lectures, they differ remarkably from bees.

The last two lectures deal with archæological subjects. Here, as might be expected, Sir J. Lubbock urges upon his hearers the importance of more efficient steps being taken for the preservation of our national antiquities—a question to which the legislature has unhappily so far turned a deaf ear.

For all those who have not time and opportunity to consult the original memoirs this volume will prove at once pleasant and profitable reading.

The Relations of Mind and Brain. By H. Calderwood, LL.D., Professor of Moral Philosophy, University of Edinburgh. London: Macmillan and Co.

The object of this book is to decide the relative claims of the two well-known rival theories of mental life. Is a physiology of brain and nerve a philosophy of thought, emotion, and will? A school of inquirers—numerous, able, and influential—reply to this question more or less distinctly in the affirmative. Thought is by some said to be a result of the brain, just as is bile of the liver, or urine of the kidneys. Others contend that if brain-power and mind-power are not identical, the one is nevertheless a measure of the other. These views the author examines in the light of recent physiological investigations. From an anatomical comparison of several of the best-known

animal species,—such as the rat, the rabbit, the cat, the dog, the horse, the monkey, the ape, the elephant, and the whale,—he concludes that intelligence and brain-complexity do not keep pace. Whilst we accept this proposition, supported as it is by evidence drawn from the comparison of different individuals and races of our own species, we must demur to the author's standard for estimating the comparative intelligence of animals. He takes as a test "the results of training when an animal is brought under the influence of man." In virtue of this test he considers the dog entitled to a higher mental rank than the monkey and the ape. But in our own species we are apt to consider the readiness with which an individual or a race submits to slavery, a proof of inferiority. The comparison, too, is utterly unfair; the dog's forefathers have for many generations been placed in close contact with man, while the monkey is rarely born in captivity, and is studied under all the unfavourable circumstances of a climate unsuited to his health. Man is apt to extol those animals which make themselves the accessories of his vices, and to undervalue such as cling to independence. The best instance in favour of Mr. Calderwood's position—the ant—he has overlooked.

Elaborate brains, he considers, are indicative of a highly-developed muscular system. The instances given seem to us to involve some misapprehension :—" There is the brain of the horse, whose muscular power has come to afford the standard by which we estimate the comparative power of the steam-engine. There is the brain of the anthropoid ape, whose muscular power is such that when full grown a single stroke of its arm causes certain death to a man. There is the brain of the whale, so wonderfully minute in the windings of the convolutions as to be quite singular in this respect; an animal the mere stroke of whose tail can endanger a large ship."

Passing over certain fishes, such as the rays, and the large serpents, as the python and boa, whose brains are simple in structure, but whose strength is certainly no less striking, bulk for bulk, than that of any of the animals here mentioned, we ask if the highly-developed brain of the gorilla is the cause, or even the mark, of its great muscular power, why should man, with a brain still more elaborate, be so much weaker? The demands of the human brain upon the system have by some been alleged as the best explanation of man's remarkable muscular feebleness. However this may be, the author shows, from the independent researches of Ferrier and Hitzig, that a large portion of the brain—including most of the region assigned by Gall, Spurzheim, and their disciples, to the "moral faculties"—is connected with the sensory and motor activity of the nervous system, and serves consequently to regulate the movements of the body. Other portions—the so-called "silent" regions, comprising the frontal lobe—have not been found to respond to

the stimulus applied, and their functions are consequently still unascertained. We cannot help here remarking how completely the once dominant system of phrenology has fallen into discredit in its old head-quarters.

The power of emotion, and of intelligent interest to affect the bodily condition, is discussed at some length, and is, in our opinion justly, regarded as a proof that the mind is something more and other than a mere corporeal function. We may give two facts in point, not adduced by Mr. Calderwood, and both taken from the history of exploring expeditions in savage countries. So long as the party is still advancing, and the minds of the men are engaged with novelties, seen or hoped for, their health is generally fair. But the moment it becomes necessary to return, and pass through scenes already traversed, a break-down is not long in making itself manifest. Again, the naturalists or other scientific men attached to such expeditions generally show a higher power of resisting malaria, fatigue, &c., than do their companions.

A most interesting incident is quoted from Kruse. "A deaf and dumb boy was in 1805 found by the police straying about Prague, and was placed in an institution for deaf-mutes, where he received instruction. When sufficiently educated to give accurate answers to questions put to him, he gave a description of his former life. His father, he said, owned a mill; he described the furniture of the house and its surroundings minutely; he gave a full account of his life when at home; told that his mother and sister had died, and that his father had married again, and that his step-mother ill-treated him until he had run away. But he neither knew his own name nor that of the mill; he knew, however, that it lay to the east of Prague. Enquiries were made, and the statements of the boy were found to be correct. The police found his home, gave him his proper name, and secured him the succession to his father's property." We think that this case alone is sufficient to warrant the rejection of Prof. Max Müller's celebrated dictum—"Without speech no reason."

The author's weak point is his evident desire to exaggerate the distinction between man and the lower animals. That among the latter, even in the wild state, there exists a conception of duty which in the rook, the ant, and perhaps in other species, takes the form of positive law, is proved by undeniable facts. In such animals, just as in man, though to a less extent, desire is checked by will and reflection.

Mr. Calderwood holds that prior to maturity of brain development "a child four years of age is found to concern itself with rational laws of life, insisting that deceit is wrong, and injustice and cruelty." Where such correct moral judgments are formed we may be sure that hereditary impressions have come into play.

Whilst, however, holding that the author has fallen into not a few errors of detail, for want of a closer knowledge of animal life, and that he has omitted certain considerations which might have been usefully brought forward, we fully admit that he has made good his main point, and has done useful service in drawing attention to the baseless assumptions of modern "somatism." We feel it a pleasant duty to acknowledge the spirit of candour which pervades the book, and its freedom from the *odium theologicum* and that kindred and equally bitter weed *odium philosophicum.*

The Aborigines of Victoria. With Notes relating to the Habits of the Natives of other parts of Australia and Tasmania. Compiled from various sources for the Government of Victoria, by R. Brough Smyth. In Two Vols. Melbourne: J. Ferres, Government Printer. London: Trübner and Co. G. Robertson.

We have here a most valuable contribution to the fashionable science of ethnology. The author, having been for sixteen years Secretary to the Board for the Protection of the Aborigines, has enjoyed exceptional facilities for obtaining information on the customs and peculiarities of the "black fellows." In addition to the results of his own personal observation, he has placed himself in communication with magistrates, missionaries, surveyors, frontier settlers—with all, in short, who, either from their position or their predilections, were able to throw light upon his subject. The outcome of this prolonged labour is a work which in another half century it would be impossible to produce. Mr. Smyth, indeed, modestly characterises his production rather as a "series of sketches" than as a "scientific work pretending to completeness." Still it will be found that he has proceeded in a systematic manner. He describes the physical and mental character of the natives, their numbers and distribution, the education and training of the young, their marriage laws, the disposal of the dead, their daily life, their food, diseases, dress, weapons, tools, and canoes. He then passes over to an account of their legends and mythology. The second volume is dovoted to the languages of the Australian tribes, and embraces a series of appendices by various contributors and an account of the extinct natives of Tasmania.

The principal shortcomings which we perceive are a proneness to needless repetitions, and a manifest tendency to defend, and even to eulogise, the "black fellows," which the facts and incidents brought forward scarcely warrant. Thus after adducing abundant testimony as to the generality of infanticide among the natives, and stating—on the authority of Mr. W. E. Stanbridge

and Mr. P. Beveridge—that "new-born babes are killed by their parents and *eaten* by them and the (older) children," and that "*meals* are too often made by mothers of their own offspring;" after adding that this custom is not a rite, not a sacrifice, he yet feels free to remark that "every newspaper one reads gives accounts of cases of infanticide, as practised by our own people, *far more horrible* than any known to the Australians."

Elsewhere we read that the Australian native has not, like ourselves, had the advantages of thousands of years of civilisation; "he is as he was created!" Whether this dictum agrees worse with the evolutionist or with the specialist view of man's origin it is difficult to say. If we read the author's description of the horrible initiatory rites undergone by Australian boys before they are admitted to rank as men—rites consisting of five successive mutilations, in the last one of which the back, from the shoulders to the hips, is deeply furrowed with sharp flint-stones; if we further study the account here given of their complicated and oppressive marriage customs, their avoidance of different kinds of food,—not as unwholesome or contaminating, but merely as restricted to some one sex or some particular time of life,—we shall certainly be forced to conclude that the native is assuredly not "as he was created," but has either retrograded or "progressed" in a very undesirable manner. Indeed it is difficult, on Mr. Smyth's own showing, to avoid the conclusion that the life of an ape is more wholesome and less irrational than that of a "black fellow."

The exceedingly involved and complicated nature of Aboriginal customs is, however, a most interesting fact. It reminds us of the parallel circumstance that in a state of barbarism, or at the first dawn of civilisation, mankind have generally spoken highly complicated languages distinguished by manifold inflections, and have gradually, as they advanced to a higher culture, tended to a greater simplicity in speech. Do we here recognise a phenomenon of a periodic natu:e? Or are the savages whom we now meet with not in their upward course from the primitive condition of man, or of the anthropoids, but engaged in a career of degradation, such as we unquestionably meet with in various parts of the animal kingdom? From whichever point we regard it the question is beset with difficulties.

A most useful lesson which may be drawn from the work before us is, that the life of a savage is not, as the ignorant imagine, a career of freedom. What with the council of old men, the dreamers and conjurors, and the inviolable customs and superstitions of his race, the individual black fellow is at every step restricted and interfered with. The most searching civilised tyranny—with the redoubtable Mrs. Grundy as a makeweight—is, in comparison, liberty. There is another conclusion which we cannot refrain from drawing: superstition is often represented as the outcome of religion. Yet of the Aborigines,

who are the most abject slaves of superstition, we read here—"I never could discover anything among them approaching to religion."

As one of the attributes of the Aborigines we learn that bodily deformity, in its various phases, is among them substantially unknown. Since we find, however, that deformed and weakly children are invariably destroyed, the absence of physical imperfection is not wonderful,

The author, if we misunderstand him not, seems to consider the "black fellow" as superior to the Malay, the Polynesian, the Chinese, and to the dark races generally. Thus he writes:—"In talking to a clever Australian native one feels that one is speaking to a person who has all the faculties (though undeveloped) of a European; but the Polynesian, the Malay, and some others, have always seemed to me to belong to races having little or nothing in common with the European." Yet whilst thus admitting racial peculiarities, he quotes, with seeming approval, John Stuart Mill's outburst of abuse against the doctrine of heredity.

A very remarkable physical characteristic is put on record. At the Coranderrk Station children from six to ten years of age have "hair on their backs one inch long and more, and as close as it can sit." The absence or scantiness of hair on the human back has been advanced by high authorities as a difficulty in the way of admitting the descent of man from the anthropoid apes. But if the above account is fully verified the difficulty is greatly reduced.

A new light is also thrown on the origin of the dingo, or wild dog. It has generally been considered to have sprung from stray dogs introduced by the first English settlers, and Mr. Wallace hesitates to recognise it as truly indigenous. The author, however, informs us that bones of this species have been found at the depth of more than 120 feet, beneath beds of clay over which lay volcanic ash. At Lake Timboon its remains have also been found in company with extinct species, such as *Macropus Titan* and *M. Atlas*. The presence of a truly native placental animal in that quarter of the globe is certainly a puzzling anomaly.

It will be seen that the chief merit of this work is as being a rich storehouse of facts and observations. When the author attempts to generalise he is less happy.

We cannot conclude this brief notice without calling attention to the munificence of the Victorian Colonial Government in endowing the learned world with a book so admirably got up and so profusely illustrated.

Journal of the Society of Telegraph Engineers. Vol. viii. No. 27.

The last number of this Journal contains reports of the meeting of the Society held on the 12th and 26th of March and the 14th of May respectively. The more important articles are those on "The Means of preventing Induction upon Lateral Wires," by Prof. Hughes, which has an important bearing on the practical employment of the telephone; on "South African Telegraphs," by J. Sivewright, M.A., giving an interesting account of the peculiar local difficulties which the telegraphic engineers have to put up with in these wild regions, their great enemies being the bullock waggons which knock down their posts and their drivers who make "contact" with their immensely long whips. The bulk of the number is, however, occupied by a very valuable and complete paper by the Honorary Secretary, Colonel Frank Bolton, extending over nearly seventy pages. The excitement caused by the recent more or less successful application of the electric light to the purposes of every-day life has raised up a swarm of would-be inventors, who put forth as new inventions of questionable value which had long since been comfortably interred in the vaults of Her Majesty's Patent-Office; these have been resuscitated and re-patented, only to be once more left to their fate. One of the first things a reasonable inventor would do (and there are such) would be to enquire whether his invention was original or not. In this enquiry he would be met by a formidable obstacle in the fact that the last Abridgments of Specifications relating to Electricity do not go beyond 1866, and that even the very Specifications themselves are always months behind-hand. So late, indeed, as May 14th of the present year it was impossible to ascertain clearly the details of any patent on electric lighting later than those of last year. Our inventors consequently would be obliged to search through the whole of the electric patents from 1866 to the present time, a task the formidableness of which can only be known to those who have gone through it. Col. Bolton has therefore endeavoured to supply the unaccountable shortcomings of the Patent-Office officials, as far, at any rate, as the electric light is concerned, by publishing in a classified list a series of copious abstracts of the Specifications of all the Patents in any way relating to the electric light which have been taken out since 1841, as well as short descriptions of all the discoveries connected with it which have been made since Faraday first procured a spark from a revolving magnet, the whole being brought down to the present time. Col. Bolton divides his subject into two parts:—I. Dynamic and Magneto-Electric Machines. II. Lamps, each part being again divided into sections, devoted to a particular type of machine or lamp. Most of the sections are preceded by a short account of the peculiar characteristics of each type of machine or lamp.

With regard to the manner in which Col. Bolton (who acknow- edges the valuable assistance rendered him by Prof. Ayrton) has erformed his irksome and laborious task we can only agree vith Major Webber, who proposed the vote of thanks customary n such occasions, "that the Paper may be looked upon as an poch on the question of electric lighting." Electricians who re ambitious of discovering the light of the future have now a oint from which to start, and Col. Bolton's Paper will not only revent them from re-inventing abandoned inventions, but will timulate discovery by showing where, and possibly why, their redecessors have failed.

The Paper is illustrated by eighteen figures.

An Introduction to the Practice of Commercial Organic Analysis, &c. By Alfred H. Allen, F.C.S., &c. Vol. i., Cyanogen Compounds, Alcohols, and their Derivatives, &c. London: J. and A. Churchill. 1879.

Mr. Allen deserves the thanks of chemical students for having rought together within a small compass a large amount of nformation relating to the proximate analysis and detection of he better-known organic products used in every-day life, such s glycerin, carbolic acid, quinine, &c.

The Introduction gives instructions for the preliminary exam- nation of organic substances in general. The body of the work egins with the particular examination of such substances as he cyanogen compounds; methylic, ethylic, and amylic alcohol, nd their derivatives, acid, alkaline, and neutral; glycerin; the hlorals and chloroform; the vegetable acids; and, lastly, the henols and their acid derivatives.

The general method in which Mr. Allen treats each substance s to first give its formula, and then its method of preparation, s physical and chemical properties, and the characteristic tests or distinguishing its presence and estimating its quantity. We hink, however, that Mr. Allen has unnecessarily added to the ulk of his book by giving a number of details respecting the ubstances of which he treats, which are to be found either in he worker's memory or in any of our common text-books. He eems to have forgotten that his book is a working manual of nalysis, and not a descriptive treatise. The four pages on Jltimate Organic Analysis, too, might have been replaced by a ew lines giving the bibliography of the subject. There is an xcellent table showing the fusing- and boiling-points, specific ravities, &c., of over fifty of the better-known organic sub- tances. A copious index ends the volume.

CORRESPONDENCE.

EXTRAORDINARY SEASONS.

To the Editor of the Monthly Journal of Science.

SIR,—Perceiving an article on "The Anomalous Season" in your Journal, I forward you the following paragraph from the "Kentish Express." Neither of the seasons therein mentioned agree with an eleven years' cycle. The hot, dry season, 1818, is distant only seven or eight years from the two next similar seasons, viz., 1825-26, and if we add a successive period of eleven years to 1818 we get the series 1829, 1840, 1851, 1862, and 1863, which were certainly not seasons noted for heat and drought. A ten-years' cycle would include 1858 and 1868, which were both fine.—I am, &c.,

A CONSTANT READER.

"Mr. Shadrach Luckhurst, of the Charity Estate, Willesborough, a retired agriculturist, who for a period embracing a long life-time was engaged in farming the parish of Hinxhill, thus writes of one or two past seasons within his recollection. He says:—'1816 was a year never to be forgotten as the wettest, coldest, darkest, and most unfruitful on record. An immense quantity of corn never came to perfection, but blackened, mildewed, and rotted on the ground. The summer of 1818 was the longest, hottest, and driest I ever remember. A hilly field facing the south was sown with wheat in the autumn of 1817. It was cut fully ripe on June 29th, thrashed on July 3rd, ground on July 4th, and bread was made from the flour on the same day. This was the earliest wheat I ever saw. In this year the harvest was general by July 7th, and it was all cut and cleared by the 25th of the month. The whole country was of a uniform brown colour; not a blade of grass, not a turnip or cabbage was to be seen. Cattle perished for the lack of pasturage and water. For all that the year was the most plentiful, fruitful, and prosperous within my recollection."

PROCEEDINGS OF SOCIETIES.

Royal Society, *June* 19.—"Note on 'Spectroscopic Papers,'" by G. D. Liveing, M.A., Professor of Chemistry, and J. Dewar, M.A., F.R.S., Jacksonian Professor, University of Cambridge. In a recent communication to the Royal Society, Mr. Lockyer has criticised the author's statement of Young's wave-lengths identifications of certain chromospheric lines. As to the wave-lengths, the authors throughout their table omitted all figures after the decimal point merely for the sake of not cumbering the table. The numbers, Young mentions, are not his own, but taken from Angström's catologue. Moreover, as to Young's identifications with metallic lines, he states expressly that they were taken from the maps of Kirchhoff, Angström, and Thalén, and Watts's "Index of Spectra." Their object was not to criticise Young's work, but only to use it for the purpose of comparing the behaviour of certain metals on the earth and in the sun, and the conditions under which certain lines appear, or do not appear, or are reversed. They mentioned, in relation to aluminium, the two lines with wave-lengths 6245·4 and 6237·3 seen by Young, not because they thought their identity with the aluminium lines proved, but because they are the only two lines in Young's table which are at all close to aluminium lines, and if they be not due to that metal, then there is the remarkable fact that aluminium in the sun gives no lines either dark or bright except the two which have been reversed on the earth. A somewhat similar remark applies to the potassium lines, only in that case Young's line has a wave-length very nearly the mean of the two potassium lines which are often seen as one, not merely from want of dispersive power (which with prisms is usually sufficient at the violet end), but because the lines are expanded until they meet. The authors at first saw them reversed as one broad line which divided in two as the potassium evaporated. The authors never set themselves to determine exact wave-lengths; but they sought to determine the conditions of reversal, and they used wave-lengths for the convenience of indicating the lines of which they wrote. In general they determined the identity of a dark line with the corresponding bright line by observing that both had the same place on the cross-wires, or pointer, or both gave the same reading of the scale of the spectroscope. They then gave the wave-length of the bright line as determined by some good authority, usually either Thalén or Boisbaudran. When they could not identify the dark lines in that way, they took the readings of known lines with their spectroscope, using, for lack of sunshine, most

frequently Boisbaudran's method and wave-lengths, and having drawn a curve, determined the wave-lengths of the dark lines therefrom. It was in this way that they obtained the wave-lengths of the two dark lines they ascribed to potassium. It is, of course, just within the bounds of possibility that the line for which they obtained the wave-length 4045 may have been an iron line reversed, but, as the principal well-known lines of potassium were at the same time seen bright in the spectrum while those of iron were not seen, it is far more probable—almost certain—that the line was really due to potassium, and the last figure of the wave-length wrongly determined. To attempt to determine wave-lengths to any greater degree of nicety would, so far as their purpose was concerned, have been labour thrown away. Whenever the wave-length of a line observed by the authors to be reversed is given differently from that given by Thalén and that given by Boisbaudran for the bright line to which they assume it to correspond, it was determined in the way above described. The authors state that by their method of working the very high dispersive power required for the specific identification of any substance by the determination of an individual wave-length is avoided; as they depend on the greatly diminished likelihood of error when several groups of lines of the same substance are seen to be continually present at the time of one or more reversals. It was the necessity of being able to rapidly sweep the entire spectrum for the above purpose that caused them to limit the dispersion. A large majority of the wave-lengths given by Thalén were obtained by means of the moderate dispersion of one bisulphide of carbon prism, a less dispersion than we have used; and they consider it would be incorrect to suppose that no enduring work in this field of spectroscopy can be effected except with the enormous dispersive power which Mr. Lockyer recommends.

"Microscopical Researches in High Power Definition," by G. W. Royston-Pigott, M.A., M.D. Cantab., F.R.S. In its general scope the paper is intended to deal with difficulties in microscopic research, usually found insuperable—such, for instance, as the invisibility of minute *closely packed* refracting spherules, existing in double rouleaux, or promiscuously aggregated; when their individual diameter varies between the 1-80,000th to the 1-200,000th of an inch. These difficulties are principally created by overlapping images—due partly to residuary aberrations both spherical and chromatic—partly to the effects of diffraction, caused by brilliant illuminations of spurious disks of light—partly to the constant development of Eidola or false images, which vary the loci of their development according to the nature of underlying structures; and according to the object-glasses being over or under corrected—and partly, and indeed very considerably, created by the use of excessively large

angular aperture. The paper discusses also the relative effects on visibility, of large and small angular apertures in objectives. It shows that the black margins or black marginal annuli of refracting spherules, constantly displayed by low aperture glasses, are attenuated gradually to invisibility as the glasses employed are endowed with the largest apertures. That the black margins also of cylinders, tubules, or semi-tubules suffer similar obliterations. And that, in consequence, innumerable minute details are concealed or destroyed till the aperture is sufficiently reduced. That minute refracting bodies obey the laws of their refrangibilities and display beautiful phenomena, discoverable by transcendent powers of definition; but totally unseen by inferior compensations. And that, in consequence, the so-called achromatism of modern glasses is an illusory approximation to correct vision. Examples are given of molecular structures, varying in form, translucency, and refrangibility, in which natural pencils are caught and displayed in the order in which, as in a rain drop, iridescent rays are emitted by the decomposed light. Several examples are also introduced, in which a high order of lenticular correction beautifully discovers structure hidden, according to Dr. Carpenter, F.R.S., from the great bulk of observers. As the paper deals so often with magnitudes very much less than the 1-100,000th of an inch, a method is introduced of readily estimating roughly such magnitudes between the 1-80,000th and the 1-500,000th of an inch, by means of a micrometer gauge. The writer has been emboldened to grapple with these difficult minutiæ, in consequence of the sharp and clear definition he has attained of spider lines miniatured down to the fourteenth part of a hundred-thousandth of an inch. The eye, accustomed to contemplate this subtlety of form, readily appreciates the one-fourth or sixth of this size—*i.e.*, 1-400,000th or 1-600,000th. The writer has also brought before the notice of the Royal Society a new test for the microscope, displaying bright lines of uniform thickness less than the 1-100,000th, and sharp black lines of much less tenuity than those given by Nobert's celebrated lines ruled on glass, and incomparably more easy of illustration. The employment of various fluids for immersion lenses is carefully considered; and the singular property of castor oil discovered by the writer is referred to. But, as the author's paper on "A Searcher for Aplanatic Images" was inserted in the "Transactions," he now introduces a new form which offers some advantages: by its extended traverse, by its simplicity and economy of light with increase of magnifying power. Finally, some examples are given of producing transcendentdefinition in cases found hopeless by a numerous body of observers. The means also of its attainment are minutely described.

"Preliminary Note on a new Tide-predictor," by E. Roberts, F.R.A.S. The Indian Survey Department having under-

taken the superintendence of tide-registration around the whole sea-board of India and at the port of Aden, and also the reduction of the observations by the method of harmonic analysis, with the view to the prediction of tides for the whole of the ports, it became a matter of necessity, in order to save the large outlay which the numerical operation of their prediction would have involved, that an instrument should be constructed to delineate the predictions. Accordingly, on the recommendation of the Surveyor-General of India, Mr. Roberts was desired to design, and to undertake the construction of, an instrument to include such a number of tide-components sufficient to predict the Indian Ocean tides with all the accuracy necessary for practical purposes. The present machine is the outcome of the recommendation. The instrument combines the following twenty tide-components:—The mean lunar semidiurnal, the first and second overtides of the mean lunar semidiurnal, two *elliptic* lunar semidiurnal, two *evectional* lunar semidiurnal, one *variational* lunar semidiurnal, the mean solar diurnal, the mean solar semidiurnal, the luni-solar-semidiurnal, the lunisolar diurnal, the lunar diurnal, the solar diurnal, one lunisolar elliptic diurnal, one lunar elliptic diurnal, one compound (Helmholtz) lunisolar semidiurnal, one compound (Helmholtz) lunisolar quarter-diurnal, the solar annual, and the solar semiannual. The chief difficulty in the construction of the machine is the finding, within reasonable limits, of proportions which shall represent with sufficient accuracy the periods of the several components, in order that the machine may be used for a considerable period of prediction—say, for twelve months. Very great success has been attained in this respect in the present instrument. For instance, the error of the period of the chief component (the mean lunar semidiurnal) relatively to the mean solar semidiurnal is inappreciable during a whole year's predictions, amounting to about 0·10° only in a period of fifty years. The largest deviation from strict accuracy is 0·37°, after a run representing twelve months. This is, however, of one of the very small components, and insensible in its results. This part of the design may be therefore regarded as practically perfect. The setting of the machine for the prediction of the tide-curves of any part for which the tide-components are known is as follows:—The dials are first turned so that the epoch or time of maximum is exactly under or above the highest or lowest point according as the component is situated on the upper or lower row of components. The cranks are set vertically (the slotted cone of the wheel on the axis having been first released) and the guide-pin thrown out to its proper range to represent the half-amplitude of the component. The proper positions of the hands having been previously determined by calculation for the time of starting, the hands are set and the slotted cones tightened up. The re-

cording barrel is then set to the time and the wheelwork set in motion. The complete setting occupies only a few minutes.

Physical Society, *June* 14, 1879.—Prof. W. G. Adams, President, in the chair.

Prof. Macleod described a plan for Suppressing the Induction Disturbances in a Telephone Circuit. The method suggested is the employment of a shunt consisting of a cell containing platinum plates or wires in very dilute sulphuric acid. The induction currents on the line, having a high potential, escape through the shunt to earth, while some of the telephonic current passes to the telephone. In this way the induction currents are entirely removed, while the sound of the voice is only weakened.

Dr. O. J. Lodge exhibited his New Reversing Key for Electrometer Work, which is preferable to the ordinary forms, as giving a high insulation, small capacity, and not requiring the hand to approach close to it to work it. It consists of four platinum wires, arranged in pairs crossing one another; one pair crossing between the other two. These are the terminals and contact pieces of the key. The middle pair are supported by an endless silk thread, which runs on two pulleys, one of which is fitted with a handle. On turning the handle to right or left the two middle wires are brought into contact with one or other of the two outer wires, and the current reversed at will. The whole is enclosed in a metal box.

Mr. J. F. Moulton then demonstrated the results of the experiments of Mr. Spottiswoode and himself on the Sensitiveness of Electric Discharges in vacuum tubes (see "Monthly Journal of Science," 3rd series, vol. i., p. 443).

June 26.—Earl Rosse in the chair.

An extra meeting of this Society was held on the above date at Cooper's Hill Indian Engineering College on the invitation of Col. Chesney, R.E.

Prof. Unwin, of the College, read a paper on "Experiments Relating to the Friction of Fluids on Solid Surfaces against which they rub." It has long been known that a board dragged through water suffers a resistance varying in some way as the square of the velocity, that a stream has a uniform motion at such a velocity that the component of the weight of the water down its inclined bed is balanced by the fractional drag on the bottom. The fluid in the neighbourhood of the stream is known not to move as a solid mass, the centre moving faster than the sides, and the different fluid layers rub against each other. The adhesion of the fluid to the solid, against which it moves, also gives rise to a sliding or shearing action. Our knowledge of the subject has hitherto been gained from observations on pipes, streams, and from the experiments of the late Mr. Froude with

a plank of wood drawn through the water of a canal. It is desirable to have a set of laboratory experiments, however, as the conditions can be varied more than can be done by such methods, and for this purpose the author had designed a special apparatus. In Mr. Froude's experiments there was a practically unlimited mass of water and a definitely limited extent of solid surface; and his results are not free from certain anomalies. The author thought it might be instructive to try the other case of a limited mass of water, and a virtually unlimited surface; a disk in rotation gives such a surface. In some respects a cylinder would (as suggested by Prof. Ayrton) be the simplest to treat theoretically, but there are experimental difficulties in its way. The apparatus of the author consists of a metal disk rotated on a vertical axis in a vessel of water; and the problem is to determine its resistance to rotation, since this will be equivalent to the water friction upon it. Within the outer vessel is placed a thin copper chamber, the diameter of which is unalterable, but the depth is variable at pleasure. The disk is placed concentrically inside the chamber, where there are two cheese-shaped masses of water, one above and one below the disk, which are dragged into rotation next the disk and retarded next the sides of the pan. The couple required to rotate the disk is equal to the couple exerted by the disk or the fluid when the motion is uniform. Hence the tendency of the chamber to rotate is measured by suspending the latter from three wires in a manner similar to the bifilar suspension of magnets. An index marks whether it rotates or not on a graduated scale, and a weight suspended by a cord measures the force required to keep the index at zero. Let M be the moment of the fractional resistance of the disk; N the number of revolutions per second. Then $M = CN^x$, where C and x are constants. The author has obtained a number of results, which are, however, not yet ready for publication. He mentioned, however, that a rough cast-iron disk has a frictional resistance almost exactly as the square of the velocity; whereas a turned brass disk gave a value of x decidedly less than 2. The resistance is a little greater when the mass of water is larger. These results were calculated for a speed of 10 feet per second. The author hopes to try the effect of temperature, &c., on fluid friction, and viscous as well as thin fluids.

Prof. Unwin also exhibited a piece of apparatus with which he hopes to study the stress of rivetted plates under shear by means of elastic substances, such as caoutchouc. He purposes to stretch the rivetted caoutchouc and photograph the appearance of stress-lines upon it.

Lieut. G. S. Clarke, R.E., explained the process invented by Prof. McLeod and himself for determining the absolute pitch of tuning-forks (see "Monthly Journal of Science," 3rd series, vol. I., p. 257).

Prof. McLeod then described an electric clock used in the experiments on tuning forks. A zinc and steel compensating pendulum moved by its own gravity; but at each beat made and broke a battery circuit by means of two bent springs, one on either side. The current passing through an electro-magnet detained a bent lever until the pendulum swung to the other contact. By this contrivance time was marked. Prof. McLeod found that the platinum contacts frequently stuck together in these experiments; but this defect had been cured by the use of a liquid shunt of dilute sulphuric acid, which destroyed the extra current. This remedy had been suggested to him by Lord Rayleigh. Prof. McLeod demonstrated the complete success of this device, which acts as well as a condenser shunt. He had also observed a curious effect with these liquid shunts, which as yet he could not explain. Two shunts, having the same acid in both, were employed, one shunting the extra current from four Daniell cells and one that from two Daniell cells. The first showed evolution of H and O gas, the platinum electrodes being unaffected. The second showed no evolution of gas, but one platinum plate was dissolved away and deposited in a black powder on the other. He also exhibited a new cell formed of zinc and mercury plates, with zinc iodide solution and mercurous chloride salt. Red iodide of mercury is formed at the negative electrode. The E.M.F. is seven-tenths of a Daniell-cell, but the interval resistance very low and the cell very constant, while there is no local action.

Prof. Guthrie suggested that as the extra current was really a succession of sparks the platinum might be carried bodily over from one electrode to the other.

Mr. F. H. Varley stated that Mr. F. Higgins had observed a similar effect with carbon electrodes in a voltameter, one carbon falling away into a fine powder, and due perhaps to the disintegrating action of liberated gases. He had also himself seen a platinum wire in contact with a carbon one eaten thin and drawn into very fine silky pens, while the carbon was stained blue, although the current passing was of low tension.

Mr. Chandler Roberts suggested that perhaps a hydride of platinum was formed in the case mentioned by Prof. McLeod.

Prof. Guthrie suggested experiments with fluorescent liquid shunts in the dark.

Mr. J. W. Clark then described some experiments on the surface tension of sulphurous anhydride sealed in a capillary tube within a second tube containing the same substance. He found that at low temperatures the level of the liquid is lower in the narrow than in the wide tube. As the temperature rises the meniscus in the narrow tube descends, till at about 156° F. it is level with that of the wider tube, both surfaces being slightly concave. About this temperature the surfaces become plane then concave, the level in the wide tube becoming higher than

that in the narrow one. The experiments are being continued, and Mr. Clark's other results will be communicated to the Society later on.

June 28.—Prof. W. G. Adams in the chair.

Prof. W. G. Adams, the President, exhibited his new measuring polariscope. It consists of three principal parts. The lower section consists of a mirror, a lens, a Nicol's prism, and two other lenses. The upper section consists of lenses and Nicol's prism arranged in the reverse order. Each lens and Nicol's prism is supported separately by screws, and its position can be altered independently of the others. These two parts form a complete polariscope. Besides these there is a middle piece, consisting of two lenses (nearly hemispheres), forming a box to enclose the crystal immersed in oil, their curved surfaces being concentric. The whole middle piece is supported on the tubes of the upper and lower portions, and may be turned about the optical axis of the instrument. The vertical graduated circle carrying the central lenses and crystal may be turned through an angle about its horizontal axis. By means of an arc fastened perpendicularly on the graduated circle, with its centre at the centre of curvature of the central lenses, the crystal may be turned about another horizontal axis at right angles to the former, so that the crystal and the central lenses can be turned abcut each by three axes which are mutually at right angles. By means of a system of toothed wheels in gear with the rims of the central lenses, the crystal and central lenses may be turned separately about the optical axis of the instrument, so as to bring the planes of the optic axes of a biaxial crystal parallel to the plane of the vertical graduated circle.

Sir John Conroy, Bart., read a paper "On the Distribution of Heat in the Spectrum." After referring to Dr. J. W. Draper's supposition that all the rays in the spectrum have the same heating effect, and to his statement that owing to the unequal dispersion of the prism for rays of different refrangibility the method that has been usual for determining the calorific intensity of the various parts of the spectrum is an essentially defective one, the author described a graphical method for eliminating the effect of the unequal dispersion of the prisms, and showed that from MM. Fizeau and Foucault's reassurements, and also from those of Lamansky and Prof. Tyndall, that the maximum intensity is about the middle of the visible spectrum and not at the red end; and, further, that the curves given by various observers as representing the intensity of the heat in different portions of the spectrum are, in reality, the "dispersion curves" for the particular prisms employed.

Captain Abney, R.E., called attention to his published paper "On the Measurement of the so-called Thermo-Spectrum," wherein he shows that the distribution of heat in the spectrum

is a misnomer, and that what was really measured by Lamansky and Tyndall was the energy absorbed by the lampblack and the absorption due to the prisms used. He considered that there was no inherent heat in the spectrum. He found that Dr. Draper had not taken into account the amplitude.

Prof. Guthrie said that Captain Abney had expressed what many thought—namely, that heat was radiant energy.

Mr. Grant then described an investigation which he had made into the induction lines round two parallel coils of wire in the primary coil, an intermittent current of electricity from a Leclanché battery flowed; and in the secondary, a telephone was connected up to detect the induction sounds. With this apparatus he found that with the coils kept parallel to each other there were lines, or rather a surface of minimum induction, surrounding the primary, and that if the secondary were placed in these lines hardly any induction noise could be detected. A diagram, representing a medial section through the coils, showed the lines to proceed from the wire of the coils in two curves resembling parabolas, one from each cross section of the wire outwards.

Dr. Shettle then described his experiments proving the lines of force in a bar magnet to run spirally round the bar between the equator and poles, the equator being decentred and oblique across the bar, as shown by diagrams.

Prof. Rowland, of Baltimore, made some observations on the new theory of terrestrial magnetism of Profs. Ayrton and Perry (see "Monthly Journal of Science," 3rd series, vol. i., p. 287). He said the experiments on which the theory was founded had been attributed to Helmholtz, but they were entirely his own, he having gone to Berlin to make them. The new theory had occurred to himself on making these experiments, but he had rejected it because he found that the potential which the earth's surface would require to have would not only cause violent planetary disturbances, but by mutual repulsion drive objects off the earth. He had made also an experiment to see if absolute motion of electricity would cause magnetisation, but failed to get any effect from it. Then he resorted to calculation to find the magnetic effect of relative motion by rotation of a changed sphere of perfect magnetic permeability that is more magnetic than iron. He found that when the sphere was uniformly charged and rotating there would be a magnetic field in its interior; but, instead of the result of Messrs. Ayrton and Perry, that if the earth were charged to a potential of, he believed 10^8 volts relatively to interplanetary space, the earth's magnetism would be what it is, he found the necessary charge to be $61+10^{15}$ volts. In the ordinary atmosphere this potential would produce a spark nine million miles long, and discharge across to the moon. If the moon were electrified to the same degree, the mutual repulsion would overcome the force of gravity between them. He

therefore considered terrestrial magnetism to be still a mystery. He had also thought that the aurora borealis might be explained by supposing the upper regions of the earth's atmosphere electrified. The winds carrying the upper strata towards the poles, electricity would condense there. This hypothesis is tenable still.

Prof. Ayrton said that whether or not the new theory of magnetism should be so rejected depended on whether or not Prof. Rowland's calculations, or those of himself and Prof. Perry, were wrong. It had been found by Sir William Thomson, from experiments at Arran, that the earth was electrified with respect to the air, and that there is a difference of potential of 30 volts between earth and air for each foot of ascent. This gave 1360×10^{12} centimetre-gramme second electrostatic units as the potential of the earth. The new theory required the potential to be 1011×10^{11}, or supposing the earth to be solid iron, or about 14 times more, a wide margin.

Prof. Rowland said he had not seen the calculations of Profs. Ayrton and Perry yet; but he believed his results to be correct, as he had checked them in various ways.

Mr. Bailley exhibited a modification of Arago's experiment, in which a copper disc is caused to rotate continuously by changing the polarity of four electro-magnets underneath by a revolving commutator.

Mr. Conrad Cooke exhibited a single voltaic element showing the internal current. This is done by forming the glass vessel containing the element into a helical tube between the poles, and hanging a galvanometer needle in the interior of the helix: the internal current deflects the needle.

NOTES.

Biology.

It must be remembered that in addition to the animal and vegetable kingdoms certain biologists admit a third, the protistic kingdom, consisting of the lowest forms of life, holding a doubtful or intermediate position between animals and plants. As such Prof. E. Haeckel ranks the *Thalamophora*, *Radiolariæ*, *Myxomycetes*, &c. To such beings he considers it probable that a polyphyletic origin must be ascribed, the same forms having probably originated at different times and places. Vertebrate animals, including man, are clearly of monophyletic descent. From the *Amphioxus* to man they are the issue of one and the same ancestral group. As regards the Arthropods the case is less certain. Either the Tracheata are descendants of the Crustacea or the Arthropoda are in their origin diphyletic. Among the Echinoderma unity of origin appears certain, and among the Mollusca, again, it is the most probable. As regards the vegetable kingdom, the Phanerogams and the Prothallophytes are probably of monophyletic origin, whilst the Thallophytes are as probably polyphyletic. A similar distinction may be drawn as regards the organs of living beings. The author divides them into typical or semantic,—such as are peculiar to a single class, such as the dorsal cord and vertebral column of the Vertebrata, the tracheal system of the Tracheata, &c. These he considers as having been evolved only in one place and on one occasion. On the other hand, the asemic or atypical organs are found under analogous conditions in various groups, and may have originated independently in a number of cases. Instances are the locomotive, the sensient, and the sexual organs, the heart, &c. A similar distinction may be traced even in languages, the higher being of monophyletic origin and the latter of polyphyletic.

Dr. Bordier has communicated to the Anthropological Society of Paris the results of a comparative examination of the skulls of thirty-five murderers. All of them are of considerable size, resembling in this respect pre-historical crania, and having, like to them, a small frontal development with the parietal region predominating. These large cranial capacities are often associated with cerebral anomalies. The author had recently in his employment a man, decidedly below the average in intelligence, whose skull, after his death, exceeded 1500 c.c. in capacity. The brain presented a conformation peculiar to certain pachyderms. The skulls of murderers present a number of defects and ano-

malies. Dr. Bordier gives the history of some of their owners, in all of whom appears atavism, morbid activity, and a want of equilibrium between the frontal and the parietal faculties. Heredity is manifest in most cases.

M. Décharme describes in the "Comptes Rendus" a remarkable flight of butterflies (*Cynthia cardui*) observed at Angers on the 10th of June. It is calculated that 40,000 to 50,000 of these insects traversed the Rue du Mail, leading to the river, within an hour. They passed swiftly in a general direction from east to west, flying at the height of from 1 to 2 metres above the ground. A violent storm took place to the east and south of the town during the following night. On June 3rd M. Genevay-Montez observed a similar migration of the same species in the valley of the Rhone.

Dr. P. Mayer has laid before the Lyncean Academy his researches on the antennæ of certain Diptera, such as *Syrphus balteatus*, *Eristalis tenax*, *Musca vomitoria*, &c. He finds in the terminal joint certain cavities lined with a sensitive membrane, and serving as organs of smell or hearing.

Most naturalists who have had occasion to walk much about grass and stubble during August have been annoyed by the irritation caused by the harvest bug (*Trombidium autumnale*), an Arachnoid which buries itself in the skin and dies there, causing in most instances troublesome sores and much pain. In most cases the nature of the irritation is misunderstood, and the parts affected wrongly treated. Upon application to a country chemist some ammoniacal preparation is usually supplied, which fails to give relief as it would do to the sting of a gnat or wasp. The wound of the harvest bug should be treated as a sore containing putrid animal matter. The following lotion answers well:—Vinegar, 40 parts; Calvert's carbolic acid, 1 part: mix with an equal proportion of water before use, and apply to the parts affected: it not only heals speedily, but, from its strong and persistent odour, in a great measure prevents further attacks from the very annoying insect.

Chemistry and Technology.

Attention has been drawn to the presence of arsenic in dark water colours by the sudden death of a mechanical draughtsman. In the "Chemiker Zeitung" Dr. H. Fleck states that on a *post mortem* examination the cause of death was first supposed to be an oxalate, and then a narcotic poison. Chemical investigation, however, showed that the liver, kidneys, lungs, heart, and brain were impregnated with arsenic, though the œsophagus contained not a trace, and the stomach with its contents gave a barely perceptible arsenical mirror. The general circumstances of the case excluding the suspicions of suicide and of malicious

poisoning, it was found that the deceased had been in the habit when drawing of passing the pencil filled with colour between his lips in order to point it. The water-colours he had used were analysed, and whilst indian-ink, gamboge, carmine, red eosin ink, neutral tint, &c., were found perfectly free from arsenic, a sample of sepia contained 3·08 per cent of arsenious acid, terra di sienna 3·14, and a reddish brown colour, the name of which was indistinct, 3·15. Burnt sienna, Vandyck brown, bistre, bladder green, brown ochre, indian red, umber (raw and burnt) were also found arseniferous. The exclusion of arsenic from such colours, in which it seems to play no essential part, should be insisted on by the authorities. Most of these colours are essentially iron lakes. Hence it appears that the mere presence of ferric oxide, except in a hydrated state and accompanied by free magnesia in quantity sufficient to neutralise the acids of the stomach, does not act as an antidote to arsenious acid. This case seems likewise to prove that arsenic taken in minute doses can accumulate in the system until it can be readily recognised in all organs, and can exert a dangerous action. The impunity with which the peasants of Styria consume small doses of arsenic would seem to depend upon circumstances not yet fully determined.

The "Apotheker Zeitung" gives the following formula for an ineradicable ink:—1·75 grms. aniline-black are ground up with 60 drops hydrochloric acid and 42 grms. alcohol, and the liquid is diluted with a hot solution of 2·5 grms. gum-arabic in 170 grms. water. If the aniline-black solution is diluted with a solution of 2·5 grms. shellac in 170 grms. spirit instead of gum-water, the result is an ink suitable for writing on wood, brass, or leather.

A letter from M. P. Truchot to M. Dumas concerning the apparatus of M. Lavoisier was read at a recent session of the Academy of Sciences. It appears that Lavoisier's chemical laboratory and physical cabinet have been reverently preserved by his family, and are now in the possession of M. E. de Chazelles, at Canière, near Aigueperse, Puy de Dome. The smallest of the three balances is sensitive to 1-612 grain. The weights belonging to these balances are wanting, but the kilogramme and its subdivisions as established by Fortin are present, recalling the fact that Lavoisier made all the determinations needful for fixing the weight of the kilogramme. There is a small model of an apparatus for the distillation of sea-water. There are a considerable number of precious stones, some of which have undergone the action of fire. Lavoisier is known to have made an experimental comparison of the heat produced by converging lenses with that of the blowpipe fed with oxygen.

With reference to the electric light superseding that obtained from coal-gas, Dr. Greiff contends, in "Die Chemische Indus-

trie," that even supposing the gas manufacture should ultimately be abandoned, the tar colours could be prepared from the residues left on rectifying the petroleum of the regions on the Caspian. These are estimated at 120 million kilos. yearly, and are ten times richer in benzol and five times richer in anthracen than is coal-tar. The American petroleum has not yet been examined from this point of view, but it will probably also prove to be a rich source of aromatic compounds.

Metallurgy, Mineralogy, Mining, &c.

In a Paper read at a Meeting of the Société de l'Industrie Minérale, M. Pourcel, after describing the rise and progress of the Thomas and Gilchrist method of dephosphorising iron, gives an interesting account of his visit to Messrs. Bolckow and Vaughan's works, at Eston, in May last, where the process was carried out in his presence. The method of working in no way differed from that described by Messrs. Thomas and Gilchrist, in their Paper read before the Iron and Steel Institute in May last. The prime conclusion which M. Pourcel draws from his investigations is that the dephosphorisation of iron in the Bessemer converter is an accomplished fact, and that the practical difficulties in the way of its industrial application may be surmounted by an attentive examination of the chemical phenomena which occur during the various operations.

A new application of rapid oxidation by which sulphides are utilised for fuel has been made by Mr. John Hollway, whose process has been fully described and discussed before the Society of Arts. This process has for its object the utilisation of the heat generated by the rapid oxidation of certain mineral substances which have not hitherto been used as sources of heat for smelting operations. The heat thus obtained is employed in the reduction of the furnace charge, which may be composed partly of sulphides and partly of silicious ores. A current of air is forced through molten sulphides, by which means they are very rapidly oxidised. Great heat is thus developed, rendering the process of smelting a self-supporting operation; therefore no extraneous fuel is required, excepting that employed in raising steam for the blowing engines; where, however, water power is available, steam can be dispensed with, in which case all the carbonaceous fuel necessary for the operation is a little coke to start the furnaces, which stands in the same relative position to the ores as wood does to coal in the lighting of an ordinary fire. The process may be defined as a system of fractional oxidation, in which the numerous constituents of a complex furnace charge can be separated from each other and concentrated in different parts of the apparatus, the heat necessary for the operation being obtained by the combustion of a portion of the less valuable constituents.

M. Galippe, in a paper read before the Biological Society of Paris, described the following experiment:—A rabbit received daily for six months a large dose of copper acetate. At the end of this time it was served up at the table of the learned chemist. The liver weighed 70 grms., and contained 13 centigrms. of copper. M. Galippe partook of it, and has suffered no inconvenience.

A new named metal, Norwegium, has been detected and isolated by Dr. Tellef Dahll in a sample of copper-nickel from Kragerö, in Skjærgaarden. The colour of the pure metal is white, with a slight brownish cast. When polished it has a perfectly metallic lustre, but after a time it becomes covered with a thin film of oxide. It can be flattened out in an agate mortar, and in hardness it resembles copper. The melting-point is 350° C., and the specific gravity 9·441. Its equivalent appears to be 145·9. Only one oxide, NgO, has been obtained. With sulphuretted hydrogen it gives a brown sulphide, even in strongly acid hydrochloric solutions, which re-dissolves in ammonium sulphide. With a slight addition of potassium ferrocyanide it gives a brown, but with larger proportions a green, precipitate. The sulphuric solution is turned brown on the addition of zinc, and the metal is deposited in a pulverulent state. The solutions of this metal are blue, but become greenish on dilution.

Two new phosphates, found at Skipton Caves, Victoria (N.S.W.), are described by Vom Rath in No. 79 of the "Bull. Soc. Min." (France). *Hannayite*—Triclinic, basal cleavage perfect; less perfect parallel to two prismatic planes; sp. grav., 1·893. Composition: phosphoric acid, 45·70 per cent; magnesia, 18·90; ammonia, 8·09; and water, 28·20 = 100·89 per cent. Loses, between 100° and 120°, 21·08 per cent water. *Newberyite*—Orthorhombic; cleavage brachydiagonal, perfect; basal, imperfect. Composition: phosphoric acid, 41·25; magnesia (by difference), 23·02; water, 35·73 = 100 per cent.

Huntilite, named after Dr. Sterry Hunt, occurs at the Silver Islet Mine, Lake Superior. According to the "Engineering and Mining Journal" it occurs in two varieties. The most abundant is amorphous, often porous; dark slate, grey, or almost black; dull. The crystalline variety is cleavable in one direction, and of a lighter slate-colour. The probable formula is $AsAg_3$.

The "Mineralogical Magazine" for July contains several interesting Papers, among which we may mention the following:—"Contributions towards a History of British Meteorites," by Townsend M. Hall; "Geognosy and Mineralogy of Scotland," "Preliminary Notice of Supposed New Scottish Minerals," and "On Haughtonite, a New Mica," by Prof. Heddle; "On some Gold Occurrences," by Rev. J. Clifton Ward; "Additional Note on Penwithite," "On Christophite from St. Agnes," and "On the 'Maxwell-Stuart' Topaz," by Mr. J. H. Collins; "On

Japanese Minerals," by Mr. John Milne; "Measurements of Angles of Basaltic Columns in Giant's Causeway," by Rev. J. H. Jellett.

According to the "Breslauer Zeitung" considerable deposits of sulphur are found in the gypsum formation of Upper Silesia, especially at Pschow and Kokoschütz, near Ratibor.

It appears from the Report of Prof. Burat on the Mine-Explosion at Frameries, given in "La Correspondance Scientifique," that more than 100,000 cubic metres of fire-damp must have been evolved in a very short time. All the two hundred safety-lamps (Mueseler's) were extinguished without causing ignition. The gas issuing from the shaft caught fire *outside* the mine (possibly from the engine furnace), and burnt with an enormous flame. When the supply was becoming exhausted the burning gas ran back into the interior of the mine, followed in its retreat by atmospheric air, and occasioned nine successive explosions. The whole occurrence is considered unexampled in the annals of coal-mining.

From the Reports of the Mining Surveyors and Registrars of Victoria we gather that the yield of gold for 1878 is estimated at 755,754 ounces, showing a decrease of 44,000 ounces as compared with 1877.

From a Paper on the Mineral Wealth of Turkey, in "Chemiker Zeitung," we learn that tin, cobalt, nickel, bismuth, and uranium are entirely absent. Chrome iron ore, emery, and copper are plentiful. Coal is found only in the basin of Eregli and Amastra on the Black Sea. There are numerous petroleum wells on the Persian frontier.

Referring to the grey modification of tin the same journal states that, according to Schertehl, tin, under certain unknown circumstances, becomes so brittle as to be crushed between the finger-nails, and has the sp. gr. 5·8. If boiled in water it recovers its ordinary colour and texture, while the sp. gr. rises to 7·3.

Chinese diamonds are, we are informed by the "Technologiste," chiefly brought from the province of Shantung. Men put on thick shoes of straw, and simply roam about the valleys and the rivers. The rough and pointed diamonds penetrate into the straw and stick fast. The shoes are finally collected together in heaps and burnt, when the diamonds remain in the ashes.

In a Paper on Amber M. Helm says that in entire fragments it is permeable to water. It contains as much as 4 per cent of sulphur in the state of organic combination. This sulphur has probably been absorbed by the fossil resin in the state of hydrogen sulphide subsequent to its formation. The author describes another fossil resin, gedanite, which differs from amber by containing a smaller proportion of oxygen, and is softer, more fusible, and more soluble in ether. It is free from succinic acid.

THE MONTHLY

JOURNAL OF SCIENCE.

SEPTEMBER, 1879.

I. THE CRIMINAL LAW OF THE FUTURE.

NOTHING is more common than for men to express an authoritative opinion upon subjects which lie remote from their ordinary sphere, and which they have never made the theme of serious study. Very frequently it happens that their lucubrations are received with more favour than are the writings of the specialist based upon the researches of a lifetime. We see for instances novelists, barristers, dramatists, statesmen, and the like, who have never worked for a single hour in a biological laboratory, come forward with the most charming confidence to solve such problems as the origin of organic species.

Hence surely it will not be deemed unpardonable presumption if a biologist, in his turn, attempts to apply one of the most recent principles of his science to an important social question—we mean to the treatment of crime.

It is well known that the general current of what is called the "spirit of the age" has tended in the direction of increasing leniency to offenders. Capital punishments have been abolished in some countries with but very unsatisfactory results, as may be seen in Italy, Switzerland, and Spain. In others they are greatly restricted, even in case of the most monstrous crimes. "Extenuating circumstances" in France and recommendations to mercy in England save many a villain from the gallows. Not content with this state of things, we have a "movement" and a society for the total abolition of the extreme penalty. What the next step will be if this is effected we know not, unless garotters, corner men, and ruffians in general are to receive a bonus for every unoffending person they maim or kill. It may

perhaps surprise those who are so anxious for the path of crime to be strewn with roses, if they are told that the legitimate conclusions to be drawn from one of the most interesting laws of organic science point in an exactly opposite direction. How this can be the case will at once appear if we refer to one of the stock-arguments of the opponents of capital punishment. They sometimes remind us that if a locomotive explodes, occasioning damage and perhaps loss of life, we do not break it up for old metal, but send it to be repaired. It might, however, strike them that between the defective locomotive and the criminal, especially the murderer, there are points of distinction which make their parable worthless. Let us suppose these differences done away with. Suppose good locomotives were so plentiful that employment for many of them could not be found. Suppose bad locomotives could not be reduced to inactivity and consequent harmlessness at our will, but were all to go on of their own accord, working and doing damage. Suppose, further, that if sent for repairs to a competent engineer we had still no positive means of knowing whether they were rendered trustworthy, or whether their defects were merely hidden for a time, to re-appear on some future occasion. Lastly, let us suppose that these unsafe locomotives had the power of reproduction, giving rise to others no less dangerous than themselves. Were such the case common prudence—the instinct of self-preservation—would at once bid us to destroy such formidable machinery before it had opportunity to work further mischief.

This brings us at once to the point—the question of heredity. Are children not absolutely certain, but at least very likely, to resemble their parents, grand-parents, and more remote ancestors, not merely in complexion, figure, or stature, but in habits, in intelligence, in disposition, in force of will, and in all that is generally summed up under the word character? The doctrine is unpopular, since it has the misfortune to come into collision not with facts, not with established truths, but—what is perhaps worse—with dogmas. Two most antagonistic schools of thought fear that should the principle of heredity be formally recognised their systems will stand in need of revision. On the one hand, we encounter the radical or revolutionary sect of world-betterers, political and social, who not unnaturally see that if heredity is a fact, human equality—a main point of their creed—must be greatly limited, and that aristocracies have an existence founded in the nature of man. On the other hand, many theological and ethical authorities of

great ability and weight consider heredity scarcely compatible with the doctrine of personal responsibility. If a man is born a murderer, how, they ask, can he be justly punished for murder?

We will first address ourselves to this latter class of objectors, as being apparently the more sincere and the more candid. They seem to consider that morally, if not intellectually and physically, every man is born alike. They do not, of course, deny the glaring, palpable fact that crime has run in certain families, but they contend that this result is due not to any transmitted innate peculiarities, but rather to evil example and bad training. These views are forcibly expressed by Dr. J. Mortimer Granville, in a paper on the "Physical Theory of Sin," recently published in "Good Words." None of the advocates of heredity, of course, are likely to deny that example and training in early life and companionship in later years are real and potent factors in the formation of character. Nor can it be questioned that the children of professional criminals are placed under very unfavourable circumstances. If not systematically misled, they are left to their own guidance, and to the contaminating influences around them. Still these post-natal circumstances are, we hold, far from explaining the whole of the case. Instances may be found where children of a criminal strain have been withdrawn from their parents at too early an age to have been corrupted either by bad precept or worse example, have been placed in virtuous society, and surrounded by the best moral influences; yet as they reached maturity their inborn criminal tendencies were manifested on the earliest opportunity. The writer may here be permitted to bring forward a case which happened within the scope of his personal observation, and for which he is prepared to vouch:—G. J. C., a young man of good family, was not certainly a criminal, but was what is known in America as a *loafer*. He was thoroughly idle, shiftless, intemperate, and profligate when he had money at command, and at other times ready to sponge upon his friends. Unfortunately he prevailed upon a young lady, ignorant of his character and position, to become his wife. After a short and unhappy married life he left her, went to Canada, and died there. The lady thus forsaken brought up her two sons—the elder of whom was not three years and the younger about ten months of age at the time of their father's departure—most carefully. That his example or conversation could have had any effect upon them is impossible. All influences for good which could be brought

to bear upon them, at home, at school, and at church, were perseveringly applied. The younger, however, grew up to be the image of his father, in character as in person. If the doubters of heredity would watch with sufficient care, they could not fail to detect the transmission from father to son of traits of character, whims, and caprices incapable of being taught, and little likely to be adopted from imitation. And if dispositions are thus inherited in their minutest peculiarities, are the bolder and more decided features likely to escape reproduction? But why should theologians and moralists fear that if we admit the inheritance of character it will no longer be possible to maintain responsibility? As regards human laws this is altogether a delusion. Suppose a murderer pleads, as an apology for or extenuation of his deed, that he is sprung from a criminal stock, and cannot help taking life. Even admitting this helplessness—which after all by no means follows—Society may reply that this proves only the more convincingly the necessity for his elimination.

Nor can heredity be pronounced incompatible with man's responsibility before God. Does it not, after all, include the theological dogma of Original Sin, which divines have always been able to reconcile with responsibility? Nay, is not the inheritance of evil tendencies formally recognised in the words "visiting the sins of the fathers upon the children unto the third and fourth generation"?

Turn we now to the revolutionary opponents of Heredity, the followers of Helvetius, who consider that if a man is only caught young, duly Board-schooled, and afterwards examined by the Art and Science Department, he can be converted into a genius, an "advanced" thinker, or a cosmopolitan philanthropist, quite as easily as china clay and sizing can be transformed into good cotton cloth. Concerning responsibility to God or to man the writers of this school are not anxious. Nor, if we mistake them not, do they confine their denial of heredity to the moral aspect of man's nature. Of their manner of argument the following may serve as a typical specimen:—"Of all vulgar modes of escaping from the consideration of the effect of social and moral influences on the human mind, the most vulgar is that of attributing the diversities of character and conduct to inherent natural differences."*

Such outbursts are of course vastly easier than the calm and exhaustive collection and appreciation of facts bearing

* John Stuart Mill. Political Economy, vol. i., p. 390.

on the question of heredity. But letting this pass, we may ask if external influences, moral or social, can modify the conduct and character of the individual, what is our right to assume—as the author just quoted evidently does—that their effects must cease with his death, and fail to reach his posterity? Everyone has seen a series of parallel portraits entitled "The Child; what will he become?" Can we suppose that the diverse agencies which have moulded the one into intelligence, refinement, and integrity, but have warped the other into ignorance, vice, and brutality, will leave their descendants equal and similar, the minds of both groups being *tabulæ rasæ* as easily open to good as to bad impressions? Unless we can grant this monstrous postulate we must, "vulgar" as it may seem, recognise heredity as an important factor in the generation of conduct and character.

In meeting the deniers of heredity we are, however, placed at a disadvantage, inasmuch as they have hitherto brought forward no definite and tangible arguments for its rejection, but have contented themselves with the easier task of vituperation, or of mourning over the possible consequences of its general recognition. Till they produce objections of a different stamp they may perhaps be safely ignored. One thing at least is certain, that if there be any truth in heredity it is a principle which cannot be rationally or safely ignored in the framing and enforcement of criminal law. Without at all denying that a normal man may, under circumstances of temptation, transgress against the property and even the persons of his neighbours, we must admit that on the theory of inheritance the criminal class must produce its like. The murderers, the burglars, the ruffians of our day, are the sons of similar characters who infested the country some thirty or forty years ago, and are in turn becoming the fathers of a new generation of evil-doers. Surely, then, a most important point in criminal jurisprudence is the arrest of this succession. Of what use is the occasional imprisonment and the ultimate execution of the habitual criminal if he is suffered to become a father? It is often said by the humanitarian school that in the treatment of crime we have tried severity and failed. To this argument our reply is easy:—Our severity, though in some cases excessive, was illogical, as bearing no proportion to the moral delinquency of the offender. It treated crimes against property more rigorously than those against the person, and hence it failed to eliminate the most dangerous class of offenders, and it totally overlooked the propagation

of a criminal type of population. It delivered over to the hangman a poor woman whose husband had been carried off by a pressgang, and who had stolen a loaf to feed her famishing children; but it left the sons of the highwayman and the burglar free from all *surveillance*, and could see in them nothing different from its most orderly and virtuous citizens.

Fortunately a great European nation has shown us how, by a consistent course of severity, steadily carried out, any given class of a community may be stamped out. No one can fail to be struck with the utter lack of mental eminence among the Spanish people during the present century. Neither in the practical nor in the theoretical sphere have they reached anything beyond mediocrity. If we turn to the history of discovery and invention, the development of the sciences, and their applications in industry, Spain finds there no place. She is eclipsed even by Denmark and Switzerland, whose joint populations do not reach the fourth of hers, but who out of such small numbers have produced men with whom she has nothing to compare. It is the same in speculative philosophy and imaginative literature, the same in statesmanship and in war. The reason of this is not far to seek. It cannot be traced to any original want of vigour and energy in the national character. There was a time when no people on earth could surpass the Spaniards in these attributes. Nor is it, as some foolishly urge, to be traced to the enervating influence of the treasures obtained from Mexico and Peru. Britain, America, and Russia, we see, have not been enfeebled in energy and intelligence in consequence of the gold discovered in Australia, California, or the Ural. The true cause is this—that for more than two successive centuries the Spanish nation was subjected to a most careful and systematic process of "selection"—we can scarcely call it "natural"—by the Holy Inquisition. Every man who towered above his fellows, either by force of character, mental grasp, or by an enquiring disposition, became an object of dislike, and sooner or later was disposed of by death, by incarceration, or by banishment. The search after anything new was substantially interdicted. Those only were safe who, beyond satisfying the animal wants of their system, went duly to mass and to the bullfight, and in all things else took the world easily. The consequence has been the condition of Spain such as we now perceive. This consideration is exceedingly significant. It is a most powerful evidence of the heredity of talent and of force of character. It may well be concluded, hence,

that if, by a systematic and persevering course of action, ability, with its culminating point genius, has been stamped out among one great nation, so, by another course of action equally systematic and equally persevering, roughdom, with its culminating point crime, might be eliminated quite as thoroughly in another. It must not be forgotten that when in the good old times the Holy Office had roasted any man in the market-place, *ad majorem Dei gloriam*, it failed not to keep an especially minute watch upon the doings of his family, whether in the ascending, descending, or collateral lines. Here then it appears was a tacit admission of the heredity of character.

A still more striking recognition of the same principle is to be found in the annals of Oriental despotism. It was nothing unusual for sultan, shah, caliph, or mogul, when condemning to death any man who from points of view then prevailing was deemed an arch-criminal, to include in the sentence all his kindred. That here the mark was grossly overleapt needs little showing. All the children and other relatives, even of a Peace, are no more necessarily criminals than are all the descendants of a blood horse necessarily fit to be entered for the Derby or the St. Leger. But what the criminal legislation of the future will do—unless it be dominated by those who pander to crime—is probably this: the lineage and connections of every offender, and especially of every habitual criminal, will be carefully scrutinised, and all surviving members will be subjected to an unobtrusive but penetrating scrutiny. The younger members of the race will be as far as possible surrounded with such moral and religious influences as may most effectually check and counteract their probable inbred tendency to crime. They will be treated, *mutatis mutandis*, on the same principle as we now observe in handling petroleum spirit, nitro-glycerin, and other specially dangerous substances. Just as we guard the latter from the approach of fire, and prevent them from being accumulated in too great quantities at one place, so will the scions of a criminal family be kept from the contact of incitements to evil. Our courts of justice will have their criminal genealogists, whose records will shed a new and most valuable light on not a few unsolved problems, both of biology and of mental science.

As for the man who has once formally declared war against Society, hoisting, so to speak, the black flag, care will be taken that he shall neither repeat his offence nor after its commission become a parent.

One of the peculiarities of the half-educated public is that

its attention is always rivetted on what happened last. To it the last antecedent seems the cause of any event; the arguments of the last speaker or author on any question appear the most convincing. In like manner it pities the just sufferings of the "poor" criminal, and totally forgets his victim. We are sometimes told that a very "humane jurist" expressed the opinion that the worst possible use to which a man could be put was to hang him. We demur to this view altogether. The worst use to which a criminal can be put is to let him loose upon Society, and they who urge such spurious mildness are in truth monsters of cruelty.

II. AMERICAN NERVOUSNESS: ITS PHILOSOPHY AND TREATMENT.*

By George M. Beard, M.D., New York.

AMERICAN nervousness, during the past half century, has expressed itself by a large variety of symptoms, a number of which are so frequent, so positive in their character, and so important that they have given names to disease, and are known as such. Among these symptoms and expressions of modern nervousness are neuralgia, sick headache, nervous dyspepsia, hay fever, and, above all, *neurasthenia*, or nervous exhaustion in all its various forms. These conditions, with others that might be mentioned, constitute a family of nervous diseases that have developed chiefly during the last half century,—at least, during the present nineteenth century,—and are most abundant, and most severe and most varied in their manifestations, in the northern portion of the United States, although they are found in, and are now extending to, England and the Continent of Europe.

The rise of this family of functional nervous diseases brings a new era into medicine and sociology, for it has no

* Abstract of an Address delivered before the Baltimore Medical and Surgical Society.

precedent in the history of mankind. The ancients had no nervous disease, or almost none, save a few cases of insanity and epilepsy here and there; and our moderns knew little or nothing about them until the present century.

The scientific proofs of this unprecedented nervousness of the Americans during this generation are very numerous.

First of all, there is the *increased sensitiveness to cold and heat* which is observed among all our brain-working classes. Our fathers were content with a temperature of 60° F. We must have, to be comfortable, a temperature of at least 70°; and there are many families who keep their rooms at even a much higher temperature. In other words, we are 10 degrees more sensitive to cold than were our fathers. The heat of our summers is no greater than it was a century ago, but the cases of sunstroke and heat prostration are widely out of proportion to the increase in our population.

One of the very best signs of our civilisation is found in the *premature decay of our teeth.* Special explanations without number have been offered for this long-observed phenomenon—such as the use of sweets, the use of acids, neglect of cleanliness, and the use of food that requires little mastication. But they who urge these special facts to account for the decay of teeth of our civilisation would, by proper inquiry, learn that the savages and negroes, and semi-barbarians everywhere, in many cases use sweets far more than we, and never clean their mouths, and never suffer, except in old age, from cavities in the teeth. The cause of the decay of teeth is subjective far more than objective, in the constitution of the modern civilised man. Similarly, also, with regard to irregularities of the teeth, which, as is now known, are dependent on bad nutrition of the jaws.

Delicacy of digestion is one of the best known and first observed effects of civilisation upon the nervous system. In all the great cities of the East, among the brain-working classes of our large cities everywhere, pork in all its varieties and preparations has taken a subordinate place among the meats upon our tables, for the reason that the stomach of the brain-worker cannot digest it. Three times a day, and every day in the year almost, pork in some form was the only dependence of our fathers in the last generation, who could eat it freely without ever asking themselves whether it was easy or hard to be digested.

The *eyes*, also, are good barometers of our nervous civilisation. The increase of æsthenopia and shortsightedness, and, in general, of the functional disorders of the eye, are demonstrated facts, and are most instructive. The great

skill and great number of our oculists are constant proof and suggestions of the nervousness of our age.

It is demonstrable that nervous diseases have increased in recent periods; and that, with this increase of nervous symptoms, there has been also an increase in the æsthenic forms of disease, and a decrease in the sthenic forms; and, correspondingly, that there has been a change in the methods of treatment of diseases; that neurasthenia—nervous susceptibility—has affected all or nearly all diseases, so that nearly all illnesses occurring among the better class of people—the brain-workers—require a different kind of treatment from that which our fathers employed for the same diseases.

The four ways by which we determine these facts are—*first*, by studying the literature of medicine of the past centuries; *secondly*, by conversation with very old and experienced practitioners—men between the ages of seventy and ninety—who link the past with the present generation, and remember their own personal experience and the practice of medicine as it was fifty years ago; *thirdly*, from our own individual experience and observation; *fourthly*, by studying the habits and diseases of savages and barbarians of all climes and ages, and of the lower orders about us.

Statistics on this subject are of very little value, for reasons that will be clear to those who are used to statistics, and who know how they can be handled. Longevity has increased almost *pari passu* with this increase of nervousness and change in type of disease, and this has been a stone of stumbling and rock of offence to those who have discussed this subject. Both facts are true; longevity has increased among the brain-working classes, and nervousness has also increased. These two apparently opposite facts are harmonised by a third factor which those who have studied this subject have failed to reach—namely, nervousness is not only consistent with longevity, but actually favours it, by preserving the system from attacks of acute inflammatory disease. We do not bear blood-letting now as our fathers did, for the same reasons that we do not bear alcohol, tobacco, coffee, opium, and physical pain as they could. The change in the treatment of disease is a necessary result of the change in the modern constitution. The old-fashioned constitution yet survives in numbers of people, and in such cases the old treatment is oftentimes better than the modern treatment.

In the study of this subject I have compared a very large number of books of travel, and I have arrived at this fact,

in regard to which there can be no doubt whatever, namely, that nervous disease scarcely exists among savages or barbarians, or semi-barbarians, or partially civilised people. Likewise, in the lower orders in our great cities, and among the peasantry in the rural districts, muscle-workers, as distinguished from brain-workers,—those who represent the habits and mode of life and diseases of our ancestors of the last century,—nervous diseases, except those of an inflammatory or syphilitic character, are about as rare as they were among all classes during the last century. These people frequently need more violent and severe purging, more blood-letting, more frequent blistering than the higher orders would endure.

What, now, are the causes of this increase of nervousness in America during the past half century? The primary cause is unquestionably civilisation, especially with its recent accompaniments, as the telegraph, railway, and the periodical press. These three institutions have drawn, and continue to draw each year, most severely on the nerves of nearly all classes, but particularly upon those who are favoured with education. The introduction and popularisation of the railway and the telegraph, and the development of the periodical press, belong, it will be observed, to the nineteenth century, and they have intensified in ten thousand ways cerebral activity and worry. This factor of civilisation applies to all the great countries—Europe as well as America.

But after we have given this cause every credit to which it is entitled, we are yet face to face with this question—Why are the Americans more nervous than any other people on this planet? The answer to this question, which has occupied the thoughts of philosophical observers for the past quarter of a century, is to be found mainly in these factors:—first, the dryness of our atmosphere; and secondly, the extremes of heat and cold. In these two respects America differs from any other civilised country.

Dryness of atmosphere produces nervousness in two ways: first, by taking up and absorbing the moisture of the body, thus causing us to literally dry up. When the atmosphere is moist, perspiration accumulates upon the surface of the body, because the air cannot take it up. Hence, in our dull dog-days, we are frequently annoyed by excessive perspiration. In a dry air which is hungry for moisture, the fluids of the body, as they become vapourised, are rapidly conducted away; the body is thus wasted of its fluids. Dry air also prevents the electricity of the body

from being conducted away, and thus we become excessively charged with that force, and excessively stimulated by its confinement in the body. Moisture conducts electricity; and the moistened air insensibly carries away the electricity of the body, so that it is impossible for the body to become so excessively charged and stimulated. The evidences of this dryness of our atmosphere are numerous and striking. Clothes on the line dry more rapidly than in Europe. The specimens of the naturalists do not so quickly mould; the hair is stiffer and drier than that of our European contemporaries, and requires more pomade and oil. This peculiarity of our climate is observed from the Atlantic to California; and the Rocky Mountain region is far more under the influence of this dryness of atmosphere than even the East. The violent extremes of heat and cold—the bitterness of our winters contrasted with the heat of our summers—excite nervousness by over-stimulation. The application of latent heat and cold, as ice in hot water, is one of the most powerful means of local stimulation that we have in medicine: to this treatment nearly all of the American people in the northern and eastern sections are constantly subjected. Secondly, extreme heat and cold produce nervousness by compelling us to live in-doors in unnaturally dry and overheated atmospheres, and making it impossible, either in summer or winter, to partake of those active out-door exercises and amusements in which our English friends indulge at nearly all seasons of the year. The English climate, as contrasted with the American, is more equable. Its moisture, and even its unpleasantness and disadvantageousness, is favourable to the nervous system; likewise, the climate of our Southern States is more moist and more uniform than of the North and West; and, according to investigations that are variously made, nervous diseases of all kinds, or nearly all kinds, pretty steadily diminish in frequency as we go South.

The institutions of civilisation common to all enlightened countries—such as schools, newspapers, excitement of elections, reforms, and revivals—are themselves the results of climate and race, and are also to be included among causes of nervousness. Civilisation is burdened with information that it must acquire; every year history raises up new facts that the schoolboy of the future must commit and recite. If we would know why the Americans are so nervous, we should contrast the Greek boy with the New York boy in their manner of training in the schools, in their play, and in the whole order of their lives. The Greek boy's life was a

poem, a constant holiday, a perpetual picnic. Of study, toil, or work, to which the New York boy is early trained, he knew nothing. Work is really a modern institution. All culture, history, science, literature, and languages that have appeared in the world during the past two thousand years, the lad of to-day must try to acquaint himself with. Of all these the Athenians knew nothing—could not even predict. When we contrast the life of an American child, from its early school days until the hour it leaves the university or seminary, the many and tiresome hours of study, the endless committing and repeating and forgetting, the confinement in constrained positions, the over-heated and over-dried atmosphere, the newspapers and novels that he is and must be prepared to converse about and criticise, the sermons and lectures which he is compelled to listen to and analyse, the strife and struggle for bread and competence against inordinate competition, the worry and concentration of work made both possible and necessary by the railway, mail-service, and the telegraph ; in view of these facts we wonder not that the Americans are so nervous, but rather wonder at the power of adaptation of the human frame for unfavourable environment. The education of the Athenian boy consisted in play, and games, and songs, and repetitions of poems, and physical feats in the open air. His life was a long vacation, in which, as a rule, he rarely toiled as hard as the American lad in the intervals of his studies.

The rapidity of our modern and American life has a tendency to concentrate an enormous amount of activity in a brief space of time. The intensity, the fierceness, and violence of our toil are the results of our climate, and in their turn they deepen and intensify our nervous sensibility. In the study of this subject the disposition has been to look exclusively at some one of these secondary elements—our haste in motion or our haste in eating, and to consider some one such factor as the sole cause of American nervousness. Indeed I may say that up to the present time this has been the popular mode of interpreting the unparalleled phenomena connected with American nervousness. Effects have, indeed, been confounded with causes—a process of reasoning which, it may be added, vitiates and destroys nearly all human philosophy, and nearly on all themes, but especially on questions of sociology, such as the effects of stimulants and narcotics, or diet, or social customs. American nervousness is a complex resultant of a number of factors—not a single result of one. In order to understand it, to grasp it, to master its philosophy, we must be able to see

these factors all at once by themselves, and in their relations to each other.

There is one disease, the type and centre of a large family of functional diseases, to which I have applied the term *neurasthenia*. If we understand the philosophy of this disease and its treatment, there will be little difficulty in understanding the philosophy and treatment of very many of the family of functional nervous diseases to which it belongs. Neurasthenia is pre-eminently an American disease. It might, indeed, bè properly called *Neurasthenia Americana*. Although it is found in England and on the Continent, it was here first systematically described, and here it exists in greater variety and frequency than in all other countries combined. The generic term neurasthenia—nervous exhaustion—I subdivide into two: cerebrasthenia—exhaustion of the brain; myelasthenia—exhaustion of the spinal cord.

Among the symptoms that I have referred to *cerebrasthenia* (brain exhaustion) are tenderness of the scalp, cerebral irritation, tenderness and whiteness of the teeth and gums, flushing of the face, special idiosyncrasies in regard to food and external irritants, morbid desire for stimulants and narcotics, insomnia in its varied manifestations, dilated pupils, melancholia or mental depression, deficient memory, or power of intellectual control, different forms of morbid fear, as astrophobia (fear of lightning), agoraphobia (fear of places), anthropophobia (fear of man and society), and its opposite, monophobia (fear of solitude), sick headache, and various forms of headache, and pains in the head, disturbances of the nerves of special sense, as *tinnitus aurium*, and specks before the eyes, subjective tastes and odours, dryness of the skin, eyes, throat, and mucous membranes generally.

Neurasthenia is differentiated from organic disease, by taking into consideration these four elements: (1) The fluctuations and inconstancy of the symptoms; (2) heightened reflex action; (3) the existence of some certain special symptoms, which will really be found in organic spinal disease; such, for example, as different forms of morbid fears, palmar hyperidrosis, excessive tenderness of the spinal cord, deficient thirst, abnormally active pupils, mental depression, extreme insomnia, morbid desire for stimulants and narcotics. In certain organic diseases, it is true, there may be heightened reflex action; but, as a rule, reflex action is diminished in organic or structural disease of the spinal cord. Closely analysed, a large proportion of the symptoms of neurasthenia, as I have before described them, are of a

reflex character coming from the stomach, or some part of the genital apparatus, or, if they are not reflex in their origin, are at least made worse by a reflex irritation. To know this fact, and to act upon it in the treatment of these cases, is indispensable for success. One may treat sweating hands and flushing face and various neuralgias and headache indifferently without any permanent effect, until we attack and destroy the cause; (4) those in whom the nervous diathesis predominates are likely to have functional nervous disease.

In regard to the prognosis in cases of this kind, this general statement is sustained by experience, viz.: All of these cases can be relieved; many of them can be absolutely or approximately cured; but in all cases time and patience are necessary to bring about these results. I have watched these cases for years after they have left off treatment, and I keep up correspondence with patients who have been under my care, and thus have an opportunity to know what the issue is. Patients of this kind live to a good old age—may attain even unusual longevity, and may have their best health during the latter part of their lives. In regard to the details of treatment, I will state but a few facts.

First comes *electricity* in its various modes of application—central, general, and local. In the dosage, we are, in recent years, learning these four facts: 1. That it is sometimes best to use it in exceedingly small doses, mild currents and short applications. 2. That it is sometimes well to use a very strong and painful current. 3. That applications may be protracted for hours in succession. 4. That applications may be made much more frequently than is the general custom. These four propositions apply to nearly all our remedies. In truth, we are widening and deepening the system and range of our therapeutic forces by modifications of the quantity and quality and mode of administration. I have long taught that for spasmodic difficulties, like local sprains of muscles, convulsive tic, facial spasms, &c., very mild galvanic currents are preferable; but I have lately seen a case where very powerful and painful faradic currents, applied with the electric brush, or with the sponge, or both, and with as strong currents as could be borne, were more efficacious than the mild currents. Likewise in sciatica, and even other forms of neuralgia, painful currents that make a blister, or are at least very irritating to the skin, may succeed after mild applications have failed. An electro-puncture directly into the nerve itself will cure when the mild currents are powerless.

The ancients classed the divinities as major, minor—*Dii majores, Dii minores.* Similarly, neurotics may be divided into major and minor remedies. At the head of the major remedies—the *Jupiter omnipotence*—stands, without question, electricity; then comes ergot. That ergot contracts the blood-vessels, and thus is useful in local congestion of the brain and spinal cord, is one of the clearly established facts in physiology, and is one of the few definite, solid foundations for therapeutics; but that this effect on the blood-vessels is all that there is in ergot in its action on the body no philosophical student of nervous diseases would claim. Indeed, this contraction of the blood-vessels must be a result as well as a cause. Behind and beyond all this there is an influence which we cannot analyse. In some instances, very large quantities of ergotine may be given with benefit and without any harm that I can trace. I give ergot for immediate effects, for sick headaches, and for headaches of other kinds, and for long continued action in spermatorrhœa and various other conditions.

Another of these *Dii majores* of neuro-therapeutics is *arsenic* in its different forms. I use, not only Fowler's solution, but de Verlangan's, with also the English preparation of the chloro-phosphide. Arsenic is a remedy the effects of which are not, as a rule, felt at once. It needs to be kept up—to be persevered with for many weeks, oftentimes for many months. A well-known physician of New York was under my care for severe neurosis of the stomach, attended with vomiting of all of his food. Though he ate great quantities, he was growing thin and feeble, and he rebelled against nearly every treatment that had been suggested, or, at least, everything that I gave soon lost its effect. I urged him to use arsenic in small doses; at first he was somewhat averse to trying it, and had made up his mind to go to Europe. For a number of months I did not see him, and supposed he had gone to Europe. A short time since he came to my office and reported that he had tried the arsenic as I had recommended, and that the effect had been immediate, and, with but a slight relapse, up to that time permanent. He had gained in flesh, and regained his power to digest food. The remedy, indeed, acted with specific effect upon him.

Another remedy that perhaps will become, if it is not already, one of the major divinities of neurology, is *cannabis indica*. It is one of the drugs by the proper use of which the treatment of sick headache, for example, has been, within a few years, revolutionised, both for temporary relief

at the beginning of an attack, and during the attack, and as a permanent cure, provided its action is maintained for weeks and months. Its quick and permanent influence over the symptoms of headache suggests its great value in other conditions allied to sick headache, or from which sick headache springs.

Another remedy, not very widely known, but one the value of which is easily proved, is *citrate of caffeine*, and allied to caffeine is *coca*, belonging, indeed, to the same family; indeed, it is the active principle of common coffee, tea, guarana, and chocolate. It relieves the pain and uneasiness that follow over-exertion, and the peculiar distress that comes from sleepless nights, for which purpose, I may say, caffeine may also be used.

The *zinc* preparations, particularly the bromide, valerianate, and oxide are sedatives of very great value in various neurasthenias, and I use them with great freedom. I gave once the zinc combination, including the bromide, the valerianate, the phosphide, and the oxide, to a physician who consulted me about a year ago for neurasthenia, resulting from over work in his profession. In a few weeks he reported himself to me to express his gratitude and to testify to the great value of the remedy as a hypnotic as well as a sedative.

Duboisia, the new remedy from Australia, is likely to take a minor if not a major place among the resources of the neurologist. Its effect is somewhat like that of atropine, but yet not entirely like it; and, for the symptom of hyperdrosis, seems to be more effective according to experiments that I have made with it.

Cimicifugin is a remedy the value of which in choreic condition is undeniable, and I am persuaded that its use need not be restricted to those conditions.

Strychnia is one of our older remedies, and I use it sometimes alone, but very frequently in combination with other remedies; yet it cannot be used in all cases, for sometimes it has a depressing effect.

Opium, in small doses, is excellent for many phases of neurasthenia; and were it not for the danger of forming the opium habit, I should use it more frequently than I do.

Alcohol also, in the form of wine, particularly claret and burgundy, is to be advised in some cases of this kind, but not recklessly, or without reference to the age, character, and temperament of the patient. Alcohol is one of the best of our hypnotics in cases where the bromides fail to produce sleep. Where chloral causes severe headache next morning, claret wine, freely used, may produce satisfactory effects

without any unpleasant after effects. In the treatment of nervous cases, it is sometimes necessary to use all of these potent remedies in incredibly and absurdly small doses.

Dilute nitro-muriatic acid, either alone or combined with the vegetable bitters, I use in different forms of nervous exhaustion, especially where the urine is over loaded, as it often is, with oxalates and urates.

Of *cod-liver oil,* I may say that it probably does more for the nervous than it does for the consumptive. Oil and fats, like cream and butter, are brain food, and if used judiciously, as the stomach can bear them, act both as food and as medicine. The oil I use generally in the form of emulsion, and I use it with great freedom.

Of *phosphates,* this can be said: that they belong to the list of over-praised and over-used remedies. All these stock remedies have a certain power which, in very many cases, they soon expend—they reach the limit of effect, beyond which they cannot be forced.

Another new remedy, or comparatively new to this country, is *koumiss*—fermented milk. The power of this remedy to produce sleep is very great, and very satisfactory. It is a means of nourishing the body without disturbing or even using the stomach to any very great degree. Koumiss is really digested milk, and is absorbed and taken up into the system without any strain upon the digestive apparatus. I am persuaded that the use of koumiss in the future is to be very widely extended for all conditions where nutrition is difficult—not only in adults, but in children. The one disadvantage of koumiss in some cases—that it constipates the bowels—is to be met by laxatives.

Another very old remedy, but as good as it is old, where it is properly used, is counter-irritation, which I employ both in the form of *actual cautery*, and galvanic cautery, and very small blisters, so small and so arranged as to cause very little annoyance. Counter-irritation in the hands of those who really understand how to use it without abusing it, is one of the three or four major remedies of neuro-therapeutics. The actual cautery, as it can be used, and is used by those who understand it, is not specially painful, even to the most delicate woman. The pain is in the idea of the thing—in the expectation, and not in the burning. I speak of this point particularly, because the cautery is an agent of such great therapeutical power. This mode of treatment, like the blisters already referred to, must be, and now can be, modified and adapted to the sensitive modern constitution.

Hydro-therapeutics in the form of bags of hot and cold water, the Russian and Turkish baths, and alternate applications of hot and cold is, in skilful and judicious hands, a great remedy for functional nervous diseases.

It is impossible to speak of the treatment of this class of troubles without referring to the *bromides* of potassium and sodium, and lime and lithium. Bromides may now be classed among the old remedies. Their great value in epilepsy has long been known. They are not, however, so well understood in other nervous diseases of a functional character. The bromides may be used in large doses, frequently repeated until the powerful sedative effect is produced, even when there is no sleeplessness; those who use the bromides in this way must know where to stop or to reduce the dose.

There are a few general principles of treatment of which I will speak. First of all, the proper use of *rest* and *work* in the treatment of nervous disease. About a month ago, a patient with ataxy came to me from a distant city in the West; I said to him, "you have left behind you a better doctor than you can find here." He asked, "Who?" I said "*rest!*" I prescribed it for him, and put him to bed. He had been accustomed to take excessive exercise—at least, far more than was good. The next day another gentleman came, also from a distant city of the West, with the history of a certain form of cerebrasthenia—brain exhaustion—without any myelasthenia, or spinal exhaustion—and of a type that would be benefited rather than injured by a degree of mental and physical activity. He had felt disheartened and thought there was little for him to do in this world. He was of about middle life, and I told him that he probably was no more than "half-way home," and, so far as the disease was concerned, he might live and be active for thirty or forty years longer. When he returned, I said to him, "you have come a long distance to consult me, but you left at home a better physician than you can get here." He asked, "Who?" I said "*work;* work I prescribe for you. Take that in connection with all your medicine and you will recover." These two cases made clear the opposite methods of treatment.

A second and general suggestion is, that of a stopping treatment or suspending it at times. Suspending treatment has a positive effect upon the system. Oftentimes it makes a direct impression, which may be better than continuous treatment. A friend of mine, formerly a sea captain, states that when sleeping in his cabin at night, if the sentinel walking the deck above him stopped, it would always wake

him. The sudden sensation of nervous activity, like a jar upon the nerves, aroused him from his slumber. I find that patients sometimes do better—make more decided progress—in these intervals of treatment than while the most active measures are being used. Patients sometimes imagine this a proof of the valuelessness of the medicines; but it is in reality a proof of their power. It has been said that success in life depends largely upon knowing just where to stop. In the practice of medicine, this maxim is certainly sound; and to know where to stop, to let up, to modify the treatment, is one of the best tests of medical skill.

The third general suggestion is, that in the treatment of nervous diseases, we should study with all our energy the psychology of our patients; we must make a diagnosis of the intellectual character as well as of the disease before we can make a prognosis or adopt a plan of treatment. There are those whose minds are so organised, which lack some qualities and have excesses of others—usually a preponderance of the emotional, with a deficiency of the higher intellectual qualities—that they act badly under any treatment, however wise. Some patients take a pleasure in their distresses; it would be cruel to cure them; their pains are their possessions. Any man wishing to make them well would be no better than a thief or a robber. There are those whose chief felicity in life consists in doctoring and being doctored, and to whom the removal of their bodily ills would be like the death of long cherished friends. When such persons come under your care, you cannot expect any treatment to be as successful as with those strong and active intellects, who understate rather than magnify their troubles, and are resolutely determined to get well.

III. MOVING ROCKS.

A REMARKABLE instance of rocks moving out of water on to dry land is recorded by the Earl of Dunraven in an article on "Moose-hunting in Canada," which which will be found in the July No. of "The Nineteenth Century." The attention of Lord Dunraven was directed to this phenomenon during a visit to Nova Scotia in the fall

of last year, but only a day or two before he left the woods, so he had not time to make any investigation into the subject. A lake of considerable extent, but shallow, was full of great masses of rock. "Many of these masses," observes Lord Dunraven, "appear to have travelled right out of the lake, and are now high and dry, some 15 yards above the margin of the water. They have ploughed deep and regularly defined channels for themselves. You may see them of all sizes, from blocks of, say, roughly speaking, 6 or 8 feet in diameter, down to stones which a man could lift. Moreover, you find them in various stages of progress, some a hundred yards or more from shore and apparently just beginning to move; others half-way to their destination; and others again, as I have said, high and dry above the water. In all cases there is a distinct groove or furrow which the rock has clearly ploughed for itself. I noticed one particularly good specimen, an enormous block which lay some yards above high-water mark. The earth and stones were heaped up in front of it to a height of 3 or 4 feet. There was a deep furrow, the exact breadth of the block, leading down directly from it into the lake, and extending till it was hidden from my sight by the depth of the water. Loose stones and pebbles were piled up on each side of this groove in a regular clearly defined line. I thought at first that from some cause or other the smaller stones, pebbles, and sand had been dragged down from *above*, and consequently had piled themselves up in *front* of all the large rocks too heavy to be moved, and had left a vacant space or furrow behind the rocks. But if that had been the case the drift of moving material would of course have joined together again in the space of a few yards behind the fixed rocks. On the contrary, these grooves or furrows remained the same width throughout their entire length, and have, I think, undoubtedly been caused by the rock forcing its way up through the loose shingle and stones which compose the bed of the lake. What power has set these rocks in motion it is difficult to decide. The action of ice is the only thing that might explain it; but how ice could exert itself in that special manner, and why, if ice is the cause of it, it does not manifest that tendency in every lake in every part of the world, I do not pretend to comprehend."

IV. PETROLEUM AS A STEAM-MAKER.

THERE are said to be 7,000,000 barrels, of 40 gallons each, of crude petroleum now above ground in the oil regions. Every hour adds to this ocean of oil; in spite of the enormous consumption the stock accumulates. Every new use to which petroleum is applied possesses interest to producers, and the day that shall see crude oil take the place of coal as a steam-producer will be a glad day for mankind in general and oilmen in particular. That such a day is not very far distant seems evident after an inspection of the working, recently, of an oil-burning device tested on a river steamer at the Monongahela Wharf. The experiment is thus described in the "Pittsburg Telegraph:"—

The invention is the property of the American Hydrocarbon Gas Company (John Campbell, General Manager), and embraces simple but vital principles of construction, wherein atmospheric air and steam are combined in proper proportions with oil, and injected into the firebox beneath the boilers in the form of spray. The latter, being immediately converted into inflammable gas, becomes a pure, bright, powerful flame, devoid of smoke, and producing intense heat.

To accomplish this result extremely simple machinery is used. A small hole is drilled into the iron front of the firebox, and into this passes a tube which branches as it leaves this point into two pipes. One of these connects with the boiler itself, and the other with the receptacle containing crude oil. At the juncture of these pipes there is an aperture for the admission of outer or atmospheric air. Valves of peculiar construction regulate the quantity of steam or oil admitted to the furnace. This is all the machinery required, but its operation is wonderfully complete and remarkably successful.

The little steamer *Billy Collins* was selected by Mr. Campbell for the test, and was fired up at 9 a.m. A preliminary blaze of wood under the boiler raised the small quantity of steam necessary to start the burner into operation. The oil valve was opened a trifle, the steam valve ditto. The petroleum trickled into the feed-pipe, was caught up by the steam, and both plunged into the depths of the firebox, a mass of many-tongued, roaring, brilliant flame.

As the pressure of steam increased, this flame grew in fury and intense heat, roaring through the entire length of the boiler with a sound like the coming of a thunderstorm. The needle of the steam gauge climbed rapidly up the dial, and in twenty minutes the safety valve blew off at 120 lbs. pressure. It was a remarkable sight. Here was a boat puffing through the water with no sign of smoke from her chimneys, no speck of soot in flues or firebox, no fireman, no opening of furnace doors, no dirt, no coal going in, and no clinkers or ashes to be seen anywhere. A turn of the hand regulated the terrible flame that seemed trying to overpower the limits of the furnace, and another turn of the hand brought the fire down to a quiet little flame, a foot or two long. During the forenoon occupied by the test about 20 gallons of crude oil were consumed; and Mr. Campbell's estimate was, that with oil at one dollar per barrel this fuel was equivalent to coal at six cents in heat-producing value, other things being equal.

But other things are not equal by any means, and everything is in favour of oil as against coal. The labour and expense of "firing up" is dispensed with, and the engineer can regulate the flame as he does the steam in his engines. The danger from sparks and flying cinders is entirely done away with. The space occupied by oil, as compared to an equal value of coal, is very much less, and this much is gained for cargo. Further, the wear and tear upon boilers, grate bars, &c., is infinitely less, and, it seems scarcely necessary to add, the comfort of passengers is greatly enhanced by the absolute freedom from dirt of all kinds.—*Scientific American.*

V. THE MAY TORNADOES OF KANSAS AND MISSOURI.

By Prof. John D. Parker, Kansas City.

ON May 30, 1879, occurred two of the most destructive tornadoes that were ever known to visit the Lower Missouri Valley. This statement is verified, whether we consider the violence of the tornadoes, the extent of territory passed over, the amount of property destroyed, the

number of persons injured, or the loss of life.* These two tornadoes we have named from the towns where they did the most damage, and will treat them as follows:—

The Lee's Summit tornado seems to have originated near Belton, Cass county, Missouri. In the afternoon of May 30, 1879, there was a heavy shower at Belton, accompanied with hail, while further east another heavy storm was raging. Late in the afternoon these two storms seemed to unite to form the tornado, which passed off in a north-easterly direction. At Raymore, Cass county, several persons state that they saw the clouds gradually approach each other, forming two funnels in the air at the same time, which seemed to approach and play around each other, and then unite to form one mighty column, which swayed and rocked to and fro like a huge balloon, with the roaring and rushing of a thousand locomotives, as it passed on its way, levelling everything before it.

At Lee's Summit the people report the weather, on the afternoon of the 30th of May, as very sultry and oppressive, with a warm wind blowing from the south. Toward evening a cold current of air came down from the north-west, accompanied with hail and some rain. About six o'clock a black cloud from the south-west suddenly burst upon them, and the tornado swept by about two miles south of the town. A correspondent, who visited the path of the tornado, reports that everything in the shape of vegetable life was mowed clean, and the ground torn up in places, especially on hill-sides, as if hundreds of men with shovels had dug it up for a road-bed of some giant railroad. The largest trees were twisted off close to the ground like pipe-stems, or taken up by the roots and carried for hundreds of yards, and then dashed to the ground and splintered, in some cases, as fine as kindling wood. Ponderous rocks were hurled from their beds hundreds of yards and broken into fragments. Buildings and fences were swept away, and timbers carried in some instances over a mile and driven endwise several feet into the hard earth. Animals are reported as having been taken up and carried some distance, and let down uninjured.

The people at Blue Springs, about twelve miles north-east of Lee's Summit, liken the tornado to a huge tower of inky

* I take pleasure in acknowledging my obligations to various persons for assistance furnished in reference to the May tornadoes, especially to Dr. F. A. Ballard and Mr. Charles H. Clark, of Independence, Missouri, and to Dr. Isaac B. Smith, of Frankfort, Kansas, and also to Miss Kate Slosson, a pupil of Mrs. Clara Hoffman, Principal of the Lathrop School, Kansas City.

blackness reaching to the sky, preceded by a fearful rushing and roaring noise, the mass of the ascending column being perfectly opaque. Passing south of Blue Springs nothing could withstand the violence of the tornado, the most substantial houses in the vicinity of that place being swept away in a moment. Numerous instances are given showing the violence of the storm. The mould-board of a plough was wrenched off and carried some distance; a new waggon was wrecked, the spokes being wrenched from the hubs, and the tires bent into a variety of fantastic shapes. Trees were stripped of their bark and looked as if scorched, which phenomena was first attributed to electricity, but afterwards found to be a discolouration by a peculiarity of the sap. Prostrated trees were found lying some at right angles to the path of the tornado, and some pointing toward the vortex. Evidences are abundant that the funnel of the tornado contained a large amount of *débris*, and mud having a sulphurous odour, which was dashed with tremendous force and plastered over every obstacle remaining in the path of the tornado. A fence near Blue Springs, running east and west, was thrown down, the west end toward the south, the east end toward the north, showing the direction of the currents. A short distance beyond Blue Springs the tornado seemed to be lifted from the earth, not doing any more damage.

A correspondent, going from Blue Springs to Lee's Summit, says that for several miles the road runs along almost parallel with the track of the tornado, which was nowhere more than three hundred yards in width, and in some places was contracted to fifty yards. Some persons escaped who were caught in the tornado, although instantly blinded, stunned, and covered with mud, and carried they knew not whither. The path of the tornado was deflected a little, about three miles south-west of Blue Springs. It seemed to sweep over hills and ridges and through ravines alike, carrying everything before it. It is impossible to give the mass of particulars gathered for this article, and many reports must be taken *cum grano salis*. Some four persons were reported killed, and a large number injured, some perhaps fatally. A correspondent traced the track of this tornado, giving range for the path examined of nine miles east and twenty-one miles north. The tornado passed along an elevated portion of the country, or a divide, the heads of streams along its course flowing from it both east and west. One of the U.S. Signal Corps observers, I learn, traced the path of this tornado back nearly to Paola, in Kansas.

The Irving tornado had its origin probably as far west as Ellsworth county, Kansas, and crossed the Saline river at the mouth of Twelve-Mile Creek, where it did its first damage. It travelled in a north-east direction, through Lincoln, Ottawa, Clay, Riley, Marshall, Nemaha, and Brown counties, and passed into Richardson county, Nebraska. The same general storm touched Cawker City and Beloit, Mitchell county, Kansas, at about 2.30 p.m., unroofing at the latter place the tent of a circus which was in full blast, blowing down a number of houses, and twisting large trees in pieces as it passed down the Solomon river; this storm being, perhaps, an outlier or feeder of the main tornado. The Irving tornado passed about four miles south of Delphos, which has since been almost destroyed by another tornado, June 9, 1879.

After passing the Solomon river, the Irving tornado seemed to increase in violence and destructiveness. Some describe its appearance in its approach as "cloudy pillars" resembling smoke, afterward assuming an inky blackness, all rolling, dashing, and clashing with each other, as if engaged in a furious battle. The tornado struck Stockdale, Riley county, doing some damage, whence it passed on, crossing the Blue river, lifting all the water out of the bed of the stream, and scooping the water, it is said, out of a well.

The people at Irving saw, just before 5 p.m., a dark mass of clouds gathering south-east of the town. A deep roar was heard, when the clouds began to lower and spin like a top, advancing upon the town and destroying one small house in the outskirts. This seemed to be a prelude, followed by a calm. Suddenly the heavens turned a greenish hue, and, with an awful roar, the tornado burst upon them, leaving the southern portion of the town a mass of ruins.

The tornado then swept onward, with a path from one-half to three-fourths of a mile in width, and when within two miles of Frankfort it passed up the west fork of the Vermillion river, in a north and north-east direction, a distance of fifteen miles, near Axtell, where it turned again upon its normal path and passed on near Sabetha, and into Richardson county, Nebraska. About fifty houses were blown down in the vicinity of Frankfort, and fifty families left destitute.

During the passage of the tornado neighbouring towns received more or less damage. Vermillion suffered slightly, some of the houses being started from their foundations.

Centralia, Blue Rapids, and Waterville received some damage.

I give some singular incidents of the tornado, as related on good authority:—

Mr. Fitch's son was blown across a ravine, over trees and fences, and landed unhurt on the door-step of a house half-a-mile distant. When asked how he came there, he replied, "I do not know!" Mr. Yawger found one of the wheels of his waggon 2½ miles from home. A tire was blown off a waggon wheel and straightened out as well as a blacksmith could do it. The body of a lady was driven into the ground head foremost, covering head and shoulders. A coat was torn from one man, divided in the centre, the halves being driven in opposite directions. Some of the fowls were picked clean of their feathers. On Snipe Creek, large elm, oak, walnut, cottonwood, and sycamore trees were twisted off and torn up by the roots, leaving a fine forest a scene of desolation.

The number of killed and injured have been variously estimated. At Irving and vicinity, thirteen were killed and fifty wounded. At Frankfort and vicinity, five were killed and forty injured. At Delphos and other places large numbers are reported killed and injured. The people along the route of the tornado were engaged in little else for several days but in burying the dead and caring for the wounded.* In Marshall county alone the damage to property and crops is estimated at 150,000 dollars.

On May 29, 1879, occurred a destructive tornado in Andrew and Nodaway counties, Missouri. A correspondent says, on that calendar day, at Bolckow, Andrew county, a hot wind blew from the north-west for several hours. The skies had been overcast from early morning with clouds, which frayed off and separated into light-coloured masses and drifted away, and finally dissolved in the atmosphere. About three o'clock clouds banked in the west and north-west in heavy black masses, and a current of hot air from the south-west appeared to hurl them in wild wreaths like battle clouds on one another. About four o'clock the citizens were startled by seeing a wild funnel-shaped cloud appear in the west, and sweep onward as if to doom their town to destruction. Its muffled thunder-roar was soon distinctly heard, and immediately it swept by the town upon

* Immediately after the occurrence of the Irving tornado, Governor St. John issued a proclamation calling on the people of Kansas to render assistance to the sufferers, heading the subscription himself with a liberal contribution, and all has been done that is possible to relieve the sufferers.

its destructive mission. The track of the tornado lay about three miles north-west of the town, and presented the looped and ragged edge, so often seen in tornadoes, on the south side. Observers say the tornado at times seemed to stop and take a backward turn, and then sweep on with renewed force. In this manner the tornado would destroy alternate pieces of property, leaving, perhaps, a house standing in the loop unscathed. The tornado passed about a mile north of Barnard, Nodaway county, and, when last observed, was moving toward Conception, Nodaway county. The violence of the tornado was very great; large trees, two and three feet in diameter, were twisted off, and the most substantial houses were lifted into the air and dashed in pieces. In one place it swept through a ravine, lifting out of its narrow walls every stick of timber, so that the bed of the ravine seemed to have been swept with fire. The northward tendency of this tornado in translation was probably due to surface currents.

Samuel W. Rhode, U.S.A., Sergeant of the Signal Corps, of Leavenworth, has kindly furnished notes, from the journal of that station, on the storm of the 29th of May, which are as follows:—

"This evening one of the most severe storms that has visited this section for several years past passed over this station. During the afternoon the sky was partially, and sometimes fully, covered with heavy, cumulus-stratus cloud, moving rapidly from the south-west. The wind was blowing briskly from the south, with a low and steadily falling barometer and rising temperature. At 5 p.m. a heavy dark mass of 'thunder heads' appeared on the north-west horizon, and gradually moved eastward, increasing in bulk and extending toward the zenith. Frequently, during the formation and development of the storm cloud, as many as four different currents in the air were indicated by the movement of the clouds. The 'cyclone,' or spiral, motion of the wind was plainly discernible in the clouds for over an hour before the force of the storm was felt at the surface. Rain could be seen falling to the north of the station for ten minutes before there was any precipitation here. It looked like vast and dense volumes of fog impelled eastward at a high velocity. At 6.35 p.m. a few scattering drops of rain began to fall. At that time the heaviest portion of the cloud was directly north of the station, the apex being about 60° above the horizon, the wind blowing in fitful gusts from the south. At 6.58 p.m. the barometer reached its lowest point, the actual reading (corrected for temperature and instrumental

error only) being 28·670. At 7 p.m. the wind suddenly backed from south to north and increased in force, blowing for about five minutes at the rate of 60 miles per hour. The cloud then moved rapidly southward, trending toward the east. The rain fell in perfect torrents for near an hour. From 7.10 to 7.40 p.m. considerable hail fell: a number of stones were measured and averaged from $\frac{1}{4}$ to $\frac{3}{8}$ of an inch in diameter. The electrical discharges were very intense and almost constant. The force of the storm expended itself about 8.5 p.m. The rain ceased at 8.15 p.m., and the amount which fell in one hour and forty minutes was one inch and forty-three one-hundredths.

"The storm produced no serious damage in this city or vicinity. A large number of trees in the city were blown down, and several large buildings suffered damage by lifting of roofs. Fruit trees and growing grain were somewhat damaged by the hail. The position of fallen trees, in different portions of the city, plainly indicated the spiral motion of the wind. The large iron bridge over the Missouri river at this point, on which was a train of heavily laden cars, swayed so much that the engineer jumped from his engine, thinking the bridge was toppling over.

"At about the same hour, a very destructive tornado struck the earth north of this station, near St. Joseph, Missouri. No doubt the disturbance felt here was an offshoot of the above mentioned tornado.

"During the evening, from 9 to 11 p.m., the electrical display in the south, south-east, and east was very beautiful and vivid. The heavens were almost continually illuminated. Frequently there were seen as high as a dozen streaks of lightning, of the zig-zag form, which seemed to radiate from a common centre."

Several theories have been advanced in reference to the causes of tornadoes. In the present article I have only space to discuss one of them.

Storms are supposed to find their origin, according to the Thermal theory, in an unstable equilibrium of the atmosphere due to solar heat. About three-fourths of the sun's rays pass through the atmosphere, and are absorbed by the surface of the earth. The envelope of the earth is thus heated mainly at the bottom, while it loses most of its heat by radiation at the top. As an increase of heat diminishes the density of the air, the envelope of the earth is in a constant state of unstable equilibrium. The upper and heavier strata of the atmosphere tend constantly to descend and force up the lower strata. Vertical currents are thus formed over greater

or less areas of the earth's surface by solar heat every day. The vapour contained in the air is thus carried up by the ascending current until it is condensed into cloud by the expansion and cooling of the air which contains it. The latent heat of the vapour thus liberated tends to elevate the temperature of the air containing it, causing it to be lifted to still higher altitudes. The violence of this upward movement is largely due to the amount of vapour, or the "steam power of the air."

The centre, or core, of a storm sometimes becomes a tornado, which possesses a linear or gyratory movement. Connected with these there is a swaying movement, caused by obstacles, such as hills and forests, along the path of the storm, and sometimes the tornado seems to be drawn up into the air.

I have noticed quite a number of whirlwinds observing the laws of tornadoes. In 1872 I saw a beautiful whirlwind at Burlington, Kansas. While walking toward the town from the north-west, I heard a rushing sound southwest of me. Soon I saw the prairie grass swaying violently, and I had a perfect view of a miniature tornado, as it passed across the road a few rods ahead of me. The atmosphere was tranquil at the time. Here was a whirlwind ploughing its way through a tranquil atmosphere at the rate of ten miles an hour, moving east by twenty degrees north, and gyrating in a direction contrary to the hands of a watch.

Tornadoes in the northern hemisphere are translated toward the east by about twenty degrees north. Deflections from this direction are supposed to be occasioned by local causes, such as rivers, surface currents, &c. The Irving tornado was deflected from its path near Frankfort, passing up the west fork of the Vermillion river, in a north and north-east direction, a distance of fifteen miles, when it again resumed its normal path. North of the equator, tornadoes revolve in a direction contrary to the sun, or the hands of a watch with its face upward, while south of the equator they revolve in an opposite direction.

Tornadoes, according to this theory, originate in disturbing causes acting suddenly, but take their character from the cosmical conditions of the globe. The average temperature of the globe is about 85° at the equator, decreasing to about zero at the poles. This excess of heat expands the air in the equatorial regions, which flows over toward each pole. The larger portion of this circulation, however, is confined to a belt not extending beyond the thirtieth parallel of latitude, because the current is cooled in the upper regions, and the meridians narrow toward the poles.

As the atmosphere is carried around with the earth in its daily revolution, the greater the elevation the greater the velocity eastward. A stream descending from the upper regions of the atmosphere would be impelled by its inertia when it reached the earth in an eastward direction. As parallels of latitude decrease in diameter from the equator to the poles, every parallel, going from the equator, revolves with less velocity than the preceding one. Should a portion of the atmosphere become saturated and unduly heated at the surface of the earth, it would be forced up by colder and heavier air. Currents of air would be formed blowing toward a common centre. In the northern hemisphere those from the north would naturally find the centre moving eastward with a superior velocity, and, falling behind, be projected toward the west, while those from the south would find the centre moving eastward with an inferior velocity, and be projected toward the east. The north half of the tornado would be impelled westward, and the south half eastward, establishing the whirl or vortex, which is a low barometer. The currents descending into the vortex on the south side having a greater impulse eastward, from the earth's rotation, than the impulse of the currents westward descending on the north side, the tornado would be deflected toward the north-east. The liberation of the latent heat by the condensation of the vapour would be the "steam power of the air" to drive up the ascending current; large amounts of electricity would be developed for destructive purposes, and the tornado would sweep on its path to overwhelm towns, crush forests, lick up rivers, and make the solid earth tremble.

The Thermal theory only uses heat, under cosmical conditions, as the *motive* power, leaving much of the phenomena to be accounted for by electricity and other destructive agencies.

Modesty should cover all theories of these awful but occult visitors like a garment. A few golden threads are in our scientific fingers, but the theoretic fabric for tornadoes is still largely to be woven. Do not let us imitate that great German scholar, who in his mature years found the works of his earlier life the hardest to answer.

There need not be any special alarm about tornadoes. Like great comets, they seldom appear, but on that account are the more noticeable. Destructive tornadoes only occur during dry seasons, and then, in this latitude, only on the last few days of May or on the first few days of June. The greatest tornado on record occurred June 3, 1860, the year of the drought. It passed over a large portion of Iowa and

Illinois, destroying Camanche, and killing fifty persons. That would not be an unusual railroad accident or ocean disaster. We stand a hundred chances of being killed in ordinary travel to one chance of being killed by a tornado. Still, we take our sleeping berth and fall into a quiet slumber. Tornadoes also occur just before evening, and, if we remember the law of their movement, we can generally escape them. Although, then, we may never be killed by a tornado, still it is always well to keep a clear conscience.—*Kansas City Review of Science and Industry.*

VI. THE PIRATE'S SHEET-ANCHOR.

THE common-sense doctrine that the right of an inventor to the creations of his own mind is the most natural and sacred of all kinds of property, though substantially self-evident, is far from meeting universal acceptance. That our ancestors in their ignorance ranked copy-right and patent-right among "monopolies"—just as they classed the whale among fishes, and confounded astronomy with astrology—is an unfortunate circumstance, which cannot, it seems, be forgotten. In a political economist the very word brings on a state of fury bordering on madness. Call any institution, right, or possession a "monopoly," and he strives to destroy it without further question.

For the benefit of persons capable of judging soberly and righteously, it may be well to examine the ostensible grounds on which pirates rely when they proclaim patent-right not property, but a mere privilege.

Their first argument is one which it is simply astonishing, and even painful, to hear from any man of mature age and good education. It is in substance this, that as an invention is often sought after or aimed at, and sometimes even attained, by several persons simultaneously, it cannot, when completed, be considered the property of any person! That we may not be accused of unfairness I will use the very words of the piratical school, as given in the "Dictionary of Science, Literature, and Art," by W. T. Brande and Rev. G. W. Cox (article "Patents," vol. ii., p. 834). The article in question appears to be from the pen of Professor Thorold Rogers, though we have no right to assume that the views expounded are his own, since he introduces them

with the words "It is alleged." We are there told that "simultaneous discovery is the rule, independent invention the rare exception; that, in fact, a patent is ordinarily assigned to the mere accident of priority, to the detriment of others who have as effectively found out the power or the process appropriated."

Mr. R. A. Macfie, in his recent work "Copyright and Patents for Inventions" (Preface, v. and vi.), writes—"An invention may be, and commonly is, originated by a plurality of persons in complete independence and ignorance one of another, and of what each other does or has done."

The present writer cannot for one moment admit the validity, or even the relevance, of this contention. The champions of piracy admit, as we see, that in a minority of cases, at any rate, an invention originates from a single individual exclusively, no other person having been searching or experimenting in the same direction. In such cases, then, the argument entirely fails, and the invention, for anything our opponents have yet advanced, must at once be classed as property.

But further, the assertion that "simultaneous discovery is the rule" must be termed a grave exaggeration, improbable on *a priori* grounds, and questionable as regards the evidence upon which it rests. If we consider what multitudes of known and admitted desiderata exist in addition to the vastly greater number of instances where the room for an invention exists unrecognised; if we reflect, moreover, that the solution of each such industrial problem, chemical or mechanical, may be attempted or even effected by a plurality of methods, we shall see that there is a great antecedent improbability in the notion of a number of independent inventors simultaneously originating the very same novelty. We are well aware that when a certain want or difficulty is felt, inventive minds will naturally turn their efforts in that direction. But as a rule, in as far as they are *bonâ fide* inventors, though in pursuit of the same object they will proceed by different ways. Of this truth the history of modern invention furnishes some striking instances. The beautiful dye known as "malachite green" has been obtained simultaneously, or nearly so, by two distinct chemists "in complete independence, and perhaps ignorance, one of another," and on superficial examination this case might seem to tell in favour of Mr. Macfie and his friends. But if we look more closely into the matter we shall see that Doebner, of Berlin, prepares his malachite green by the joint action of dimethyl aniline, chloride of zinc, and benzo-trichloride. Fischer, of

Munich, on the other hand, obtains the same dye by the reaction of dimethyl-aniline, chloride of zinc, and the oil of bitter almonds. Thus when rightly considered the inventions of these two chemists, though relating to the same substance, were distinct.

We may take another signal instance, which though belonging to the sphere of scientific discovery rather than technical industrial invention, still illustrates the same principle.

Not very long ago two eminent physicists, M. Cailletet and M. Pictet, succeeded almost simultaneously in condensing into a liquid state the permanent gases formerly so considered. But here, again, though the end gained was identical, the means employed were distinct. In this manner close and intelligent scrutiny will be found to dispose of a very large proportion of the alleged cases of simultaneous invention.

In another very numerous class of cases the simultaneity, or rather the invention altogether, is doubtful or spurious. There are a certain class of persons addicted to vague and aimless experimentation, whether chemical, physical, or mechanical, on any subject that engages public attention. Twenty years ago they were "messing about" with aniline, napthaline, and phenol, and obtaining products which they had not the skill and the perseverance to purify. Five years ago they were deep in the mystery of roller-skates, and now the electric light has dazzled their minds with its brilliance and its future. When a real improvement is originated in the matter with which such muddlers are engaged, it is very easy for them to say, and perhaps to think, that they too had tried the very reaction or combination in question, and in a few more weeks' or months' time they could have brought their ideas to perfection. It is hard to disprove such assertions. Oftentimes a claimant of this stamp can even prove that at the time in question he was working with such or such substances. But all this generally falls far short of a demonstration that an invention identical with one just claimed in a patent has been independently and distinctly originated by some other experimentator. It is very significant that these multiple or reiterated inventions arise only after some man of merit has paved the way. When Mr. Perkin brought out his original aniline violet no rival inventor complained of having been beaten by a neck. A few years later every fresh aniline dye became a bone of contention between candidates for priority.

Summing up the above considerations we find, therefore

that one class of inventions is confessedly single in its origin; that in another the simultaneity of origination is only apparent; and that in a third it is spurious or doubtful. Thus, contrary to the view stated by Professor Rogers, a mere exceptional minority remains, where, save in point of time, a second person has an equal claim to the authorship of the invention. But even as regards this minority, the argument under examination proves nothing. Let us suppose that two chemists had independently and simultaneously effected the combination of atmospheric nitrogen with hydrogen so as to produce ammonia and ammoniacal salts at a greatly reduced cost. Let us further assume that each had arrived at this result by exactly the same process. In such a case the honour would fall equally to both. The beneficial interest in the invention, in default of omniscient patent-commissioners, falls to the one who first took date by petitioning for letters patent. This arrangement may not agree with the dictates of ideal equity, but is probably as near an approximation as is possible in human institutions. That A has completed an invention in January, and that B has independently originated the same invention in February, can certainly never justify C—the pirate, or idea stealer—in saying that it is therefore the property of neither. The "accident of priority," if accident it can be legitimately termed, is in many cases the very point upon which admitted property hinges. The mere fact that E is the eldest of the sons of D ensures his succession on the death of D to the real estate of the latter. Nor is the existence of primogeniture ever advanced as a reason why land should not be regarded as property.

In the case of islands or regions previously unknown, priority of discovery, registered by hoisting the national flag, has always been considered as giving the title to possession. Suppose that two ships, commissioned by different powers, were each in turn to discover one and the same guano island, no one would contend that the second discoverer was in any way "prejudiced" or entitled to call in question the right of his anticipator. Still less would it be argued that, because the island had been independently discovered by the vessels of two powers, it could become the property of neither.

A case still more closely analogous is afforded by the mining laws and customs of certain districts. Two or more persons may independently and almost simultaneously have become aware of the existence of an unclaimed and unwrought vein of tin or copper ore. But it becomes the pro-

perty, subject to the payment of certain dues, of the man who first breaks ground. No one contends that his right is "to the prejudice" of the second discoverer, or blames him as a "monopolist," yet the mine is less truly the property of the finder than is the invention of its originator. The vein of ore had a prior existence, while the invention had not. These considerations will, we think, satisfy every disinterested man of the utter futility of the piratical argument drawn from the alleged simultaneity of invention.

Another ground brought forward for denying to inventors the character of property has been expressed as follows:—"What he (the landowner, or the fundholder, or, we suppose, the successful gambler) occupies, is something tangible and limited which *can* belong to him without his requiring to go out of his way or into any other person's way in order to watch it, work it, and profit by it. *True property* is manifestly a right to an existing thing, visible and palpable, that only a single person, or at the most a limited number of persons, can hold, occupy, employ and enjoy."

Here again the attempt to establish a difference between patent-right and property utterly fails. A patent, just like an estate, is generally in the hands of one person, and, just as the landowner can sell or let the whole of, or certain fractions, of his domain, so the patentee can assign or let the whole of his interest to one person or to a number. There is the closest analogy between the two cases.

Again, we admit that a patent is in itself nothing "visible, tangible, or palpable." But the same may be said of various kinds of recognised property. A. B. holds, for instance, ten shares in a manufacturing or banking company, or a thousand pound's worth of consols or of Egyptian bonds; where in these cases is the "existing thing visible and palpable?"

If this "existing thing" be the share certificates, they are simply analogous to the patentee's official document. The patentee, no more and no less than the landowner or fundholder, has to "go out of his way or into any other person's way" to watch, work, or profit by his possession. Other persons and their ways the inventor leaves as he finds them, except they wilfully and knowingly come out of such ways in order to trespass upon his novel creation. It is sometimes, with admirable simplicity, contended that patent-right is a privilege in restraint of trade. This is what every thief might say of the institution of property; it restrains him, to his great annoyance, from carrying off and selling his neighbour's goods and chattels.

There may be sometimes greater difficulty in defining the

rights of a patentee than those of a landowner, though the boundaries, water-rights, and easements of estates have been a fruitful source of litigation. But if we, on that account, decline to recognise property in invention, we follow the example of a certain examinee—we will not say student—who, being unable to classify certain mineralogical specimens, got over the difficulty by throwing them away.

If the advocates of free-booting still refuse to recognise the property of the inventor in his creation, they should at least produce some more pertinent arguments in defence of their position. It is mainly by priority in invention that England's old industrial supremacy was gained, and if it is to be retained, or rather re-conquered, invention alone can point the way. A Bessemer, a Siemens, or a Perkin, is of more value to the country than all the professors of political economy and all the chambers of commerce that ever existed.

VII. THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

Sheffield Meeting.

THE Meeting of the British Association at Sheffield, which closed on the 28th ult., though not so largely attended as the Dublin Meeting of last year, proved a very successful one. The arrangements of the Local Committee, of which Dr. H. C. Sorby, F.R.S., and Mr. J. F. Moss were the Secretaries, gave great satisfaction, and never has the Association been received with greater hospitality than by the Mayor and inhabitants of Sheffield.

At the concluding meeting it was announced that the grants of money appropriated to scientific purposes by the General Committee amounted to £960. The following is a list of the grants:—A. Mathematics and Physics: Dr. Lodge, new form of high insulation key, £10; Prof. Adams, standard of white light, £20; Prof. Everett, underground temperature, £10; Dr. Joule, determination of the mechanical equivalent of heat, £50; Sir W. Thomson, elasticity of wire, £50; Mr. Glaisher, luminous meteors, £30; Mr. G. H. Darwin, lunar disturbance of gravity, £30; Prof. Sylvester, fundamental invariants, £50; Mr. J. Perry, laws

of water friction, £30; Mr. W. E. Ayrton, specific inductive capacity of Sprengel vacuum, £20; Rev. Prof. Haughton, completion of tables of sun-heat co-efficients, £50; Prof. G. Forbes, instrument for detection of fire-damp in mines, £10; Mr. J. M. Thompson, inductive capacity of crystals and paraffines, £25. B. Chemistry: Dr. Dewar, spectral analysis, £10; Dr. Wallace, development of light from coal gas, £10. C. Geology; Prof. Duncan, R.M., report on carboniferous polyzoa, £10; Prof. A. L. Adam, caves of South Ireland, £10; Prof. Seeley, viviparous nature of ichthyosaurus, £10; Mr. John Evans, Kent's cavern exploration, £50; Mr. John Evans, geological record, £100; Prof. W. C. Williamson, miocene flora of the basalt of North Ireland, £15; Prof. Hull, underground waters of Permian formations, £5. D. Biology; Dr. Pye Smith, elimination of nitrogen by bodily exercise, £50; Mr. Lane Fox, general anthropological notes, £20; Mr. Stainton, record of zoological literature, £100; Dr. M. Foster, table at zoological station at Naples, £75; Dr. A. Gamgee, investigation of the geology and zoology of Mexico, £50; Sir J. Lubbock, excavations at Port Stewart, £15. F. Statistics and Economical Science: Dr. Farr, anthropometry, £50. G. Mechanics: Mr. Bramwell, patent laws, £50. Total, £960.

On the motion of Mr. Sclater, seconded by Prof. Ramsay, it was resolved that the Council be authorised to take such further action, having regard to the correspondence with the Treasury as to the natural history collection, as they may deem necessary in the interests of science.

The following is an abstract of the Inaugural Address of the President, Prof. G. J. ALLMAN, M.D., LL.D., F.R.S., &c.:—

The PRESIDENT selected as his subject the most generalised expression of living matter, and gave an account of the results of the latest researches into its nature and properties. Some forty years ago, he said, Dujardin pointed out that the bodies of some of the lowest animals consist of a structureless semi-fluid, but contractile substance, which he named Sarcode. Mohl afterwards detected and studied a similar substance in the cells of plants, which he called Protoplasm. Schultze subsequently demonstrated that these two substances, the sarcode of animals and the protoplasm of plants, were identical. Recent research has not merely confirmed this conclusion of Schultze, but has shown that protoplasm lies at the root of all vital phenomena, whether animal or vegetable, constituting, as Huxley expresses it, the "physical basis of life." Wherever there is life there is

protoplasm, and conversely wherever there is protoplasm there is life.

Co-extensive with the whole of organic nature—every vital act being referable to some mode or property of protoplasm—it becomes to the biologist what "ether" is to the physicist, only that instead of being a mere hypothetical conception, accepted as a reality merely from its adequacy in explaining phenomena, it is a reality, visible and tangible.

The chemical nature of protoplasm is very complex, and not yet fully determined. It is, however, a combination of albuminoid bodies, its main elements being oxygen, carbon, hydrogen, and nitrogen. In its typical state it is a tenacious glairy liquid, in consistence something like the white of unboiled egg. Under the microscope it displays movements. Waves traverse its surface, or it is seen to flow away in streams of the most varied characters, not only in accordance with gravity, but in directions utterly opposed to gravitation. All these movements take place without any obvious impulse from without which might send ripples over its surface or set streamlets flowing from its margin. These phenomena are such as we never meet with in a simply physical fluid; they are spontaneous movements due to its proper irritability, to its essential constitution as living matter. On still closer examination it is found, if not absolutely homogeneous, still totally destitute of structure. It is a living liquid, which, though organless, manifests the essential phenomena of life.

Such is protoplasm in its most generalised aspect. The speaker then proceeded to give some examples of protoplasm as actually met with in Nature. He described the far-famed *Bathybius*, first dredged up in the North Atlantic by the naturalists of the exploring ship *Porcupine*, from depths of 5000 to 25,000 feet. This substance has been examined by Huxley and Haeckel, who pronounce it to be living protoplasm in its simplest and most primitive condition. On the other hand, the *Challenger* explorers met with no traces of this living matter,—for it can scarcely be called a living being,—and they conclude that it is merely a deposit of sulphate of lime, inorganic, and of course inanimate. Bessels, however, the naturalist of the *Polaris*, confirms the views of Huxley and Haeckel, and states that he dredged up from the Greenland Seas masses of protoplasm, living, but undifferentiated. Further research is here, therefore, required.

As a further and indisputable form of protoplasm we have *Protamœba primitiva*, little living lumps which multiply by spontaneous division.

A little higher is the *Amœba* of our pools and gutters, a being without definite shape, perpetually changing its form, throwing out and drawing in thick lobes and finger-like "false feet," in which its body seems to flow over the field of the microscope. It is no longer a mere homogeneous particle of protoplasm, like

the *Protamœba*. Towards its centre a small globular mass of firmer matter has become differentiated from the remainder, forming a nucleus, while the protoplasm constituting the extreme outer boundary differs slightly from the rest, being more transparent, destitute of granules, and apparently somewhat firmer than the interior. We may also notice that at one spot a clear spherical space has made its appearance. On watching we see it suddenly contract and vanish, and after a few seconds it dilates, comes into view, and again disappears,—all this in regular rhthymic sequence. This pulsating cavity is the "contractile vacuole." In the *Amœba* we have the essential characters of a *cell*, the morphological unit of organisation and the physiological source of specialised function. The term "cell" is, however, somewhat misleading; it denotes merely a definite mass of protoplasm containing a nucleus, which may or may not assume the form of a vesicle. To the non-nucleated forms of life, such as *Protamœba*, Haeckel assigns, in distinction, the name Cytode. Many aquatic beings beside the *Amœba* never pass beyond the condition of a simple cell, in which reside the whole of the properties which manifest themselves in the vital phenomena of the organism. As we pass from these lowest forms to higher we find cell added to cell, until many millions of such units become associated in a single organism, where each cell or group of cells has its special work, while all combine for the welfare and unity of the whole. Still even in man the component cells are far from losing their individuality. The colourless blood-corpuscules retain most of the characters of the *Amœba*.

The animal egg, which in its young state forms an element in the structure of the parent organism, is a true cell, consisting of a lump of protoplasm enclosing a nucleus, and having in the interior of this a nucleolus. Whilst still very young it has no constant form, and may, like an *Amœba*, wander about by the aid of its pseudopodial projections. The life of an organism is made up of the lives of its component cells, and here we find most distinctly expressed the great law of the physiological division of labour. In the lowest unicellular organisms the performance of all the processes which constitute its life must devolve on the protoplasm of this one cell; but as we pass to more highly organised beings the work is distributed among a number of workers,—to wit, the cells which make up the complex organism. No cell, however great may be the differentiation of function in the organism, can dispense with its irritability, the one constant and essential property of every living cell. In very many instances the protoplasm becomes confined within resisting walls, composed in plants of cellulose, still losing none of its activity, as is manifest in the Characeæ, in *Valisneria spiralis*, and even in higher plants. Even in these higher plants, further, truly naked protoplasm still occurs, as has recently been shown by Mr. F. Darwin in the teazel (*Dipsacus*)—a phenome-

non probably connected with the absorption of nitrogenous matter.

That there is no essential difference between the protoplasm of plants and of animals is rendered plain by a number of motor phenomena which we have been in the habit of erroneously regarding as peculiar to animals. All living protoplasm alike possesses the property of resisting the entrance of colouring-matter into its substance, though as soon as dead it can be coloured throughout.

Recent researches on the division of animal cells show how close is the agreement between plants and animals in all the leading phenomena of cell-division, and afford further proof of the essential unity of the two great organic kingdoms. The egg, as a typical cell, distinguishable by no tangible characters from thousands of other cells, is nevertheless destined to run through a definite series of developmental changes which have as their end the building up of an organism like that to which the egg owes its origin. It is obvious that such complex organisations as thus result can be derived from the egg-cell only by a process of cell-multiplication. The birth of new cells derived from the primary cell or egg thus lies at the basis of embryonic development. It is here that the phenomena of cell-multiplication in the animal kingdom can be most successfully observed.

The President then briefly described the so-called "cleavage" of the egg, which is merely a multiplication of the egg-cell by repeated division. The egg has its protoplasm and its nucleus, and is, as a rule, enveloped in a delicate membrane. The protoplasm forms the vitellus or yolk, and the surrounding membrane is called the vitellary membrane. The division about to take place is introduced by a change of form in the nucleus, which assumes the shape of a spindle precisely as in the cell-division of plants. On each pole of the spindle transparent protoplasm collects, forming here a clear spherical area. Each pole of the spindle has become the centre of a system of rays, which stream out in all directions into the surrounding protoplasm. The protoplasm thus shows enveloped in its mass two sun-like figures, whose centres are connected to each other by the spindle-shaped nucleus. To this, with the sun-like rays streaming from its poles, Auerbach gives the name of "Karyolitic figure." A phenomenon very similar to what occurs in cell division among plants now shows itself. The nucleus becomes broken up into a number of filaments which lie together in a bundle, each filament stretching from pole to pole of the spindle. In its central point each filament shows a knot-like enlargement, and from the close approximation of the knots there results a thick zone of protoplasm in the equator of the spindle. Each knot soon divides into two halves, and each half recedes from the equator and travels along the filament towards its extremity. When arrived at the poles of the spindle each set of half knots

coalesces into a globular body, while the intervening portion of the spindle gradually disappears. Instead of the single fusiform nucleus whose changes have thus been traced, we have two new globular nuclei. The egg now begins to divide along a plane at right angles to a line connecting the two nuclei. The division is introduced by a constriction of its protoplasm commencing at the circumference just within the vitelline membrane, and extending towards the centre, divides the whole mass of protoplasm into halves, each including within it one of the new nuclei. Each of these two young cells divides in its turn in a direction at right angles to the first division-plane, while by continued repetition of the same act the whole of the protoplasm or yolk becomes broken up into a vast multitude of cells, and the unicellular organism—the egg with which we began our history—has become converted into an organism composed of many thousands of cells.

In the greater number of plants the protoplasm of most of the cells exposed to sunlight undergoes an important differentiation, part of it becoming separated from the remainder in the form of green granules known as chlorophyll granules. These consist of true protoplasm, as the green colouring-matter may be extracted, leaving behind the colourless protoplasmic base. That chlorophyll is a living substance is sufficiently obvious. On the cells which contain it devolves the faculty of decomposing carbonic acid. On this depends the assimilation of plants—a process externally manifested by the exhalation of oxygen, which occurs under the influence of light. This action of chlorophyll in bringing about the decomposition of carbonic acid is not, as was till recently believed, confined to plants. In the green *Hydra*, and in certain green Planariæ, chlorophyll is differentiated in their protoplasm, and probably always acts here under the influence of light exactly as in plants. Geddes has shown that these animals contain starch-grains in their tissues—another striking point of resemblance between them and plants. A further approximation between the two organic kingdoms has been shown by the researches of Mr. Darwin and others on carnivorous plants. Naegeli has further proved that the cell of the yeast fungus contains peptin, a substance formerly known solely as a product of the digestion of nitrogenous food of animals.

Nothing shows more strikingly the identity of the protoplasm in plants and animals than the fact that plants may be placed, like animals, under the influence of anæsthetics. Plants while under this influence absorb oxygen and exhale carbonic acid, the true *respiratory* function which is ordinarily masked by the predominant function of *assimilation*, which devolves on the green cells of plants, and which manifests itself under the influence of light in the absorption of carbonic acid and the exhalation of oxygen. Hence the respiration of living beings is identical whether manifested in the plant or the animal, and the

supposed antagonism in its respective character is totally demolished.

Enough has been said to show that in protoplasm we find the only form of matter in which life can manifest itself. We are thus led, said Prof. Allman, to the conception of an essential unity in Organic Nature,—a structural unity in as far as every living being has protoplasm as the essential matter of every living element of its structure, and a physiological unity in the universal attribute of irritability, which has its seat in this same protoplasm, and is the prime mover of every phenomenon of life. Mere form has little to do with the essential properties of protoplasm. To suppose that all protoplasm is identical where no difference can be detected by the means at our disposal would be an error. Of two particles of protoplasm between which we can detect no difference, one can develop only to a jelly-fish, the other only to a man; and one conclusion alone is possible—that deep within them there must be a fundamental difference which thus determines their destiny. Of this we know nothing, and can assert nothing beyond the statement that it must depend on their hidden constitution. In the molecular condition of protoplasm there is probably as much complexity as in the disposition of organs in the most highly differentiated organisms. Herein lies the many-sidedness of protoplasm, and its significance as the basis of all morphological expression, as the agent of all physiological work. From the facts which have been here briefly noticed there is but one legitimate conclusion—that life is a property of protoplasm. The essential phenomena of living beings are not so widely separated from the phenomena of lifeless matter as to render it impossible to recognise an analogy between them. Even irritability, the one grand character of living beings, is not more difficult to be conceived of as a property of matter than the physical phenomena of radial energy. It is quite true that between lifeless and living matter there is a difference greater far than any which can be found between the most diverse manifestations of lifeless matter. No one has ever yet built up one particle of living matter out of lifeless elements. Every living creature has its origin in pre-existent living matter; the protoplasm of to-day is but the continuation of the protoplasm of past ages. When we say that life is a property of protoplasm we assert as much as we are justified in doing. We stand upon the boundary between life in its proper conception, as a group of phenomena having irritability as their common bond, and that other group of phenomena which we call consciousness or thought, and which, however closely linked with those of life, are yet essentially distinct from them. When a thought passes through the mind it is associated, as we have reason for believing, with some change in the protoplasm of the cerebral cells. Are we therefore justified in regarding thought as a property of the protoplasm of these cells, in the sense in which we regard mus-

cular contraction as a property of the protoplasm of the muscle? or is it really a property residing in something far different, but which may yet need for its manifestation the activity of cerebral protoplasm?

If, said the PRESIDENT, we could see any analogy between thought and any one of the admitted phenomena of matter, we should be bound to accept the first of these conclusions as the simplest, and as affording a hypothesis most in accordance with the comprehensiveness of natural laws. But between thought and the phenomena of matter there is not only no analogy, but there is no conceivable analogy, and the path we have hitherto followed comes to an end. The chasm between unconscious life and thought is impassable; for even from irritability, to which on a superficial view consciousness may seem related, it is as absolutely distinct as it is from the ordinary phenomena of matter. It has been argued that because physiological activity must be a property of every living cell, psychical activity must be equally so; the language of the metaphysician has been carried into biology, and the "cell-soul" spoken of as a conception inseparable from life. How far back in the scale of life consciousness may exist, we have no means of determining. But even admitting that every living cell were a conscious, thinking being, are we therefore justified in asserting that its consciousness, like its irritability, is a property of the matter of which it is composed? The sole argument on which this view rests is that from analogy, and as there is an absence of all analogy between the things compared it must fall to the ground.

Prof. Huxley, in his lecture on the "Physical Basis of Life," contended that no difference, however great, between the phenomena of living matter and those of the lifeless elements of which such matter is composed, should militate against our ascribing to protoplasm the phenomena of life as properties essentially inherent in it; since we know that the result of a combination of physical elements may exhibit physical properties totally different from those of such elements, the physical phenomena presented by water having, *e.g.*, no resemblance to those of oxygen and hydrogen, As regards the phenomena of life in the stricter sense of the word, the argument is conclusive. But if it be pushed further and extended to consciousness it loses all its force.

That consciousness is never manifested except in the presence of cerebral matter or of something like it, there cannot be a question; but this is a very different thing from its being a property of such matter in the sense in which polarity is a property of the magnet, or irritability of protoplasm. The generation cf the rays which lie invisible beyond the violet in the spectrum of the sun cannot be regarded as a property of the medium which by changing their refrangibility can alone render them apparent.

I know that there is a special charm in those broad generalisa-

tions which would refer many very different phenomena to a common source. But in this very charm there is undoubtedly a danger, and we must be all the more careful lest it should exert an influence in arresting the progress of truth, just as at an earlier period traditional beliefs exerted an authority from which the mind but slowly and with difficulty succeeded in emancipating itself.

But have we, it may be asked, made in all this one step forward towards an explanation of the phenomena of consciousness or the discovery of its source? Assuredly not. The power of conceiving of a substance different from that of matter is still beyond the limits of human intelligence, and the physical or objective conditions which are the concomitants of thought are the only ones of which it is possible to know anything, and the only ones whose study is of value.

We are not, however, on that account forced to the conclusion that there is nothing in the universe but matter and force. The simplest physical law is absolutely inconceivable by the highest of the brutes, and no one would be justified in assuming that man had already attained the limit of his powers. Whatever may be that mysterious bond which connects organisation with psychical endowments, the one grand fact—a fact of inestimable importance—stands out clear and freed from all obscurity and doubt, that from the first dawn of intelligence there is with every advance in organisation a corresponding advance in mind. Mind as well as body is thus travelling onwards through higher and still higher phases; the great law of Evolution is shaping the destiny of our race; and though we may at most but indicate some weak point in the generalisation which would refer consciousness as well as life to a common material source, who can say that in the far-off future there may not yet be evolved other and higher faculties from which light may stream in upon the darkness, and reveal to man the great mystery of Thought?

The first Evening Discourse was by Mr. Crookes, F.R.S., "On Radiant Matter:" the second by Prof. Ray Lankester, F.R.S., "On Degeneration;" while the Saturday evening Lecture to Working Men was delivered by Mr. W. E. Ayrton, the subject being "The Transmission of Power by Electricity."

Mr. Crookes's discourse was illustrated by experiments on the movements of molecules in high vacua, some of which were recently described in the "Monthly Journal of Science." According to the best authorities, a bulb 13·5 centimetres in diameter contains more than 1,000000,000000,000000,000000 (a quadrillion) molecules. Mr. Crookes showed that when exhausted to a millionth of an atmosphere there are still a

trillion molecules left in the bulb. To suggest some idea of this vast number he perforated an exhausted bulb by a spark from the induction coil. The spark produced a hole of microscopical fineness, yet sufficient to allow molecules to penetrate and to destroy the vacuum. The inrush of air impinged against the vanes and set them rotating after the manner of a windmill. "Let us suppose," said Mr. Crookes, "the molecules to be of such a size that at every second of time a hundred millions could enter. How long, think you, would it take for this small vessel to get full of air? An hour? A day? A year? A century? Nay, almost an eternity! A time so enormous that imagination itself cannot grasp the reality. Supposing this exhausted glass bulb, indued with indestructibility, had been pierced at the birth of the solar system; supposing it to have been present when the earth was without form and void; supposing it to have borne witness to all the stupendous changes evolved during the full cycles of geologic time, to have seen the first living creature appear, and the last man disappear; supposing it to survive until the fulfilment of the mathematicians' prediction that the Sun, the source of energy, four million centuries from its formation will ultimately become a burnt-out cinder; supposing all this,—at the rate of filling I have just described, 100 million molecules a second,—this little bulb even then would scarcely have admitted its full quadrillion of molecules.* But what will you say if I tell you that all these molecules, this quadrillion of molecules, will enter through the microscopic hole before you leave this room? The hole being unaltered in size, the number of molecules undiminished, this apparent paradox can only be explained by again supposing the size of the molecules to be diminished almost infinitely—so that instead of entering at the rate of 100 millions every second, they troop in at a rate of something like 300 trillions a second. I have done the sum, but figures when they mount so high cease to have

* Mr. Johnstone Stoney has shown (Phil. Mag., vol. xxxvi., p. 141) that 1 c.c. of air contains about 1000,000000,000000,000000 molecules. Therefore a bulb 13·5 centims. diameter contains $13 \cdot 5^3 \times 0 \cdot 5236 \times 1000,000000,000000,000000$ or 1,288252,350000,000000,000000 molecules of air at the ordinary pressure. Therefore the bulb when exhausted to the millionth of an atmosphere contains 1,288252,350000,000000 molecules, leaving 1,288251,061747,650000,000000 molecules to enter through the perforation. At the rate of 100,000000 molecules a second, the time required for them all to enter will be

12882,510617,476500 seconds, or
214,708510,291275 minutes, or
3,578475,171521 hours, or
149103,132147 days, or
408,501731 years.

any meaning, and such calculations are as futile as trying to count the drops in the ocean." Mr. Crookes concluded his lecture by remarking that at length we seemed to have within our grasp and obedient to our control the little indivisible particles which with good warrant are supposed to constitute the physical basis of the universe. We had seen that in some of its properties Radiant Matter was as material as the table, whilst in other properties it almost assumed the character of Radiant Energy. We had actually touched the border land where Matter and Force seem to merge into one another, the shadowy realm between Known and Unknown which for him had always had peculiar temptations. He ventured to think that the greatest scientific problems of the future would find their solution in this Border Land, and even beyond; here, it seemed to him, lay Ultimate Realities, subtle, far-reaching, wonderful.

The Mathematical and Physical Section was presided over by Mr. G. JOHNSTONE STONEY, M.A., F.R.S.

In his presidential address Mr. STONEY sought to show that in the study of mechanics and chemistry the two great methods of investigation, viz., the deductive and the experimental, could best be acquired, and that for a sound grasp of the remaining physical sciences, and especially with a view to further advance in physical science, a command of both methods of investigation is essential. He said that, in order to understand the present position of Natural Science upon the earth, we must remember that the universe is in itself one great whole, which includes minds no less than bodies, for thought is as much a phenomenon of what really exists as motion. But though the universe be but one, man with his limited powers is unable to treat it as such, but has to push his investigation of Nature when and where he can. Thus have arisen many sciences which were at first quite isolated. Their separate condition is a mark of the feebleness of our powers of investigation. Their gradual convergence, and especially where any complete contact can be established between them, is the mark that our advancing knowledge is penetrating deeper.

In the present passing condition of our knowledge then there is one group of sciences which investigate the phenomena of consciousness; another distinct group of the biological sciences; and a third, the group of the physical sciences. These are all but parts of the one great investigation of Nature, but for the present they exist almost disconnected, as separate provinces of human inquiry.

When we endeavour to investigate mental phenomena, we are

encountered by the complexity and remoteness of the effects which present themselves for examination, and by a deep and unpenetrated obscurity hanging over the interval between them and their causes. In order to make any progress even in the subordinate task of tracing out the relations of these effects to one another, the inquirer finds it necessary to venture upon hypothesis, and in all metaphysical speculation we sadly miss that healthy discipline with which Nature in other branches of science relentlessly refutes our hypotheses if they are wrong. Here, then, is a region in which the plausible may be mistaken for the true; and it is unfortunately certain that it has sometimes been so mistaken by the ablest human minds.

The biological sciences treat of all the phenomena of living beings except their mental phenomena, which are those which lie most remote from their causes. Here the complication is less, but it is still too great for the human mind to have yet penetrated behind it. We are still occupied with phenomena which lie at a great distance from their real causes. We are accordingly still far beyond the range of the exact sciences. Most of the great discoveries of biological science have been made by estimating the *general drift* of what is taught by a vast number of particular facts. This, it will be observed, is a kind of reasoning that is necessarily more or less inexact, and, as a consequence, it is one which requires wide intellectual training and great experience and tact to handle it with safety. When the investigator has brought these qualifications to his task, astonishing progress has been made in these sciences: without them the reasoning may degrade into being either trivial or loose.

In the rest of the study of Nature we are not embarrassed by the phenomena of life, and many mysteries therefore stand aside out of our path. Here lies the domain of the physical sciences. It is here that the mind of man has best been able to cope with the realities of the universe, and in which its greatest achievements have been effected. It is here that exact reasoning finds a predominant place.

In meteorology, owing to the complication of the materials that have to be dealt with, we must have frequent recourse to the same kind of reasoning as has been found so effectual in the biological sciences; but in the other physical sciences exact reasoning prevails, and on this account they are frequently classed together as the exact sciences.

The process of investigation in the exact sciences is fundamentally one in all cases. It has been well described by Mill in the third book of his "Logic." Nevertheless, it is notorious that minds which are well fitted for some branches of physical inquiry find difficulty—sometimes insuperable difficulty—in pursuing others. It is not every eminent mathematician who would have made an equally good chemist, or *vice versa*. This

is because there exists a practical distinction separating the investigation of exact science into two well-marked classes when they are viewed, not as they are in themselves, but in their relation to the powers of us human beings. All valid investigations in exact science appeal to what can be directly perceived, and all lead to a conclusion which can be reasoned out from it; but there are some of these investigations in which the main difficulty consists in making the appeal to the senses, and there are others in which the main difficulty lies in the process of reasoning.

To contend with these difficulties successfully requires very different qualities of mind and body. In experimental science the powers principally called into requisition are readiness and closeness of observation, dexterity in manipulation, skill in devising expedients, accuracy in making adjustments, and great patience. It also requires that the investigator should have an accurate memory of what else he has witnessed resembling the phenomenon under observation, that he should be quick to detect every point of agreement and difference that can be perceived, and be skilful to select those which are significant, and to employ them as materials for provision to guide his further proceedings. But the strain on the reasoning powers is generally less, often of trifling amount. The question is put to Nature, and it is Nature usually that gives the bulk of the answer. The most striking monument of splendid achievements by the experimental method of investigation unaided by the deductive method is to be found in the science of chemistry.

An equally typical instance of the power of the deductive method is the science of mechanics. This science, which has sunk deeper into the secrets of Nature than any other science, and which is the science towards whom all other physical sciences are at present more or less gravitating, is essentially deductive. There is little or no difficulty about its fundamental data. They are facts of Nature so patent to all men, and so indelibly implanted in human conception, that some persons have supposed that we have an intuitive perception of them. But, while the materials from which the mind is to work are thus easily obtained, it has taxed to the utmost the reasoning powers of understandings like Newton's to evolve the few consequences of them which are already known, and the investigator has to call to his assistance every aid to prolonged consecutive thought which mathematicians can devise.

No reach of intellect applied to the materials in existence before 1860 could have elicited the fact that iron exists upon the sun. This great discovery was made by Professor Kirchhoff, a scientific man who was equally versed in both methods of investigation. It was the experimental method he employed. Kirchhoff's great merit and the real difficulty of his work lay in

the scientific foresight and the industry which were required to frame hypotheses that were worth testing, to guide the investigation by these hypotheses, to contrive, construct, and adjust adequate apparatus, and to make with it the elaborate observations and the exact observations and maps which were necessary. But when by these means the new facts had been brought to light, the inference from them that there is iron in the atmosphere of the sun was an easy one. This example will better convey than a definition what are the characteristic features of an experimental inquiry.

On the other hand, no series of observations or experiments, however skilfully arranged, could have enabled anyone to understand the cause of that familar but truly surprising phenomenon that a top stands upon its peg while it is spinning. But a full explanation of it is within the reach of any student who will train his mind to reason consecutively, and avail himself of the aids to prolonged consecutive thought which mathematicians have contrived. He will then see that the most obvious and familiar mechanical facts involve as necessary consequences all the phenomena which he finds in the schoolboy's top, in the physicist's gyroscope, and in the precision and nutation of the heavens. This, then, is a problem of Nature which falls within the province of the deductive method.

Whatever data are known exactly, these inferences from these data, however remote, may be depended upon as corresponding with what actually occurs in Nature. And if in such cases the mind of man has proved equal to the task of drawing inferences which can effectually grapple with the problems he finds around him in the Universe—which is, alas! as yet but too seldom—then will the deductive method, our plummet, explore depths in the great ocean of existence which our anchors of experiment could not have reached.

We must bear in mind that either method of investigation may be misapplied, and that this is a risk carefully to be guarded against. The deductive method when misapplied lands us in speculation; the experimental method becomes empiricism; and it so happens that the sciences of mechanics and chemistry are not only monuments of the power of the two great methods of investigation, but instructive examples of their weakness also. For in chemistry scarce any attempt at prolonged reasoning, carrying us by any lengthened flight to a distance from the experiments, can be relied on. The result has seldom risen to anything better than speculation. And, on the other hand, in mechanics, conclusions which depend on experiments only are empirical; that is, they are deficient in accuracy, and their relation to the other phenomena of the science is left in darkness. Here, then, we find in these two sciences not only how strong these two methods of investigation are, but how weak they may become if misapplied.

In the study of mechanics, however, and in the practice of chemistry, the two great methods of investigation may be studied separately, by steps of graduated difficulty, and with a superabundance of materials; and each of them supplies the necessary cautions with respect to the method which is all powerful in the other. No scientific man is really equipped for the pursuits in which both methods have to be employed till he has separately acquired a grasp of each. For it is only then that he will be armed against the errors which lead so many to mistake empirimism on the one hand, and speculation on the other, for solid science, or to underrate solid science, mistaking it for speculation. Nor is it only in his scientific occupations that he will derive benefit from this training. All exact reasoning, whether in science or in common life, belongs to these great divisions; and in the numberless instances in which we must be satisfied with reasoning which falls short of being exact, our only safety lies in having by the practice of exact reasoning, both deductive and experimental, attained to that intellectual tact and caution which alone will enable us to handle with safety the sharp and slippery tool. It is thus that a sound judgment with regard to truth may best be acquired by man or woman; and soundness of judgment is the noblest endowment of man's understanding, just as veracity is first among his virtues.

We shall conclude our report of the Proceedings of the Association in our next issue.

NOTICES OF BOOKS.

Fragments of Science: A series of detailed Essays, Addresses, and Reviews. By JOHN TYNDALL, F.R.S. Sixth edition. London: Longmans and Co. 1879.

THIS valuable collection of scientific essays now appears in the form of two substantial volumes, containing articles contributed to various magazines, Royal Institution lectures, and the celebrated Belfast Address. The range of subject-matter is very large: physics proper, chemistry, geology, philosophy, and even theology, are discussed in one or other of the articles. While the first volume deals specially with the phenomena of matter; the second treats of the correlation of phenomena of mind and matter. In the preface to this edition the author says: "In neither volume have I aspired to sit in the seat of the scornful, but rather to treat the questions touched upon with a tolerance, if not a reverence, befitting their difficulty and weight."

In the first volume, the author introduces various contributions to molecular physics; experiments on para-magnetic and diamagnetic forces; on dust and disease; and on fog signals. The memoirs of Faraday, Mayer, and Joule also appear; and the first article discusses spiritualism. In the second volume we find the well-known articles on scientific materialism, on the efficacy of prayer, and on the scientific use of the imagination. The Belfast Address, the apology for it, and the reply to the Rev. James Martineau's essay on the subject, are also here given. Then follow various lectures on fermentation, the germ-theory, and spontaneous generation, and the book is concluded by the lecture on the electric light delivered at the Royal Institution in January last. We feel sure that although the essays on materialism and prayer will offend the sensibilities of many readers, these fragments of science will continue to receive a large share of public attention.

Researches on the Motion of the Moon, made at the United States Naval Observatory, Washington. By SIMON NEWCOMB, Prof. U.S. Navy. Part I. Washington. 1878.

THIS first part of what will be a very lengthy and valuable monograph relates to the reduction and discussion of observa-

tions of the moon before 1750. An historical introduction is followed by an account of various ancient eclipses of the sun and moon, beginning with the eclipse of Thales, passing on to the Ptolemaic eclipses of the moon recorded in the Almagest, and the Arabian observations, as given in the writings of Ebu Gounis. These are followed by the observations of Bullialdus, Gassendus, and Hevelius, and the astronomers of the French School before 1750. A technical mathematical discussion of the moon's mean motion and of the value of the secular acceleration concludes this portion of a work which will be welcomed by astronomers in every part of the world.

A Treatise in Popular Language on the Solar Illumination of the Solar System. By COLLYNS SIMON. Williams and Norgate. 1879.

THE contents of this work is so very fully set forth in the title page, and we can have so little to say to such a treatise, that we confine our notice to a transcript of the title page:—"The solar illumination of the solar system, or the law and theory of the inverse squares; being an analysis of the two received laws relating to the diminution of light by distance, wherein it is shown that, according to undisputed facts of nature and of science, the solar illumination is equal throughout the whole system, and the law of inverse squares for light, physically impossible. To which is added the prospectus for a prize of fifty guineas offered for disproof of the scientific facts here for the first time indicated."

In the same category we must place Mr. Orson Pratt's "Key to the Universe, or a new theory of its mechanism founded upon a continuous orbital propulsion, arising from the velocity of gravity and its consequent aberrations, and the resisting ethereal medium of variable density."

Dreams of my Solitude on the Mysteries of the Heavens. By JOSHUA PRUSOL. London: Reeves and Turner. 1879.

THIS work, of a severely essaic type, may commend itself to certain dreamers who are not *au courant* with the courses and ways of modern science. Although highly imaginative, it is not devoid of profound thought, and we cannot too highly praise the singular modesty of the preface; specially of the concluding sentence, in which the author says, "Dear, naturally, as these

speculations are to him, the truth is dearer, and according as they consort or do not consort with it, does he expect others to give them entertainment, or undertake to denounce and repudiate them himself."

Navigation and Nautical Astronomy, with Special Table, Diagram, and Rules adapted for Navigating Iron Ships. By the Rev. W. T. READ, M.A. London: Elliot Stock. 1879.

AN useful work containing many original examples, and well adapted for teaching young naval officers the first principles of navigation.

The Electric Light in its Practical Applications. By PAGET HIGGS, LL.D., D.Sc. London: E. & F. N. Spon. 1879.

THIS work is specially devoted to a description of the various attempts which have been made to obtain a practicable system of electric lighting. An introductory chapter on the general principles of the voltaic arc and the method of lighting by incandescence is followed by chapters on electric lamps and candles; magneto and dynamic machines and their efficiency, and electric regulators; the division of the electric light is briefly discussed, and finally the commercial aspect of the question, and the various applications to military, maritime, mining, and other purposes.

In the first chapter the avoidance of waste of light is much insisted on, and the author asserts that when the laboratory of the Sorbonne was first lighted by electric candles "at least one-half of the light produced" was lost by radiation towards the sky through the glass roof of the building which served to light it during the day. The loss by this means in the case of an open-air light exposed at a considerable elevation, and unfurnished with reflectors, must be enormous. In the second chapter the various voltaic-arc lamps are described and figured; the last-mentioned of these—Higgs's lamp—is said to produce, with only four ordinary Bunsen elements, sufficient light to illuminate a building 60 feet by 40, and, when worked by a dynamo-electric machine of 2½ horse power, to produce four lights each equal to 400 candles. It simultaneously utilises the principles of incandescence, of the arc, and of the extra spark. Among the candles and candle lamps of course Jablochkoff's candle is the most prominent. It was invented in 1876, and has the special advantage that it entirely dispenses with any

kind of mechanism. In fact, the candle simply consists of two cylindrical rods of carbon, about three-sixteenths of an inch in diameter, and from $6\frac{1}{2}$ to 10 inches in length; they are placed vertically side by side, with a space of three-sixteenths of an inch between them, which is filled with plaster-of-Paris. The latter is fused as the arc passes at the extremity of the candle, and this fusion absorbs 30 per cent of the electric current; moreover, the candle, if once extinguished, cannot be re-lighted. With a light equal to 760 candles, three inches of carbon are consumed per hour. Wilde has modified Jablochkoff's candle, by removing the insulating plaster-of-Paris. The lamps for lighting by incandescence alone have not at present made much way.

In the fifth chapter the general principles of magneto and dynamo-electric machines are fully discussed. Commencing with the machines of Pixii and Clarke, and ending with those of Gramme, Edison, and Lontin-Siemens machine is effective, but it can only be used with one Serrin's or Siemens's lamp. It is said to give the following results:—

Revolutions per minute.	Illuminating power. Standard candles.	Horse power.	Weight.
850	1,200	2	280
650	6,000	4	420
360	14,000	8	1,288

Useful tables showing the efficiency of different forms of dynamo-electric machines are given on pp. 133, 136, 138, 140, 143, and 149. The highest recorded efficiency of a dynamo-electric machine is 38 per cent., while the efficiency of an ordinary steam engine in utilising the heat of the fuel does not exceed 20 per cent. The power expended by a dynamo-electric machine in producing the light of one sperm candle is about equivalent, according to the author, to 90 lbs. falling through one foot in one minute.

Some very interesting details as to the loss of production of the electric light compared with gas are given in the ninth chapter. The cost in Paris is said to be double that of an equally intense street lighting by gas. One electric light can usually be substituted for ten or twelve gas jets. The power absorbed is equal to one horse-power per candle. The subject of electric carbons is discussed in the last chapter. Archereau mixes carbon with magnesia. Carré with different salts. Those of potash and soda at least double the length of the arc. Gaudoin introduces phosphate of lime and various silicates and borates; others have coated the carbons with a deposit of metal such as nickel or copper, or have incorporated iron or copper in powder with the carbon. Gramme's experiments with carbons saturated with nitrate of bismuth proved that a considerable increase of light resulted.

Mr. Higgs's work gives us a very fair *resumé* of the whole subject of electric lighting, accompanied by an impartial discussion of the subject, and illustrated by excellent and numerous woodcuts. The perusal of the book forcibly impresses upon us the belief that the subject is still quite in its infancy, and that if the advances of the next fifteen years are as great as those of the last fifteen, we may expect to see gas very largely replaced by electricity.

The Student's Text Book of Electricity. By HENRY M. NOAD, F.R.S. A new edition. Edited by W. H. PREECE. London: Crosby Lockwood and Co. 1879.

THIS well-known work has been carefully edited by Mr. Preece, who has supplied new chapters containing a description of the most recent developments in all branches of electricity. The principles of duplex and quadruplex telegraphy, of the various electric lamps and candles, and of the telephone, and microphone have been fully explained, and constitute some of the most valuable portions of a book which will meet the requirements of a large number of students now as heretofore.

Four Lectures on Static Electric Induction. By J. E. H. GORDON, B.A. London: Sampson Low and Co. 1879.

THESE lectures, delivered at the Royal Institution, contain, as the author truly remarks, an account of a few phenomena which we can explain, and of a great many which we cannot. The results, however, have been discussed in a very suggestive and able manner, and the work cannot fail to recommend itself to all students of electricity. The more so when we remember that since the memorable monographs of Faraday comparatively little has been written on static electrical induction.

Mechanics. By R. S. BALL, F.R.S., Royal Astronomer of Ireland. London: Longman and Co. 1879.

THIS volume belongs to the "London Science Class Books" Series, and is carefully written and well illustrated. It will be useful for the higher forms in our public schools. The style is

good, and the subject is not overburdened by mathematical treatment. A fair example of the lucid explanation and admirable illustration of a subject will be found under the head of "friction on the inclined plane" (pp. 92, 93.)

Outlines of Geology and Geological Notes of Ireland. Being an Account of the Formation and Localities of its Mineral Resources. With an Addenda, containing Disintegration of Rock, Antiquity of the Earth, Climatic Changes of the Earth, Supposed Antiquity of Man, and the author's concluding Note. By WILLIAM HUGHES. Third edition. Dublin: M. H. Gill and Son, and W. H. Smith and Son.

WE have here a manual of the mineral resources of Ireland, along with descriptions and illustrations of the wild and sublime scenery of its coasts. Such a work, doubtless, might serve the very important purpose of drawing the attention of its readers to the great truths of geology and leading them on to become observers. So far as the author confines himself to a description of facts we can follow him with approval. But when he enters upon speculation and theory we are compelled to put in our protest. Mr. Hughes is in the first place a catastrophist of a school which we had supposed extinct. He declares that "this earth was subjected to repeated changes, that thousands of years intervened between them, and that each completely destroyed all vegetable and animal life." To him the history of our globe is not a career of regular development, but an alternation of extirpations and creations, separated by "periods of repose." It might have been hoped that notions so irreconcilable with recent discoveries and observations would no longer be reproduced in a popular treatise.

We are unfavourably impressed, too, with the apologetic tone of the work and with its vacillating character. At one moment the author seems to have fully grasped the great truth that "revelation was given not to inculcate physical science," but the next he is reconciling geology with Genesis—a process which his former admission ought to render needless. On the supposed high antiquity of the human race he declares (p. 117) that "we must leave our readers to draw their own conclusions from the evidences we have laid before them;" but, in his concluding note, he withdraws this permission and declares, "To those whose minds are not warped (!) there can be no doubt whatever but that man's appearance upon this terrestrial scene is as we have been all taught to believe it, and as Holy Writ gives it in the simple narrative of the creation." So then, in this respect at least, "revelation has been given to inculcate physical science."

Though holding very positively the belief that the interior of our globe is in a state of igneous fusion, he does not appear to accept the nebular hypothesis, since he repeatedly speaks of the earth being first "launched into space" and likewise suggests that it may have existed for ever.

Descending to matters more closely connected with daily life, we find Mr. Hughes adopting the theory, first broached, we believe, by Prof. Hull, that the seams of coal formerly occurring in Ireland have been transferred to Britain! That the central limestone plain of Ireland was once covered over with carboniferous beds, so that these strata have been gradually denuded by the action of water and other agencies we do not dispute. It seems to us, however, that if a region of coal-seams, intersected and separated from each other by beds of shales, sandstones, &c., were carried, little by little, into the adjoining ocean, and there deposited, we should not find them there re-arranged in distinct layers. Coal, shale and grit-stones would be all comminuted and blended together in confusion. Prof. Hull, it must not be forgotten, only gives a questioning adhesion to this wild hypothesis, since he speaks of the Irish strata as forming "perhaps some of the strata which were being piled up over the ocean-bed of the British area." What good cause can be shown that the British coal-seams were not formed from vegetable matter *in situ?*

What shall we say of this passsge: "All his (Voltaire's) ingenuity was employed to oppose the Mosaic account of the creation and the deluge; for instance, he asserts that the shells discovered in Alpine regions are simple freaks of nature, or else they were carried thither by pilgrims from Syria; therefore, so much for the opinions of men of science of the last century." Although we can by no means recognise Voltaire as a true man of science, and though we consider his explanations of fossil shells as utterly groundless, we must remember that the former of them was shared by many orthodox divines, and that neither of them is more outrageous than their ascription to the Noachian deluge, or than the hypothesis advanced by a man of science still living that they are forgeries, divinely perpetrated to lead vain man into error. If Mr. Hughes will search he will find that the eighteenth century is rich in illustrious men of science.

We are by no means disposed to deal severely with inelegancies and inaccuracies of expression so long as they do not obscure an author's meaning. Still, such utterances as "I have added an addenda," "an isolated strata," &c., rather grate upon our feelings. What, too, must the classical scholar think of the subjoined rendering:—"eocene, miocene, pliocene and, post-pliocene, all compounded of Greek words signifying 'earliest new,' 'less new,' 'more new,' and 'often more new.'"

We very much regret that in the interests of science we cannot give this book our recommendation.

Annual Record of Science and Industry for 1878. Edited by SPENCER F. BAIRD, with the assistance of eminent men of Science. New York: Harper Brothers. London: Trübner and Co. 1879.

THE eighth issue of this useful compilation is fully equal to those which have preceded it. The volume is divided into sixteen sections—Astronomy, Physics of the Globe, Physics in General, Chemistry, Mineralogy, Geology, Hydrography, Geography, Microscopy, Anthropology, Zoology, Botany, Agriculture, Engineering, Technology, and Industrial Statistics. The Appendix contains a necrology and the bibliography of the year.

One of the best sections in the book is the first, Astronomy, edited by Prof. E. S. Holden, of the Naval Observatory, Washington. The great solar eclipse of July 29th, 1878, of course occupies the largest amount of space, the American observers naturally getting the lion's share. The transits of Mercury and Venus, also, come in for a large share of attention. Professor Holden gives an interesting *bibliographie raisonnée* of his own particular subject, and it would have been as well if all his colleagues had followed his example instead of relegating the matter to an Appendix. So much trash in a scientific way is published now-a-days that one wants to know briefly, but honestly, what a book is worth, and not merely its name, author, size, and price. An interesting Report on American Observatories is given in this section.

The section edited by M. Clement Abbé and Prof. Rockwood, and devoted to Physics of the Globe, is also well treated, taking up no less than 118 pages, or a fifth of the whole book. The subject is well arranged, being divided into three broad headings—the Earth, the Ocean, and the Atmosphere. The value of the section is however, greatly impaired by the paucity of bibliographical references. Such books as the present are not bought merely as a matter of scientific curiosity, but as works of reference, and neither names, discoveries, nor researches should be mentioned without giving every particular that is necessary for obtaining a perfect knowledge of the matter.

These remarks apply with more or less justice to many of the other sections, especially to that on Physics, by Professor Barker, which is the most deficient of them all in bibliographical references.

The very unequal amount of space given to the different sections is apparent in the meagre account given of the year's chemical research, also by Prof. Barker.

Setting aside the defects we have mentioned, Mr. Baird's work is most valuable.

CORRESPONDENCE.

THE ANOMALOUS SEASON.

To the Editor of the Monthly Journal of Science.

Sir,—The author of the article "The Anomalous Season," inserted in your last issue, might have added, on the authority of Knapp, that the hot summer of 1825 had been preceded by a mild wet winter, and was followed by another summer almost as hot and dry, whilst in the intervening winter (1825-26) snow and frost only lasted about ten days. This certainly confirms the view that exceptional weather, when it sets in, lasts for more than one season, and that a warm summer is generally preceded by a mild winter.—I am, &c.,

An Observer.

ACTION OF LIGHT UPON THE COLOURATION OF THE ORGANIC WORLD.

To the Editor of the Monthly Journal of Science.

Sir,—I have this year observed a phenomenon which scarcely agrees with certain generalisations upon which I ventured in an article on "The Action of Light upon the Colouration of the Organic World." I there said that "pure and bright colours are connected with the highest vitality only," and further, that the process of decay is "attended by a degradation of colour." This year, however, I have been struck with the fact that the flowers of the common whitethorn, when about to fall and after their odour has disappeared, assume, not like other white flowers a dirty brown, but a pure and delicate pink or rose shade, with minute specks and points of a very decided red. I have carefully examined these colours, and find them free from any approach to a maroon or a liver.colour, which we might more easily associate with decay. It is striking that we should have here a change exactly the reverse of what takes place in the rose, the apple-blossom, and the almond. All these open with

various shades of red or pink, and fade towards a white; yet they belong to the same botanical family as the whitethorn. The change in question, though very general, was by no means universal, and it seemed to me most pronounced in situations little exposed to the direct rays of the sun.—I am, &c.,

J. W. S.

HUMANITARIANISM EXTRAORDINARY.

To the Editor of the Monthly Journal of Science.

Sir,—At a meeting held in connection with the recent Westminster Exhibition, Mr. J. G. Talbot, M.P., is reported to have said, in reference to some cases of butterflies there displayed, that such collections involved a "fearful waste of insect life," and to have expressed a wish that Entomology could be "studied in a more humane manner." It really seems as if, in these virtuous days, the infliction of pain or of death is to be tolerated for any and every purpose, save in the pursuit of knowledge. The number of insects sacrificed for preservation in museums and in private collections, or for microscopic examination, &c., seems to me a mere trifle compared with those destroyed out of mere wantonness or for ostentatious decorations. In the shops of certain "naturalists" (save the mark!) we may see trays and boxes full of splendid Buprestidæ, &c., which are sold to jewellers, milliners, &c. Might not Mr. Talbot have found here a "waste of insect life" much more to be regretted? With birds the case is even more glaring. I will venture to say that for one which falls a sacrifice to Science, ten become victims to Sport and to Fashion. But the modern humanitarian dares not attack Sport and Fashion, so he seeks to fetter Science. Biologists greatly deceive themselves if they think that even the total abolition of vivisection will satisfy the "hysterical party." It is surely time for the organisation of a "Biological Defence League."—I am, &c.,

Awake!

HABITS OF SERPENTS.

To the Editor of the Monthly Journal of Science.

Sir,—Waterton strongly insists, in his Essays, that serpents never pursue a retreating prey, and never act gratuitously upon the offensive. Both these statements are contradicted by more

recent observers. The late Mr. Belt describes a serpent chasing a lizard among the branches of a tree, and Dr. Livingstone gives an instance of an African serpent emerging from its hole, biting with fatal effect a man who was passing, and then returning to its hiding-place. Is anyone among your readers able to state whether wild animals, too large to become the prey of poisonous snakes, are ever bitten and destroyed by them? Has anyone ever found in the woods of India, of the Malayan Islands, or of Africa, any dead carnivore, ape, antelope, &c., whose death could be plainly traced to serpent-bite? That domestic animals thus perish, especially dogs, is a well-known fact.—I am, &c.,

VERIFIER.

THE MONTHLY

JOURNAL OF SCIENCE.

OCTOBER, 1879.

I. ON THE TEMPERATURE OF THE SUN.*

By Professor S. P. LANGLEY.

IT is known to all that there is a problem of the highest interest in solar physics at present waiting solution. I mean that of the temperature of the sun; and, so far as the whole radiant energy is inferable from the rate of emission of heat, the problem is one the theoretical solution of which is evidently dependent on our knowledge of the laws of cooling.

Every operation of Nature, whether in the organic or inorganic kingdom, is accompanied by the emission or absorption of heat, and, considering that—whether the subject of observation be the germination of a seed, the heat of a stove, or the outflow from the sun upon the planetary system—we want to know the rate of the deperdition of energy, one might certainly suppose that no physical law would have been better ascertained; but we are here, however (at least in regard to high temperatures), in a state of nearly complete ignorance, and know almost literally nothing about what so intimately concerns us. This is a reproach to modern physics, which has probably made no real advance here since Newton. To justify this language I remark that, in the case of the solar temperature, the *amount* of heat the sun sends us is scarcely in question, as we are all substantially agreed on the way to measure this and on the results of measurement. The latest of these give, it is true, larger values than those of Pouillet, which were about 1·75 calories per centimetre per minute, instead of 2·50; but these considerable variations are so trifling compared with those in the deductions made from them, that we may still say

* A Paper read before the American Academy of Arts and Sciences, October, 1878. Communicated by the author.

there is substantial agreement as to data. From like data, then, Sir John Herschel concludes that the temperature of the solar surface is over 5,000,000° C.; Mr. Ericsson, whose labours on this point deserve wider recognition, is confident that the temperature is not materially different from 4,000,000° F.; Father Secchi, in his latest research, makes it 133,000° C.; Sir Wm. Thomson and others estimate 30,000° to 60,000° C.

These extremely gross discrepancies having drawn general attention, many distinguished French physicists have lately re-investigated the subject, and, using Dulong's and Petit's formula, have after most elaborate research arrived at the nearly unanimous conclusion that the temperature of the solar surface is altogether lower than any of these,—is in any case not more than 2000° to 2500° C., but is more probably below than above the temperatures which are reached in our furnaces, and in fact is probably less than that of melting platinum.

It is here to be borne in mind that we really know nothing about the absolute emissive capacity of the solar surface, and that to simplify the problem, when we speak of the sun's being at a lower temperature than that of a certain lamp-black surface or hot platinum or steel, it is assumed, for the purpose of comparison, by myself as well as by the above-named investigators, that both the solar and terrestrial sources of heat have the same emissive capacity. The temperature thus defined has been called the "effective" temperature.

M. Violle, one of the most distinguished students of the subject, whose experiments bear evidence of intelligent care, found by observations at Grenoble, in March, 1874, that, with an emissive power thus defined, the temperature of the solar surface was 1230° C.* In a subsequent memoir he finds for the same the rather higher value of 1354° C.† After allowing for absorption in our atmosphere, it remains true that the temperature is then much below that of melting platinum, and this seems to be confirmed by his later results, which give about 1550 C. as the highest "effective" temperature.

All these and other observations involve the use of the empirical formula, well known as that of Dulong and Petit, which has replaced the earlier and simpler one of Newton.

Now, whatever be the apparent presumption of opposing my opinion to that of so many conscientious and recent

* Comptes Rendus, vol. lxxxviii., p. 1425.
† *Id.*, p. 1816.

investigators, I feel there is something yet to be said; and the present paper is an account of experiments of a special character, undertaken at the expense of the Rumford fund, not with the hope of at once solving so arduous a problem, but with the wish, in this confusion of opinion, to contribute one or two incontrovertible facts as material towards the construction of future theory. I hope to show convincingly that the sun's "effective" temperature is, at any rate, far above that of any ordinarily attained in the arts (very much above that of melting platinum for instance), and incidentally that the law of Dulong and Petit is untrustworthy precisely where we need to apply it.

If we have no formula by which to infer the temperature of the sun, there remains the comparison of its radiation with that of a terrestrial source of high *known* temperature. Thus the late Father Secchi has measured the radiation from the electric arc, and M. Violle that from a Siemens-Martin's furnace; but, by comparing these only with others made at other times on the sun, discrepant results appear also. Were we, however, to compare the sun *directly* with a terrestrial source of high temperature, and bringing them face to face find one giving more heat than the other, there could (with equal emissive powers) be no question but that the body radiating more heat was also the higher in temperature. Strange to say, this simple test has never, that I know, been applied to this problem* until in the experiments I am about to describe.

We have in the arts one process which gives what we want ready to hand in the production of a vertically disposed surface of several square feet of a liquid metal, hotter than melted platinum itself. I refer to the Bessemer process now in use in several places in this country, among others at the Edgar Thompson steel-works, about twelve miles from Pittsburg. I have received every possible assistance from the managers of this great establishment, and owe my acknowledgments here for their kindness.

As the Bessemer process may be as vaguely known to some as it was till lately to me, I will first briefly describe so much of it as concerns the present purpose.

An enormous egg-shaped vessel called the "Converter," capable of holding 30,000 to 40,000 pounds of melted metal, is swung on trunnions so that it can be raised by an engine to a vertical position, or lowered so as to pour its contents into a cauldron. First, the empty "converter" is inclined,

* Experiments with the lime and electric lights made for other purposes are not here in question.

and into its mouth is poured about 15,000 pounds of fluid pig-iron, whose temperature as it flows in from an adjacent furnace, where it has previously been melted, is about 1400° C. Then the "converter" is lifted to an erect position, and an air-blast from a powerful blowing-engine is forced up through its liquid contents. In the 15,000 pounds of impure iron there are ordinarily found about 230 pounds of silicon and 540 of carbon; and as each pound of carbon gives 8,000 calories, and each pound of silicon 12,000 to 14,000, in connection with the air-blast's unlimited supply of oxygen, the temperature of the already molten metal rapidly rises under this enormous inflow of several million calories of heat. After the blast has continued eighteen to twenty minutes, the temperature of the contents is from 1800° to 2000° C., or higher than that of melted platinum, taking the lowest estimate; and now the "converter" is again lowered, and about 1500 pounds more of melted iron added. The temperature here, perhaps, falls slightly, but its effect may be judged by any one who sees this second lot of iron poured in. Melted iron by itself, every one knows, seems dazzlingly bright; but as this streams into the open mouth, the interior is so much brighter still, that the stream is deep-brown by comparison, presenting a contrast like that of dark coffee poured into a white cup. The contents are no longer iron, but liquid steel ready for pouring into the cauldron; and, looking from in front into the inclined vessel, we see the almost blindingly bright interior dripping with the drainage of the metal running down its sides, so that the circular mouth, which is twenty-four inches in diameter, presents the effect of a disk of molten metal of that size, were it possible to maintain such a disk in a vertical position. In addition, we have the actual stream of falling metal which continues nearly a minute, and presents an area of some square feet. The shower of scintillations from this liquid cataract of what seems at first "sun-like" brilliancy, and the immense area whence such intense heat and light are for a brief time radiated, make the spectacle a most striking one.

Platinum dipped in the steel as it pours from the lip melts away; and not to rely on this evidence, which might be alleged to be due to an alloying rather than a true melting, I procured some platinum wire which Mr. Preusser, the chemist of the works, stretched, at my request, across the open mouth of the "converter" when in an erect position. The platinum, here several yards above the metal, was melted by the blast which came from it.

Heat Comparisons.

After many visits to the works, much trouble and repeated failures due to the difficulties of working in such novel circumstances, I secured a series of trustworthy measures, in May last, both of heat and light. I describe my apparatus here in principle, not in detail; and I omit many preliminary experiments, as well as some minute corrections applied for small instrumental errors, giving my results in general terms. One difficulty attending a simultaneous comparison was to obtain a station looking into the "converter" at the time it was inclined and pouring, and yet necessarily outside the building in the sunlight. To do this, I stood in a window (whence the sash had been removed) of the west wall, sixty-one feet from the "converter" mouth. A platform was erected here for my apparatus, part of which was clamped to the wall itself; but though this was the best point of observation, the noise, the shower of sparks driven over the instruments from within by the blast at each "pour," and the rain of wet soot without which fell thick at times on apparatus and observer from the combined steam and smoke of adjacent chimneys, made the task of observation another thing from what it is in the quiet of a physical cabinet.

From this window-station, a *porte-lumière* reflected the sun's rays, so that traced through the dusty air the beam was seen to enter the "converter" mouth, or fall on the stream which ran from it. In the path of this beam was a cylinder, containing within a double enclosure an Elliott thermopile of forty small elements, similar to that I had used for some years on the sun, and surrounded by all the precautions against air-currents and extraneous influences taught me by experience. The pile exposed both faces at once, one to the furnace, the other to the reflected sunbeam; and a Thompson reflecting galvanometer read by an assistant, and placed at a considerable distance from any moving iron, gave prompt evidence as to which face was hotter.

The angular area, subtended at the pile by the fluid metal, was always many times that subtended by the sun's disk, and there was no lens or medium of any kind (except air) between the "converter" mouth and the pile. Supposing, then, the metal to have only presented a disk equal in angular diameter to that of the sun, if the needle remained stationary, it is plain that each was sending an equal amount ot heat, and that any square foot of the solar surface was

radiating at least as much heat as a square foot of the metal; for it is obvious that the distances of the two sources have nothing to do with this effect under the given conditions.

The metal area, however, being many times that of the sun, the latter still over-balanced the metal; showing that the sun was actually very much the hotter. Accordingly, there was interposed between the *porte-lumière* and the pile a telescope which diffused the sun-light over an image of any given diameter. As the solar light entered only through a diaphragm of known dimensions, it was easy to say how much the sun's heat was weakened to balance that from the metal. It must be borne in mind, however, that there was no account taken of the loss of solar heat by reflection and absorption in the lenses, by reflection from the mirror, and more than all by the frequent clouds of smoke and steam, while the furnace heat suffered no diminution whatever. Further, every other condition of the experiment was designedly such as to weigh in favour of the furnace and against the sun's heat. The value found for the latter, then, is a minimum value. I should, perhaps, have remarked that experiments had shown that the trifling heat from objects near the melted metal might be neglected. That from the atmosphere about the sun was also insignificant. Except, then, for the diminution of solar heat by absorption, reflection, and so on, our method is equivalent to bringing a specimen piece from the sun's surface (if I may so express myself) face to face with one from the furnace, placing our thermopile mid-way between them, and determining how much we have to diminish the size of the former to make its heat-radiation no more than equal the latter's.

The result of these experiments was that the minimum value we can assign to the solar radiation is eighty-seven times that from an equal area of the pouring metal. This, it will be remembered, is not an actual but a minimum value. The true value may be indefinitely greater.

Photometric Comparisons.

Of the complex radiations from any source of high temperature, a part is interpreted by the pile as heat, a part by the eye as light; but as the temperature is raised, it is now well known that the waves of shorter length increase in amplitude much faster than the longer ones. If the temperature of the sun, then, be much greater than that of

the furnace, we shall have a quite independent proof of the fact in a photometric comparison, which, we can safely pronounce *a priori*, will then give a very much greater ratio of sunlight to furnace-light than that of sun-heat to furnace-heat. To make this comparison, a photometer box, about 8 inches in square section and 66 inches in length, is placed so that its central axis lies as before in the path of the reflected beam from the mirror to the furnace. Two similar telescopes of 1·66 inch aperture and 20·01 inches focus, having their objectives outside the extremities of the box and their optical axes in the path of the beam, project, by their eye-pieces, images of the sun and of the pouring metal on the two sides of a Bunsen disk, whose normal position is in the centre of the box. Both images are viewed simultaneously by mirrors attached to the disk, which is movable along a graduated scale. (I here omit certain small corrections applied in practice, and describe the use of the instrument in brief terms.) We do not now need to consider the relative angular areas of the sun and furnace, for so long as both are of appreciable size the images of both falling on the screen, when nearly midway between the two telescopes, will be sensibly proportioned in brightness to the absolute intensities of the sun-light and furnace-light. We do, in fact, however, at the outset find the sun-light so immensely brighter that no direct comparison is possible. We then diminish the aperture of the solar telescope (which we will call A), till it has a small known ratio to that of the furnace telescope (which we will call B). In practice B was always left with the full aperture of 1·66 inch diameter, while that of A was 0·192. Were the original sources of equal intensity, the sun-light would have been reduced to $\left(\frac{0 \cdot 192}{1 \cdot 660}\right)^2 = 0 \cdot 013 +$ or a little over one one-hundredth of the other. But it was surprising to see that the image from A was even now incomparably stronger than that formed either by the flame from the blast at its brightest, or by the pouring metal. Under these circumstances, the Bunsen disk was moved from its central position toward B, thus approaching the apex of one light cone and withdrawing from the other, so as to diminish the sun-light still further in an exactly determinable ratio. The lowest value obtained in a series of accordant measures gave intensity of sun-light over (5300) five thousand and three hundred times that from the metal; and this value is, I think, considerably below the truth.

It results from these experiments: (1) That direct observa-

tion disproves the statement that the sun's effective temperature does not exceed 1500° C. It is demonstrably over 1800° C., and may for anything here shown to the contrary be indefinitely greater.

(2) The solar heat-radiation, so far from being comparable to furnace heat, is at a minimum something like 100 times that from melted platinum, area for area, and probably much greater.

(3) The solar-light radiation (which offers a more trustworthy indication of the total difference between the sum of all degrees of radiant energy than the heat) is over 5300 times that from a temperature above that of melted platinum.

(4) Since all the above results are simple statements of the facts of experiment, and are independent of formulæ, we conclude that the formula of Dulong and Petit (which from well-conducted experiments, like those of M. Violle, deduces conclusions which trial disproves) must be itself wrong. Further, since this formula contains no term depending on the wave-length, it takes no account of the difference here proved to exist between the relative quantities of heat and light radiation from sources of high temperature, and is thus found especially untrustworthy at those temperatures at which it has been most frequently applied.

I do not yet venture an opinion of my own on the real temperature of the sun, further than that I think it much higher than has been of late believed.

The preceding observations and inferences all seem to point to the use of the highest attainable terrestrial temperatures (*e.g.*, that of the electric light) in comparisons (and the consequent least dependance on formulæ) as the safest line for future investigation.

II. SCIENTIFIC MATERIALISM AND ULTIMATE CONCEPTIONS.*

UNDER the above title Mr. Sidney Billing has issued a work which even those who dissent most widely from the author's views will find, we think, worthy of a careful examination. Its main features are a vigorous critique of scientific materialism as expressed more or less overtly in many works and speeches of the present day, and especially in the famous "Belfast Address" and "Birmingham Oration" of Prof. Tyndall; a protest against the license of so-called "scientific imagination" and against the dogmatic intolerance—shall we say of official science? Lastly, the author calls in question the received theory of heat as a molecular and ethereal vibration, and suggests that it should be regarded as a substantive entity, "the primordial unity out of which matter arose," the principle of all terrestrial phenomena, but itself a product of intelligence. These three questions are not discussed separately and seriatim, but are interwoven together, and accompanied by utterances and reflections on a great variety of subjects, which, if not always strictly relevant to the main issues, are often highly original and suggestive. As an instance we extract the following note:—"The red-skin hunter hoped to be translated to a region abounding in game. The Maori believed that life after death is a series of skirmishes in which the blessed are always victorious. The Teuton of old nourished the same hope. Civilisation cramps such aspirations. Would the cotton-weaver be content to labour for ever in cotton-mills, even though they were miles upon miles in length?"

Mr. Billing brings to his task an amount of erudition which we must pronounce wonderful. He has read and has at command not merely scientific and quasi-scientific, but non-scientific and even anti-scientific, works. Carlyle, Fichte, Bruno, Spinoza, Lucretius, Hume, Cousin, Radcliffe, and Noah Porter are as familiar to him as Humboldt, Liebig, Kekulé, Helmholtz, Darwin, Du Bois-Reymond, or the "Bridgwater Treatises." We find, however, no sign that he is an experimentalist or observer in any branch of

* Scientific Materialism and Ultimate Conceptions, by SIDNEY BILLING. London: Bickers and Son.

science. For a further understanding of his position we may remark that he is an Evolutionist—though somewhat encumbered with teleology—and an admirer of Darwin. Though a stout opponent of what is called "materialism," he is far from identifying himself with any of the creeds. Hence every man's hand will be against him. He has blasphemed South Kensington in the person of some of its chiefs, and has failed to conciliate Exeter Hall and the tea-tables.

As far as regards his denial of the dogma that in matter we have "the promise and potency of every form and quality of life," we are with him heart and soul. Nor can we dissent from him when he enters his indignant protest against the part now played in science by imagination, in the absence of demonstration. Philosophers have in former days denounced the credulity of religionists. Alas! it is to be feared that in our time the authorised expounders of science make larger demands on the unreasoning faith of their hearers than did ever the apostles of a new religion. When we overlook the field of human knowledge, and consider how much is assumed and how little is really established, we are seized with a deep sadness. Nor is this scientific faith genial and tolerant. It is too true, as the author quotes from Prof. Kekulé, that all "who sin against these dogmas are persecuted as heretics." Said Professor Tyndall in his much discussed Belfast Address:—"There is in the true man of science a wish stronger than the wish to have his beliefs upheld, namely, the wish to have them true, which causes him to reject the most plausible support if he has reason to suspect that it is vitiated by error." These are brave words; but though the accepted theories of heat and of light are admittedly not free from difficulties, how many physicists could be found willing to submit such theories to a full re-consideration, and to weigh the arguments in favour of the hypothesis which our author seeks to revive? Nay, what is the reception met with by an experimental fact which seems to point in some heterodox direction?

Mr. Billing, who takes for his motto the sober watchword of Du Bois-Reymond, "*Ignoramus—Ignorabimus*," compares with the thoughts which it embodies certain other phases of German speculation as brought popularly forward in the "Munich Addresses" of Nägeli, Haeckel, and Virchow. The first of these worthies, who for our tastes is far too sanguine in his expectations of what scientific certainties we may yet attain, proposes as a counter-cry "We know,

and shall know, if we be content with human insight,"—a saving clause capable of exceedingly wide application. As to Prof. Virchow, we distrust him. His ostensible watchword, as here quoted, is "That which honours me is a knowledge of my own ignorance;" his esoteric motto is said to be "Restringamur," *i.e.*, academic freedom, and, probably, the liberty of the author as well as of the professor. The charge brought against him, that he is intriguing to eject all Evolutionists from their chairs in the German universities, has not been formally demonstrated, but it is strongly countenanced by the following language which he has undoubtedly used:—"I only hope that the theory of Descent may not bring all those horrors in our country which similar theories (!) have actually brought to our neighbours. Anyhow this theory, if carried through to its consequences, has an extremely dangerous side, and that the Socialists have a certain notion of it already you will doubtless have remarked." We will not dictate to Mr. Billing what estimate to form of the man who makes this artful and disingenuous attempt to excite the *odium politicum* against a theory which he dislikes. In spite of his well-known abilities Prof. Virchow might feel somewhat puzzled if bound to name the "similar theories" which have given rise to "horrors" in France. If he really believes what he here professes he can scarcely have taken the trouble to examine what the tendencies of Evolutionism truly are, and his "knowledge of his ignorance," before it can throw any great honour upon him, will require to be supplemented. Were this the legitimate sphere for such a discussion it would be easy to show that the doctrine of Descent points in a direction the very opposite, and is flatly antagonistic to not a few of the leading principles of "social democrats" and "advanced political thinkers." This task, however, has been ably executed by Prof. Oscar Schmidt, to whose memoir we refer the reader.

The author's criticism of the Belfast Address and the Birmingham Oration, able as it must in many respects be pronounced, comes too late. We cannot help, however, noticing that the story of the "merchant convulsed into action by a telegram," quoted by Prof. Tyndall from Lange, points in reality to one of the most powerful arguments against the materialistic (somatic) theory of life A man is, we will suppose, at dinner with a brisk appetite. Suddenly he receives—whether by letter, telegram, or verbal message it matters not—some alarming message. A dear friend may have been taken dangerously ill; or a litigious

idiot, of whose very existence he was ignorant, may have brought against him an action for libel. What is the result? His appetite is gone, and all the ordinary symptoms of indigestion come on. Yet for all this there is no physical cause. No material substance has been introduced into or withdrawn from his system. No "force," using the word in its ordinary sense, acts upon him in a different way from what it did previously. A purely immaterial something, a piece of intelligence, has acted upon him like a dose of poison. That such psychic poisons—if we may be allowed the expression—can even prove mortal the annals of medical science fully show.

These considerations bring us to one of the points on which we have not the pleasure of agreeing with Mr. Billing; we refer to his disposition to draw what to us appears far too absolute a boundary-line between man and the lower animals. It is superabundantly proved, as far as testimony can prove anything, that in brutes, also, ill health—chronic or acute—may be occasioned by purely immaterial causes, such as fear or distress.

The author's assumption that the intelligence of the lower animals is "not that of an individual, but of the whole species," is flatly contradictory to the experience of all who have taken trouble to observe closely and fairly. Between different individuals of the same species, and even of the same race, there occur the most manifest differences in mental power. One dog hits upon devices and stratagems to secure his ends which never strike another. One spider will ballast its web in stormy weather with, *e.g.*, a chip of wood, whilst other spiders of the same species, with the same facilities and under identical circumstances, do no such thing. Mr. Billing, we are sorry to perceive, goes the length of ascribing cases of animal intelligence to an error on the part of the observers. In other words, he refuses to admit facts contrary to his prepossessions. The assumption of an "innate potence" distinct from reason, and yet fulfilling the tasks of reason, is in truth a desperate attempt to uphold a decaying error.

The extent to which our author's science is diluted with "Bridgewater" is also to be regretted. We demur altogether to the fundamental assumption of teleologists that God's purposes are known to mankind; nor would it be hard to show, from indisputable facts, that if a maximum of earthly happiness for man had been His object, the world would assuredly have been constituted very differently from what it now is. Teleologists have been challenged to

produce a new Bridgewater Treatise having for its theme the mosquito, the chigo, the *Lucilia hominivora*, the trichina: but they have prudently held back.

Bell is quoted as saying that "Canine teeth accompany a carnivorous appetite and boldness of disposition; boldness, fierceness, and cunning accompany retractile claws and sharp teeth." The truth is, however, that canine teeth are splendidly developed in the non-carnivorous gorilla, and are absent in the bold and aggressive buffalo; whilst in "boldness, fierceness, and cunning" the Canidæ and the Ursidæ, which have not retractile claws, do not fall short of the cats. Galen is incorrect if he says that the eaglet will attempt to fly when first freed from the egg-shell; it will sit helplessly waiting to be fed.

But space will certainly not permit us to enter upon the minute discussion of Mr. Billing's biological views, which to the present writer, as an old naturalist, seem the weakest part of an otherwise most valuable and interesting work.

The author not merely admits the high antiquity of the human race, but suggests the possibility of a high civilisation having been attained in times of which no written record has survived. "It is possible," he says, "Suleiman and the pre-Adamite kings, with their attendant genii, were traditional allegories of knowledge and power, and it may be the dwellers on the earth had compassed a knowledge thousands and thousands of years before the so-called historical era, and far exceeding that pourtrayed or which science conceives." All this *may* be, but we can at present merely guess, and not prove.

Two passages seem to us to convey a grave and needed warning. "A nation actuated alone by moral law, with conscience as a regulator or administrator, could not exist beside other nations impulsed by a lower ideal, because it would be the prey of instinctive rapacity."

The following reflection may be taken to heart by us all:—"So enslaved is the general mind by the authorities of the time that it is assumed to be treason to doubt the dicta of the leaders of the day, talk what or how they may—absurdities become logic; sensationalisms, eloquence; suicidal fanaticism, patriotism; and prose run mad, poetry; all because at some time in their era they have earned a name for *some themes logically reasoned, for some experiments successfully conducted, for some political conduct ably directed, and for some poems admirable and artistic*."

We hope our readers will make personal acquaintance with a work which we would gladly have noticed in a style less hasty.

III. EXPLOSIONS FROM COMBUSTIBLE DUST.*

By Prof. L. W. Peck.

I WISH to demonstrate to you this evening, by a few simple experiments, the fact that all combustible material, when finely divided, forming a dust or powder, will, under proper conditions, burn with explosive rapidity.

If a large log of wood were ignited it might burn a week before being entirely consumed; split it up into cord-wood, and pile it up loosely, and it would burn in a couple of hours; again, split it into kindling-wood, pile loosely as before, and perhaps it would burn in less than an hour; cut it up into shavings and allow a strong wind to throw them into the air, or in any way keep the chips comparatively well separated from each other, and it might be entirely consumed in two or three minutes; or, finally, grind it up into a fine dust or powder, blow it in such a manner that every particle is surrounded by air, and it would burn in less than a second.

Perhaps you have noticed that shavings and fine kindlings will sometimes ignite so quickly in a stove that the covers will be slightly raised, the door forced open, or perhaps small flames will shoot out through the front damper. You have, in such a case, an explosion on a very small scale similar to that of the Washburn, Diamond, and Humboldt Mills of this city, on the night of May 2nd,—upon which occasion the rapid burning of hundreds of tons of flour, bran, &c., completely demolished the solid masonry walls (6 feet thick) of the mills, and threw sheets of iron from the roof of the Washburn so high into the air that they were carried 2 miles by the wind before striking the ground.

Let us now see why such explosions occur. Wood has in it a large amount of carbon, the material of which charcoal is composed, and the air is about one-fifth oxygen. Now, at the ordinary temperature, the carbon of the wood and the oxygen of the air do not combine; but when they are heated —as by friction, concentration of the sun's rays, chemical action as from a match, or in any other way—they combine to form carbonic acid gas. This chemical action produces a large additional amount of heat, which keeps up the action as long as there is any carbon and oxygen left to unite, and

* Lecture delivered June 1, 1878, at Association Hall, Minneapolis, Minnesota, at the request of the millers of the city.

also makes the temperature of the gas which is formed very high.

As the space occupied by the carbonic acid gas and that occupied by the oxygen which entered into the combination is the same at the same temperature, there would be no bursting if, after combination, the temperature were the same as before; but it is a fact, which you have all observed, that fuel in burning produces heat: it is also a fact that heat expands a gas, and it is this great amount of heat, taken up by the carbonic acid formed, that produces the immense pressure in all directions.

Let us return to our log of wood. There is exactly the same amount of heat and carbonic acid produced when complete combustion takes place in each of the cases of burning, the only difference being as to time. In the first case, the explosion or pushing aside of the surrounding air occupies a week, in the last only a second.

Snow-flakes fall gently upon your shoulders, and you are required to perform an insensible amount of work to resist the crushing effect of each flake; but suppose that all the snow that has fallen upon your head and shoulders for the last ten years was welded together in one solid mass of ice, weighing perhaps one hundred pounds, and that it should descend with the velocity of a snow-flake upon you, an immense effort would be required to prevent its crushing you, even if you were able to withstand the shock at all. The work of many days would be concentrated into an instant.

So it is with burning wood: four or five cords of wood and a large stove will give you a roaring fire all winter; the work done is manifested by the heat obtained, by the rushing of hot gases up the chimney, and of air from outside into the room through every crack. But if the wood were ground into a powder and scattered through all the house, and burned instantly, the cracks, doors, windows, and flues would not be sufficient to give vent to the hot gas, and the roof and sides of the house would be blown to pieces.

What is true of wood is also true of grain; also of vegetables, with their products when they contain carbon, with this exception—grain, either whole or ground, will not burn readily when in bulk. A fire could be built upon a binful of flour, and kept burning for half a day without igniting the flour; it would char upon the surface, but it lies in such a compact mass that the air does not get access to it readily; hence it does not burn.

I wish to show you now how combustible dust will burn

when blown into the air by means of a pair of ordinary hand-bellows.

I have here two boards, about 12 by 18 inches, nailed together, forming a V (*see* Fig. 1). Just outside of the V an

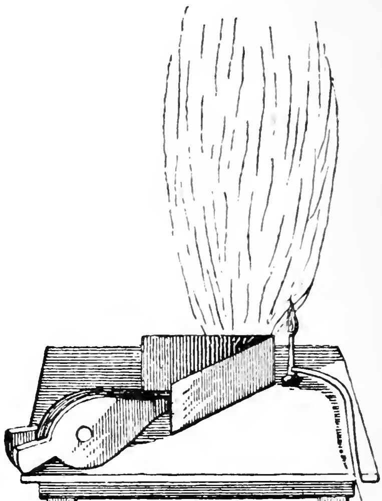


Fig 1

ordinary Bunsen gas-burner is placed, and within is a small handful of dust taken from a sash and blind factory. Upon blowing it smartly with the bellows a cloud is formed about 15 feet high,—extending, in fact, to the ceiling,—which ignites from the lamp and produces a flash, very quick and exceedingly *hot*, resembling very much a gunpowder flash. You will notice that a large amount of dust falls from all around the edge of the flame without burning; that is because it is not thick enough. Two things are necessary: first, that each grain of dust be surrounded with air, so that it can get the oxygen required *instantly*; and, secondly, that each grain shall be so near its neighbour that the flame will bridge over the space and pass the fire from particle to particle.

I think, after seeing the immense flame produced by such a small amount of fine saw- and sandpaper-dust, you will no longer wonder at the rapid spread of flames in furniture and similar factories. You know it is practically impossible to put out a fire after any headway is attained in these establishments; the draught produced will blow all the dust from walls and rafters into the air, and the building in an

instant is a mass of flame. Perhaps many of you remember the fire in the East-Side Saw-Mills, a few years ago. Large masses of fine sawdust had probably collected upon the rafters, and the whole roof was perhaps filled with cobwebs loaded down with dust. A fire started from one of the torches used, and shot through the mills with lightning-like rapidity, and save for the fact that the ends and sides of the building were all open, there would have followed an explosion like that at the flour-mills. As it was, the men had very great difficulty in escaping with their lives, notwithstanding that a short run in any direction would have taken them out of the mill.

It is very evident that too great care cannot be taken to keep all such factories and mills as free from dust as possible.

I will now blow some ordinary starch into the air in the same way, and you notice the flame is more vivid than in the last experiment, and if you were in my position you would notice that the heat produced is much greater. Notice now that this powdered sugar burns in the same way.

You will see, from the experiments further on, that three-quarters of an ounce of starch will throw a box, weighing 6 lbs., easily 20 feet into the air, and that half an ounce

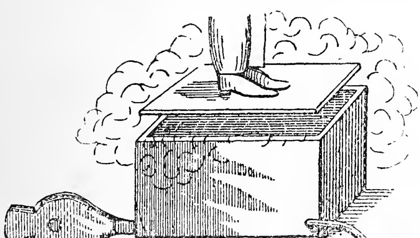


Fig. 2

burned in a box will throw up the cover 3 inches with a heavy man standing upon it.

With these facts, which I have demonstrated before you, no one need regard as a mystery the Barclay Street explosion in New York city, where a candy-manufactory, in which large amounts of starch and sugar might in many ways be thrown into the air by minor disturbances, took fire, and completely wrecked a building and destroyed many lives.

I will now burn in the same way some buckwheat, which, as you will observe, gives a very large blaze; now some corn-meal, which is too coarse to burn as well; now some rye-flour, which burns much better than the corn; now

some oatmeal, the finer part of which only burns; and so I might continue with all sorts of finely-ground vegetable material.

Let us take up now the products of the manufacture of flour from wheat. There were between 300 and 400 tons of these materials, upon which I am now to experiment, in the Washburn Mill at the time of explosion, and there was a corresponding amount in the Diamond and Humboldt Mills, which, by their sudden burning, produced the second and third shocks heard directly following the explosion of the larger mill.

The wheat is first placed in a machine, where it is rattled violently and brushed. At the same time a strong draught of air passes through it, taking all the fine dust, straw, &c., and conveying it through a spout to a room known as the wheat-dust room, or perhaps more commonly it is blown directly out of the mill.

You see some of this material here; it looks like the wood-dust of the first experiment, and, as you see, burns with a quick and sudden flash when subjected to the same conditions.

Here, then, we have the first source of danger in a flour-mill. A thick cloud of this dust, when conveyed through a spout by air, will burn in an instant if it takes fire; and if there is any considerable amount of dust, as there would be if there were a dust-room, an explosion will follow which may become general if it stirs up a thick dust-cloud throughout the mill.

The wheat, after it has been cleaned in this way, goes to the crushers, which are plain or fluted iron or porcelain rollers, working like the rollers in a rolling-mill. The object of these rollers is, I believe, to break off the bran in as large pieces as possible, and to crush out or flatten the germ so that it can be separated with the bran from the rest of the meal.

The crushed wheat goes now to the stones, where so much heat is produced (average 135° F.) that a large amount of steam is formed from the moisture in the materials. This steam would condense in the meal, and interfere with bolting, &c., if it were not removed. To effect this another draught of air and another spout are employed, and, as might be expected, this current takes a large quantity of the very finest flour, called flour-dust, with it. To save this a room is provided near the end of the spout, called the flour-dust house. The spout conveying steam and dust enters this room on one side, and another spout opposite leaves it,

passing to the open air. It is in this comparatively dead-air space that the dust settles, and can be collected from the floor. Here is some of this material, which, as you see, when blown into the air, produces a vivid flash, extending from the table to the wall.

The evidence taken before the coroner's jury shows very clearly that it was this material that started the great explosion of May 2nd. Just how the mill took fire will probably never be known, of course, but in all probability the stones either ran dry—that is, were without any meal between them—or some foreign substance, such as a nail, was in the feed, producing a train of sparks such as is produced by an emery wheel or a scissors-grinder's wheel. These sparks set fire to small wads of very hot dust, which, as soon as they were fanned into a blaze, communicated it to the spout and house full of dust. An eye-witness of the explosion first saw fire issuing from the corner of the mill where this flour-dust spout was situated, the end of the spout having probably been blown out. This fire was followed instantly by a quick flash, seen through all the windows of the floor upon which the flour-dust houses were situated, followed instantly by a flash in the second story, then the third, and, in rapid succession, fourth, fifth, and sixth stories; then followed the great report produced when the immense stone walls were thrown out in all four directions, and the roof and part of the interior of the mill shot into the air like a rocket.

It would seem that a blaze is necessary to ignite the mixture, for I have tried powerful electric sparks from a machine and from a battery of Leyden jars; also incandescent platinum wire in a galvanic circuit, and glowing charcoal, without producing any fire, however thick the dust might be. Perhaps, however, under more favourable conditions the dust would ignite directly from sparks, but it seems very improbable.

Let us continue now with the process through which the ground wheat is made to pass. From the stones it is conveyed to the bolting-reels, where the very finest is sifted out first, and we obtain a grade of flour; after the finer material is sifted out it goes to a coarser bolt, where the "middlings," as it is called, passes through, leaving the bran which comes out at the end of the reel. The middlings, as it comes from the bolts, has fine bran and dust in it, and, to purify it, it is subjected to an operation similar to that of cleaning the wheat,—that is, in the middlings purifiers it is subjected to a draught of air which takes away all the light bran and

dust, leaving the heavier material (purified middlings), which goes again to the stones to be ground into flour,

Here is some of the dust from these "middlings-machines;" you observe it burns as the other materials burned, quickly, and with intense heat.

Here is some of the purified middlings; each grain is comparatively large and heavy, making it difficult to blow it well into the air, but, as the blaze produced by each particle is quite large, a flash is produced which does not differ materially from the others.

Here is some of the general dust of the mill,—that is, dust swept up from the floors, walls, beams, &c. You will see it acts in all respects like the other substances.

And, finally, here is some of the flour taken this afternoon from the flour-sack at home; it burns, you observe, if possible with even more energy than the other kinds of dust.

I have performed a few experiments, which I will now repeat, which will illustrate to you the immense power that these materials exert when burned in a confined space.

This box (Fig. 2) has a capacity of 2 cubic feet; the cover has a strip 3 inches deep nailed around it, so that it telescopes into the box: there is in this lower corner an opening for the nozzle of the bellows, in this an opening for the tube to the lamp. I place now a little flour in the corner, light the lamp, and my assistant places the cover upon the box and steps upon it. Take notice that upon blowing through the hole, and filling the box with a cloud of flour, the cover comes up suddenly, man and all, until the hot gas gets a vent, and a stream of fire shoots out in all directions.

Here is a box (Fig. 3) of 3 cubic feet capacity, including this spout, 9 inches square and 15 inches long, coming from the top of it; at the ends doors are arranged closed like steam-boiler man-holes; openings for light and bellows are arranged as in the previous box.

Here is a box, weighing 6 lbs., that will just slip over the spout; it has a rope lest it should strike the wall after the explosion. Placing now the lamp in the box, some dust in the corner, and the box over the spout, we are ready for another explosion. You observe, after blowing vigorously for a second or two, the dust in the box takes fire; the box over the spout is shot off, and rises until the rope (about 12 feet long) jerks it back; it strikes the stage with great force, rebounds, and clears the foot-lights, and would strike the floor below were it not for the rope.

I have thrown a box similar to this in the open air 20 feet

high, while, as we shall see presently, less than an ounce of flour is being consumed.

I have fastened over the top of the spout five thicknesses of newspaper: upon igniting a boxful of dust as before, the paper is thrown violently into the air, accompanied by a loud report as it bursts.

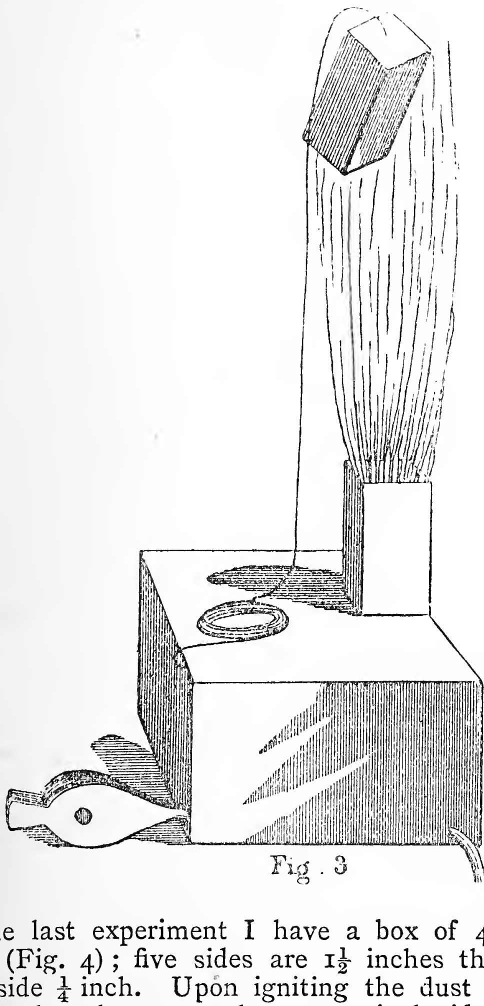


Fig. 3

For the last experiment I have a box of 4 cubic feet capacity (Fig. 4); five sides are $1\frac{1}{2}$ inches thick, the remaining side $\frac{1}{4}$ inch. Upon igniting the dust in this box, filled as in the other cases, the quarter-inch side bursts, and a stream of fire shoots out half-way across the stage.

One lb. of carbon and $2\frac{2}{3}$ lbs. of oxygen, when they combine to produce carbonic acid, will evolve heat enough, if it

were applied through a perfect heat-engine, to raise 562 tons 10 feet high; if, therefore, 40 per cent of flour is carbon, it would require $2\frac{1}{2}$ lbs. to accomplish this result, if an engine

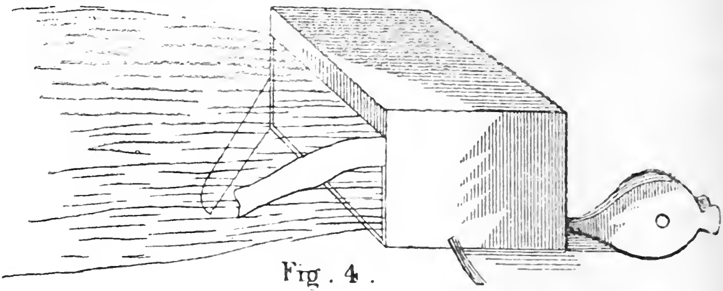


Fig. 4.

from which there would be absolutely no radiation, conduction, or loss of heat, in any way, were a practical possibility. Let us see how much air would be required to supply oxygen enough. Under ordinary conditions every 100 cubic inches of air contains 7·13 grains of oxygen, from which we find that $151\frac{1}{2}$ cubic feet of air would be required for the $2\frac{2}{3}$ lbs. of oxygen. Hence the $2\frac{1}{2}$ lbs. of flour must be equally distributed as a dust through $151\frac{1}{2}$ cubic feet of air, in order to produce the most powerful result.

If 41 ounces of flour require 151 cubic feet of air for perfect combustion, 1 cubic foot of air will supply oxygen enough for $\frac{40}{151}$ of an ounce of flour. Hence our box, which lifts the man so readily, burns $\frac{1}{2}$ ounce of flour or less; and the other, which throws the box into the air, $\frac{3}{4}$ of an ounce, unless, as I think quite probable, an additional amount of air is drawn in through the cracks as soon as the vent is opened at the top of the box. In fact these experiments work better if a few small holes are made near the bottom of the boxes.

It may be worthy of mention here, as a point of interest to insurance companies, that in all dust-explosions a fire precedes the explosion in every case. The dust must burn before the heat that produces the immense expansive force is generated.

Too great precaution cannot be taken in all kinds of manufactories, where combustible dust is produced, against fire, especially in those establishments where it is conveyed in thick clouds by air-draughts through spouts and rooms.—*Popular Science Monthly.*

IV. ANOTHER VIEW OF THE ANTIQUITY OF MAN.

By Prof. B. F. MUDGE, Manhattan, Kansas.

WE were much interested with the article in the June number of the "Review" on the Antiquity of Man, by the Rev. J. L. Templin. He has very clearly and fairly stated the facts in the case, though we think he has left the question of the age of our race, in years, too indefinite.

We agree with him (and Dana) that man was on earth "at the close of the Glacial epoch, that he witnessed the retreat of the glaciers from Central Europe," &c. After the Glacial epoch, geologists fix three distinct epochs,—viz., the Champlain, terrace, and delta,—which have been considered of nearly equal lengths. The question then arises, have we no evidence to show the length of either of these periods?

There are facts which circumstantially give us a near approximation of the age of each, but they are nearly all based on considerations which only an expert geologist can appreciate. There are, however, in the delta period of the Mississippi, proofs which will give a very close approximation, in years, to the length of that epoch.

Various estimates have formerly been made for the time of the delta period, with varied results, Lyell and others placing it at least at 100,000 years, and others making it less. Further researches, extensively made, have confirmed Lyell's estimate. The most reliable evidence is as follows: —For a distance of about 200 miles of the delta are seen distinct forest growths of large trees, one over the other, with interspaces of sand,—showing distinct periods of the entire destruction of the forests, and, after burial, again a new generation of trees over the others. There are ten of these distinct forest growths, which have begun and ended one after the other. The outstretched, undisturbed position of the roots of the trees, as well as the other conditions of the deposits, show that they were not washed in, but grew on the spot.

Now if we can fix the time occupied by the life of these trees, we can give, at least, an approximate result. Fortu-

nately the data are clear, and can be studied by any person of ordinary intelligence. The trees in question are the bald cypress (*Taxodium*) of the Southern States, one of the largest and longest of life of the world's trees. Specimens cut down in the present age are known to be from 5000 to 7000 years old. Humboldt saw one in Mexico of extremely large size, which he estimated—from counting the rings of smaller trees of the same species to be 8000 years old.

Such are the trees found in these old, buried forests of the delta of the Mississippi. Some of the old trunks were over 25 feet in diameter. One contained over 5700 annual rings. In many instances these huge trees have grown over the stumps or fallen trunks of others equally large. Such instances occur in all, or nearly all, of the ten forest beds. Dr. Dowler, one of the best physicians and scientists of New Orleans, saw such instances in many places, and concluded that each of the ten forest growths contained, on the average, two such trees of 5000 years each, living in succession. This gives to each forest a period of 10,000 years. Ten such periods give 100,000 years, without considering the time covered by the intervals between the ending of one forest and the beginning of another. The thickness of the intervening sand shows that this interval was not, in most cases, a short one.

Such evidence would be received in any court of law as sound and satisfactory, where common sense evidence is used. We do not see how such proof is to be discarded when applied to the antiquity of our race. If the antiquity of the mastodon was in discussion no one would doubt it. Human bones have been found in the fifth forest bed, and stone implements still lower. As Rev. Mr. Templin has stated, there is satisfactory evidence that man lived in the Champlain epoch. But the terrace epoch, or the greater part of it, intervenes between the Champlain and delta epochs, thus adding to my 100,000 years. If only as much time is given to both those epochs combined as to the delta period, 200,000 years is the total result.

There are other evidences in Europe, generally clear to the geologist, which sustain this long time for the antiquity of man.—*Kansas City Review of Science.*

V. THE FORMATION OF COAL.

THE paper read by M. Frémy, the eminent Director of the Museum at the Parisian Académie des Sciences, under the title "Chemical Researches respecting the Formation of Coal," is of such great interest that we have no hesitation in making it known to our readers.

It is known that coal is produced by the decomposition of vegetable matter, which for many ages covered the surface of the earth. The learned chemist has made a series of analyses, of which he gives an account in his paper, and which have enabled him to establish the laws of this decomposition, and to explain the transformation of the tissues of the vegetable matter into coal by the loss of their organic form.

With reference to peat, the hypothesis has been put forth generally up to the present time of a possible relation between their formation and that of coal. M. Frémy has been led by the same investigations to the conclusion that the peaty fermentation is, so to speak, the first stage reached by the ligneous tissues before arriving at their complete transformation into coal.

After making these short preliminary remarks, we hasten to introduce M. Frémy in his own words.

The paper, says M. Frémy, which I read to-day on the formation of coal is the latter part of the general investigations on the vegetable tissues which I have carried on since 1850, that is to say, since my nomination to the professorship of the museum.

It is at the Jardin des Plantes that I have found everything needful for pursuing my enquiries into certain matters highly interesting and important to chemistry and vegetable physiology.

The questions which I proposed to myself were as follows:—

What is the nature of the elements of which the organs and the tissues of vegetables are formed? Is it possible to discover these elements without altering them, and to determine with some exactness their proportions? Does this chemical analysis of the tissues permit of following the development in the vegetable organisation, and of establishing a comparison between them such as science demands? Is it possible for chemistry to make known the exact com-

position of these elements, at present but imperfectly defined, such as chlorophyllus gum, the gelatinous substances of fruits, which are found in a great number of tissues, and which certainly play an important part in the vegetable organisation? Lastly, will it be possible, when all these substances are understood, to determine under what influences the tissues of these vegetables are capable of being transformed into fossil fuel, such as lignite, coal, and anthracite?

Taking this programme as the basis of my researches, I shall make a rapid survey of those investigations by which I sought to establish the nature and composition of those substances forming the skeleton or framework of vegetable matter.

The organic elements which I first examined were but little known. Their characteristic property is to produce, under the influence of fermentation or by the action of chemical reagents, a series of gummy and gelatinous substances. They may be confounded with the cell-walls themselves when they are merely subjected to microscopical examination. They often join the cells together, as is shown in the tissue forming the pith of the elder tree. I have demonstrated that the gelatinous substances of vegetables are derived from a primal insoluble substance which I have named *pectose*, and which, by successive polymerical transformations, forms at first gummy substances, then gelatinous bodies, and lastly a soluble acid, the strength of which may be compared with that of the acids found in fruits. All these transformations are produced during the growth of the vegetation.

M. Frémy then undertook the investigation of the properties and nature of the stable and fixed bodies forming the fibres, the cells, and the ducts. From these researches it results that the vegetable framework is not so simple as was commonly supposed. It is not constituted of a single substance, the cellular variously incrusted with foreign substances, but of several kinds of isomeric cellular substances. In addition to this, a very important body is also met with, which differs from the cellular in respect to its composition and properties, a body which runs abundantly in the ducts, and which M. Frémy has for this reason called *vasculose*. It is this substance which causes the hardness and density of the ligneous tissues to vary according to the proportion in which it is contained. Oak may contain 30 per cent of it, and as much as 50 per cent may be found in nutshells. The *vasculose* is placed between the walls of the fibres and

the cells, and unites them. It is dissolved by caustic alkalies.

After these internal tissues M. Frémy studied the bodies which cover and protect them, such as the cuticle. He called by the name of *cutose* the substance which forms the cuticle, and which is even found in cork. It is remarkable for its fixity, and is scarcely affected by sulphuric acid. Lastly, to complete this general study, the learned Director of the Museum examined those bodies which are most frequently met with in the tissues. Gum, for instance, is not a neutral substance, as has been commonly supposed, but a veritable salt, resulting from the combination of a genuine acid with lime. The chlorophyllus itself is not a neutral substance. It is formed of a bluish-green alkaline salt, phyllocyanate of potassa.

These preliminary researches being concluded, M. Frémy placed the top stone to the edifice by attempting the solution of the difficult and interesting problem of the formation of fossil fuel. If vegetable palæontology has made such great progress in these latter times, it may be said that the chemical part of the question has remained in absolute darkness. We know not under what influences the vegetable organisation has been destroyed to form this black bituminous mass, partly fusible, non-organic, and insoluble in dissolvents, which constitutes coal. This substance neither resembles the pyrogenous bodies produced in our laboratories nor the ligneous tissues which have formed it. By distillation it gives forth volatile products which do not resemble those given by wood. It leaves also, as a fixed body, a special substance called coke, which is very different from charcoal.

Having already introduced this question in a previous paper, M. Frémy mentioned several chemical reactions which characterised wood, peat, lignite, coal, and anthracite. Wood is not perceptibly affected by a diluted solution of potassa, whereas peat often renders to this alkali considerable quantities of ulmic acid. Xyloide lignite, or fossil wood, also contains notable proportions of ulmic acid; but it is not to be confounded either with wood or with peat, because it is transformed into yellow resin by nitric acid, and is completely soluble in hypochlorites. Compact or perfect lignite does not contain any sensible quantity of ulmic acid, but is soluble in nitric acid and hypochlorites. With respect to coal and anthracite, they are characterised by their insolubility in all the neutral solvents, acids, alkalies, and hypochlorites.

The experiments of M. Daubrée upon anthracite, and the no less interesting experiments of M. Baroullier upon coal, lead us to think that the transformation into coal is brought about by the action of heat and pressure upon the vegetable matters. In order to analyse the phenomenon, M. Frémy arranged a series of tests, in which the vegetable tissues, on the one hand, and, on the other, those substances which most frequently accompany them in the organisation, were heated to between 200° and 300° for some hours in glass tubes sealed at both ends.

In these experiments M. Frémy observed a great modification. The tissues became black, brittle, and free from water, acids, gas, and tar; but they retained their original organisation, and furnished a fixed product, offering no resemblance to coal.

The learned academician then submitted to the same test a certain number of bodies produced by organism, such as sugar, starch, gum, &c., and arrived at very different results. These bodies were transformed into substances having a certain analogy to coal: they were black, shiny, often melted, and quite insoluble in acids and alkalies.

The analysis of these bodies, which M. Frémy designates by the name of coally substances, has demonstrated the complete analogy of their composition with that of natural coal. Coal from gum gives in analysis almost the same quantities of carbon, hydrogen, and oxygen as natural coal.

From these coincidences of composition M. Frémy draws the conclusion that the elements contained in the vegetable cells—such as sugar, starch, and gum—play an important part in the production of coal.

This result was doubtless a very interesting one; but, in order to complete it, it was necessary to explain how the tissues of vegetables could lose their organic form and produce the amorphous mass called coal. Besides this, also, it was necessary that the tissues should be proved to be capable of undergoing the same coal transformation which was realised by operating upon the non-organic bodies which accompanied them.

The studies of the eminent chemist with respect to lignite and peat now proved of great assistance to him. He had seen in these combustibles the ulmic acid appear in proportion as the ligneous tissues lost their organisation. When a peat was advanced he only found insignificant remains of organic tissue, but then it contained from 50 to 60 per cent of ulmic acid. On examining the fossil wood M. Frémy found pretty thick, black, and shining layers of ulmic acid

proceeding from the transformation of *vasculose* besides the ligneous fibres, which were not yet completely disorganised.

This observation demonstrated the transformation on the spot, and in the ligneous tissue itself, of a part of the wood into ulmic acid. In this way M. Frémy was led to admit the fact which appeared to predominate in his researches, that the vegetable matter was first changed into peat before producing coal, and that in this modification the disappearance of the organic tissues was owing to a kind of peaty fermentation. To confirm this hypothesis he found it necessary to prove that the ulmic acid could, like the bodies previously experimented upon, be transformed into coal.

In consequence M. Frémy studied the transformation of three kinds of ulmic acid, and discovered that, after a calcination of 120 hours, the ulmic acid of the peat was converted into artificial coal, presenting the same composition as the coal of Blanzy.

Lastly, the mixtures of chlorophyllus, fatty bodies, and resins obtained from leaves by treating them with alcohol, when submitted to the same operation,—that is to say, heated under pressure for 150 hours,—produced a black, viscous, odorous substance, insoluble in alkalies, and presenting an evident analogy with natural bitumen.

From these facts M. Frémy draws the following conclusions :—

1. Coal is a substance which proceeds from the transformation of vegetable matter, but which no longer preserves its organic form.

2. The vegetable marks which the coal presents are produced by it, as in schist or other mineral substances, and do not prove its organisation. This fuel is a bituminous and plastic matter, on which the external features of the vegetation are moulded. When a piece of coal presents on its surface, or within its interior, marks of vegetation, it is not to be thence inferred that the adjacent parts are necessarily the result of the alteration of the tissues which were covered by the membranes whose form has been preserved.

3. The principal bodies contained in the vegetable cells, submitted to the double influence of heat and pressure, produce substances which present the properties and composition of coal.

4. The colouring, resinous, and fatty matters contained in the leaves are changed, by the action of heat and pressure, into substances which approach very nearly to natural bitumen.

5. With regard to the ligneous tissues at the base of the

cellulose and the *vasculose*, they are not transformed directly into coal. Before being so changed they pass through a process of peaty fermentation, which produces ulmic acid. It is only in the second place that this ulmic acid is transformed into coal, mingling with all the coal-forming substances produced by the contents of the cells.—*The Colliery Guardian, being translated from L'Houille.*

VI. THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

Sheffield Meeting, August, 1879.

(Continued from p. 641.)

After the delivery by Prof. Johnstone Stoney of his admirable Address to Section A, an abstract of which concluded our report in the "Monthly Journal of Science" for September, the Report of the Committee—consisting of Prof. Sir William Thomson, Prof. Clerk-Maxwell, Prof. Tait, Dr. C. W. Siemens, Mr. F. J. Bramwell, and Mr. J. T. Bottomley—for commencing Secular Experiments upon the Elasticity of Wires, was read by Mr. Bottomley. The most important of the experiments conducted during the past year are those that have been made on the elastic properties of very soft iron wire. The wire used was drawn for the purpose, and is about No. 20 B.W.G., its breaking weight, tested in the ordinary way, being about 45 lbs. This wire has been suspended in lengths of about 20 ft., and broken by weights applied, the breaking being performed more or less slowly. During the breaking it was found that the wire becomes alternately more or less yielding to stress applied. Thus, from weights applied gradually, between 28 lbs. and 31 lbs. or 32 lbs., there is very little yielding and very little elongation of the wire. For equal additions of weight, between 33 lbs. and about 37 lbs., the elongation is very great. After 37 lbs. have been put on the wire seems to set stiff again, till a weight of about 40 lbs. has been applied; then there is rapid running down till 45 lbs. has been reached. The wire then becomes stiff again, and often remains so till it breaks.

The Report of the Committee for making more accurate determinations of the mechanical equivalent of heat stated that the work in progress was the protracted one of supplying a means of correcting errors in the determination of the temperature arising from the temporary changes of the fixed points of thermometers constructed of glass. An extensive series of experiments had recently been made by Prof. H. A. Rowland, of Baltimore, who, being unaware of what had been done by the Committee, had arrived at an equivalent almost identical with that determined by Mr. Joule.

The Report of the Committee for procuring Reports on the Progress of Mathematics and Physics contained two communications to the Committee, the one from Prof. Clerk-Maxwell, F.R.S., and the other from Prof. Stokes, F.R.S., giving their opinions on the questions with which the Committee has to deal.

The Twelfth Report of the Committee on Underground Temperature was read by Prof. J. D. Everett, F.R.S. One of the methods employed for taking underground temperatures is the lowering of maximum thermometers into deep bore holes filled with water. Another mode is to take observations in mines and collieries of the readings of very slow-acting thermometers. The experiments referred to include those of Dr. Stapff, made in the St. Gothard Tunnel. The mean temperature gradient for the whole length of the tunnel (about 9 miles), was found to be 1° F. for every 88 feet, but Dr. Stapff found that where the surface of the upper ground is a steep ridge the temperature gradient is less rapid than that, which is quite consistent with the hypothesis that the earth was at one time very much hotter than it is now, and that it is undergoing a process of cooling down.

The Report of the Committee on Atmospheric Electricity in Madeira was read by Dr. M. Grabham. Observing the regular winds and breezes, Dr. Grabham traced the steady rise of electricity in the early morning to a maximum at 11.30 a.m., which declines after much steadiness for two hours, at first suddenly, and then very gradually towards night.

The Report of the Committee on Luminous Meteors—read by Mr. James Glaisher, F.R.S.—dealt in detail with the accounts of conspicuous detonating fire-balls that had occurred in the United States on August 11th and December 18th, 1878, and on January 27th, 1879; in Bohemia and Saxony on January 12th, 1879; and in England on February

22nd and 24th, 1879, the real paths of all of which had to a greater or less degree of certainty and closeness been approximately ascertained. A description of the past year's aërolites was also given. The expected return of Biela's comet to its perihelion in the present year, leading a shower of shooting stars to be looked for on the 27th of November next, is to be taken advantage of to report next year on meteor showers.

The Report of the Committee on Instruments for Detecting Fire-Damp in Mines was read by Professor G. Forbes, F.R.S.E. Two new instruments have been constructed: the one of a large size, and worked by an electric battery; the other was small, portable, easily worked, and it answered all the purposes for which it was required. Both instruments were founded upon the facts that sound travels quicker in light gases than in dense ones, and that air which is contaminated with fire-damp is lighter than pure air. The velocity of sound in different qualities of air was compared by noting the lengths which must be given to a brass tube to cause it to resound to a tuning-fork. The accuracy of the instrument was such that the percentage of fire-damp could be determined with an error of considerably less than one per cent. The committee had descended the Wharncliffe Silkstone Colliery, in the neighbourhood of Sheffield (this pit has a depth of 200 yards), and were taken to a disused part of the mine where it was known there was a blower. Gas in sufficient quantities were found, and the instrument registered gas with more readiness than the Davy lamp. But the greatest quantity registered was 6 per cent, or twelve times the smallest quantity which the indicator detects. From the experiments they could assert that this instrument was capable of detecting and measuring fire damp even in small quantities.

The paper which, perhaps, attracted the most attention in Section A was that by the Rev. S. Earnshaw, M.A., on "Etherspheres as a Vera Causa of Natural Philosophy." In defining the term "ethersphere," Mr. Earnshaw said all space not filled by matter is pervaded by ether, so that every atom of matter is surrounded by ether, but this is not what is included in the word "ethersphere." If any portion of space be rendered void by ether from any cause whatever, that space has become void of the repulsive forces which were centered within it, and that, consequently, when these forces are taken away the medium outside the space will draw closer towards that space; and if the space be occupied by an atom of matter, the density of the sur-

rounding ether will be greater than before, and the ether, being in contact with the atom at its surface, will press upon it. The *excess* of ether about the vacant space above its original quantity constitutes the ethersphere; and though this gathering together of ether about the space now occupied by the atom is a consequence of the presence of the atom, it is in no way owing to its action on the ethereal medium. Mr. Earnshaw then argued that if every material atom, so must every compound system of atoms, *i.e.*, every material body, whether gaseous, liquid, or solid, have an ethersphere, which not only surrounds the whole body, but also penetrates the interstitial spaces of the body which lie between its atoms. By means of these etherspheres the author believes the phenomena of heat may be satisfactorily accounted for, on the supposition that the ethereal medium is the medium of heat as well as of light. He therefore concludes that etherspheres constitute a *vera causa* the existence of which in Nature is as certain as is that of the ethereal medium itself, about which no philosopher expresses doubt in the present day.

Mr. Gordon read a paper on "Secular Changes in the Specific Inductive Capacity of Glass." Mr. Gordon's experiments have led him to the conclusion that in the course of a year and a half an actual change had taken place in the glasses he used, which was shown by a considerable real increase in their specific inductive capacities. These experiments had some importance as regards Prof. Clerk Maxwell's electro-magnetic theory of light. In a recent lecture he (Mr. Gordon) had suggested that it was quite possible that the relation between electric induction and light exists—namely, that they are disturbances of the same ether, but that there is some unknown disturbing cause affecting the electric induction. Possibly a clue to the nature of this disturbing cause may be found in the fact that the specific inductive capacities were affected by some of the changes which chemists assert are constantly going on in glasses, but that these changes do not affect the refractive indices.

Prof. Johnstone Stoney read a paper "On the Cause of Bright Lines of Comets." Dr. Huggins and others had seen the bright lines of the carbon spectrum in the spectra of several comets. This established the fact that some compound of carbon was present in comets. Mr. Stoney suggested that the bright lines were due, not to the incandescence of the carbon compound, but to the sun's light

falling upon the compound of carbon and rendering it visible, in the same way that light renders the moon, the planets, and other opaque objects visible, the vapour of carbon being opaque in reference to the particular rays, which appear as bright lines in its spectrum.

Dr. Janssen, Member of the Institute of France, and Director of the Observatory, Meudon, read a paper "On Photographic Spectra of Short Exposure," in which he showed that in the solar spectrum the point of maximum actinic intensity was near the line G, while the point of maximum luminous intensity was near D. It occupied sensibly the same position for nearly all photographic substances and for lenses of different material. The time of exposure was graduated from five minutes to a small fraction of a second. By this means results were obtained which practically afforded a chronometric method of analysis of the solar spectrum. They had a bearing on the question of oxygen in the sun, and they led to practical improvements in the construction of photographic apparatus.

Mr. H. Courtenay Fox read a paper "On the Synchronism of the Mean Temperature and Rainfall in the Climate of London. The paper was accompanied by copious tables exhibiting the months and seasons for sixty-seven years, arranged in the order of their respective temperature and rainfall as for the Royal Observatory. The following is a short summary of the results:—1. In each of the four months from November to February, extreme cold tends to be synchronous with dryness, warmth with large rainfall. 2. In the summer months, June to August, cold tends to be accompanied by much rain, warmth by dryness. 3. To put this in popular language, rain brings warmth in winter and cold in summer; that is (if rain be the cause, which is by no means proven) it mitigates the special character of each extreme season, winter and summer. 4. Very wet years tend to be either cold or warm, whilst years of drought tend to assume an average temperature.

A paper "On Lightning Protectors for Telegraphic Apparatus" was read by Mr. W. H. Preece. For many years it was not the practice in England to protect telegraphic apparatus from the injurious effects of atmospheric electricity because the damage done was so insignificant, and because the remedy was found to be worse than the disease. But as telegraph systems increased lightning protectors became essential. Many forms were tried based on the fact that when a discharge takes place through a non-conductor, such

as dry air, at the moment of discharge the resistance along the line of discharge is practically nothing, and therefore all the charge is inducted away. Most of those tried failed. The survival of the fittest has been exemplified in the "plate" protector. In this form—one of the earliest introduced—one thick plate of brass is in connection with the earth, and another similar plate in connection with the line is placed above it, but separated from it by paper or by insulating washers. The lightning entering the wire, bursts across the paper or air-space in preference to passing through the apparatus, and thus escapes to earth. It is the practice of the Post-Office department to keep these plates apart by thin paraffined paper 0·002 inch thick. The paper contains an account of some important experiments by the author on plate-protectors, which confirm very decidedly the accuracy of the figures obtained by Drs. Warren de la Rue and Müller on the striking distance between two flat disks given by them in a paper read before the Royal Society.

Section B.

The Chemical Section (B) was presided over by Professor Dewar, F.R.S., who, in his opening Address, remarked that the time was past when the President could give a review of Chemical Work from year to year, so he took a broad view of the progress of Chemical Science. Referring to Messrs. Thomas and Gilchrist's process for the elimination of phosphorus from pig-iron, Prof. Dewar said that by far the greater portion of iron ores contain very appreciable quantities of phosphorus. Of the total phosphorus which goes into a blast-furnace, whether with the ore or with the limestone, nine-tenths are contained in the resulting pig-iron. The Bessemer process did not eliminate phosphorus from pig-iron treated by it, and extremely small proportions of phosphorus render steel "cold short," so that the process had hitherto only been applicable to the rarer and costlier kind of pig-iron produced from exceptionally pure ore. Even thus restricted the Bessemer process had produced results of incalculable importance, and the imagination could scarcely grasp the extent of the further development it must receive and the importance of the further benefits which must now flow from it.

We have not sufficient space to refer to the many valuable papers brought before this Section. We must, however, mention a note by Mr. J. Norman Lockyer, "On the Sup-

posed Compound Nature of the Elements." Continuing his researches on this subject, he found that when carefully distilled metallic sodium is condensed in a capillary tube, placed in a retort, and heated in a Sprengel vacuum, it gives off twenty times its volume of hydrogen. Phosphorus, carefully dried, gives off 70 volumes of gas, chiefly hydrogen. A specimen of magnesium, carefully purified by Messrs. Johnson and Matthey, was magnificent in its colourings; it gave first hydrogen, then the D line, then the green lines of magnesium (*b*), then the blue line, and then, as the temperature was increased, various mixtures of all of them, the D line being always the most brilliant. In this experiment only two volumes of hydrogen were collected. From gallium and arsenic no gas is given off. From sulphur and some of its compounds sulphurous anhydride was always obtained. From indium, hydrogen was given off before heating; while from lithium no less than 100 volumes of hydrogen were given off.

Dr. Gilbert, F.R.S., read a paper "On some Points in connection with Agricultural Chemistry," in which he stated that Mr. Lawes, at Rothamsted, and himself, had now grown wheat for 36 years in succession on the same land, barley for 28 years in succession, oats for 10 years, root crops for more than 30 years, beans for more than 30 years, and they had experimented with mixed herbage on grass land for 24 years. They found only minor distinctions in the manurial requirements of different plants of the same natural family, but very great distinctions in the requirements of plants of different natural families. The gramineous crops are very low in the percentage of nitrogen, and yield but a very small quantity per acre. Yet nitrogenous manures are very effective when applied to such crops. Leguminous crops, on the other hand, are very high in the percentage of nitrogen, and yield a very large amount of it per acre. Yet nitrogenous manures are of little avail to these plants, and potash manure is especially effective. The differences in manure requirements of plants of all natural families were also pointed out. Much more complicated, however, was the problem when experiments were made upon mixed herbage of grass land, where they might have fifty or more species growing in association, representing perhaps twenty natural families. One point of especial interest was the difference in the amount of nitrogen yielded by the plants of the different natural families. It was assumed by some that some plants assimilated the free nitrogen of the atmosphere; but so far as existing evi-

dence went the author considers it much more probable that the soil is the source of the combined nitrogen. Experiments show that by the growth of wheat or barley for many years in succession on the same land without nitrogenous manure, the annual yield of nitrogen in the crop gradually diminishes. There was also a diminution in the percentage of nitrogen in the soil. In the case of the root crops the diminution in the percentage of nitrogen in the soils was also greater. With beans there was also a diminution in the yield of nitrogen in the crop, but still much more was yielded, even at the conclusion, than with either wheat or barley. In these cases there was not found a reduction of nitrogen in the soil. In the case of mixed herbage experiments, very much more nitrogen was yielded by the application of potash manure, and here there was a great reduction in the percentage of nitrogen in the soil. The percentage of nitrogen in the soil was also very largely reduced in the case of clover grown for many years in garden soil. Part of this reduction might be due to other causes, but the indication was that the Leguminosæ had derived their nitrogen from the soil. Admitting that the sources of the whole of the nitrogen of vegetation were not conclusively made out, he nevertheless considered that the existing evidence was against the idea of the assimilation of free nitrogen by plants, and in favour of an opinion that free nitrogen was mainly, if not entirely, derived from the soil.

Section C.

The President of the Geological Section (C), Professor P. Martin Duncan, in his Address, said that of all the geological formations the carboniferous gave the earliest clear and definite idea of a land surface on the earth, or rather of the existence of many lands. In reflecting upon the history of the carboniferous deposits, in relation to the subsequent great changes in the physical geography of the earth, the idea that geological histories repeated themselves did not obtain that importance with which it is credited in relation to human events. It was true that there were extensive triassic, oolitic, wealden, neocomian, and tertiary lands, whose vegetation had been metamorphosed into a kind of coal; but the wonderful depth, and the extraordinary vertical repetition of organic and inorganic deposits of the carboniferous formation, and the remarkable crust movements which enabled them to accumulate, were without subsequent examples.

The Sixth Report on the Conductivities of Coral Rocks, prepared by Prof. A. S. Herschell and Prof. G. A. Lebour, was read. The research and correspondence necessary to complete a historical sketch of the attempts hitherto made to determine experimentally the thermal conductivities of the terrestrial rocks most widely distributed, which the Committee had proposed to draw up during the past year, were not so far advanced at present as to allow them to be comprised in this year's report. The Committee hopes, by continuing its inquiries for another year, with the addition to its number of Prof. W. E. Ayrton and Mr. J. Perry, of the Imperial College of Engineering in Japan, to carry out the object of their undertaking, so as to exhibit our existing knowledge of the date of thermal conductivity of those widespread kinds of rock which constitute the external materials of the globe.

The Fifteenth Report of the Committee for Exploring Kent's Cavern, Devonshire, was read by Mr. W. Pengelly, F.R.S. He observed that this was probably the last report he should present on the subject, as the exploration work was fast drawing to a close. Work during the past year has been carried on in the "High Chamber" and its branches. This "High Chamber" contains only breccia, the oldest mechanical deposit in the cavern, and the crystalline stalagmite which overlies it. Bones of bears and implements have been found in the breccia here, and also in that of the southern branch, called the "Swallow Gallery."

Section D.

The Biological Section was presided over by Professor St. George Mivart, F.R.S., who, in his Presidential Address. referred to the life and labours of Buffon, noticing his speculations concerning animal variation; his belief in the direct effect of the surrounding circumstances on organisms; and also his belief that new species have arisen by degradation. We may, Prof. Mivart thought, accept as true two propositions:—(1.) Animals may change in various ways, and amongst them, by degradation. (2.) Changes in the environment with isolation, induce and favour changes in form. He urged that inquiries should be pursued in two directions simultaneously. (A.) There might be undertaken one set of inquiries to investigate the effects on different species of the same variation of environment. (B.) Other inquiries might be undertaken with a view to ascertaining

the effects of different changes of environment on one and the same species. The President then directed attention to the resemblances and differences which exist between the mind of man and the higher physical faculties of animals.

In the Department of Anatomy and Physiology the President was Dr. Pye-Smith, F.R.S., whose address attracted great attention. Biology, he remarked, is the science of the structure, the functions, the distribution, and the succession in time of all living beings. If the proper study of mankind be man, he has learnt late in the inquiry that he can only understand himself by recognising that he is but one in the vast network of organic creation; that intelligible human anatomy must be based upon comparative anatomy; that human physiology can only be approached as a branch of general physiology, and that even the humblest mould or sea-weed may furnish help to explain the most important problems of human existence. Speaking of the relation of physiology to the national health Dr. Pye-Smith said that if the art of keeping a community in health is but the application of plain physiological laws, it is no less true that the art of restoring the health, curative as distinct from preventive medicine, rests upon the same basis. We now know that disease is, as the name implies, a purely subjective conception. The disease of a host is the health of the parasite, and we cure a human sufferer by poisoning the animals or plants which interfere with his comfort. The same changes which in the old man are the natural steps of decay, the absence of which after a certain age would be truly pathological, are the cause of acute disease in the young. Pathology has no laws distinct from those of physiology. It clearly follows that all "systems of medicine" are in their very nature condemned. All that the art of medicine can do is to apply a knowledge of natural laws, of mechanics, and of hydrostatics, of botany and zoology, of chemistry and electricity, of the behaviour of living cells and organs when subjected to the influence of heat and of cold, of acids and alkalies, of alcohols and ethers, of narcotics and stimulants, so as to modify certain deviations from ordinary structure and function which are productive of pain, or discomfort, or death. Rational medicine, or keeping right and setting right the human body, must therefore rest upon a knowledge of its structure and its actions, just as a steam-engine or a watch cannot be mended upon general principles, but only by one who is familiar with their construction and working, and who can detect the source of their irregularity. With regard to the

endowment of research in biology, Dr. Pye-Smith confessed that he should be sorry to see it undertaken by Government funds. This country is not so poor, nor our countrymen so wanting in public spirit, that we need appeal to the national purse to supply every ascertained want. Great as is the national importance of science, the nation was more important still; and even if that were the alternative he would rather that we should indefinitely continue dependent on Germany for our knowledge than give up the local energy, the unofficial zeal which has made England what she is. Coming to Vivisection Dr. Pye-Smith referred to the reasons which animate much of the opposition to Vivisection, many of which we have discussed in the "Monthly Journal of Science." With regard to the recent legislation he said that a system of licenses and certificates, revocable at the will of a Minister who may, by the accidents of party, be at any time amenable to anti-scientific influences, adds serious difficulties to those already in the way of experiments. As an illustration, he asked his audience to suppose that certain persons opposed on various grounds to learning, and especially hostile to Greek, had attacked the study of Plato. They would point out the danger of modern ladies becoming as well read in his writings as was Lady Jane Grey. They would show that the laxity of modern manners was coincident with the popularity of the "Symposium," and that the notorious increase of infanticide was the result of the teaching of the "Republic." Associations for the total suppression of Plato would be formed, with hired advocates, and anonymous letters, and "leaflets," spreading a knowledge of his most objectionable passages. Scholars would be threatened with eternal punishment, and schoolmasters with the withdrawal of their pupils. Then a royal commission would be appointed—a great Latin scholar, a Whig and a Tory statesman (who, having taken a B.Sc. degree at Oxford would be impartially ignorant of Greek) the most intelligent despiser of Plato who could be found, the master of a grammar school on the modern side, and (perhaps the most efficient of all) a lawyer, who knew nothing about Greek but hated cant. This commission would take evidence that the Platonic writings were not all immoral, that they had been quoted with approval by Fathers of the Church, that they were of great importance to literature and philosophy, and even to the elucidation of the Sacred Writings. It would also be proved that the Platonic Dialogues were far less immoral than multitudes of other widely circulated books, or than a French novel

which one of the royal commissioners happened to be reading, and, lastly, that the morals of Greek scholars, and of clergymen who had read Plato at college, were not obviously degraded below those of other people. On the other hand, witnesses would depose that a knowledge of Plato was of no consequence to a student of philosophy; that if it were, the text was in so corrupt a condition that no two scholars agreed as to a single chapter, and that, after all, philosophy was of no practical use, least of all to clergymen. Others would affirm that though they had never read a line of him, they knew that his style was as vicious as his sentiments; and perhaps some cross-grained scholar might be found who, having once edited a play of Euripides, would declare that all studies in Greek literature ought to be restricted to the tragedians, and that for his part he had never opened any other authors, and had never felt the want of them. At last the commission would report that there was no question of the value of the works of Plato, that it would be mischievous and impracticable to prohibit their study, and that there was no evidence that schoolmasters habitually chose the least edifying passages as lessons for boys. Then what is called a compromise would be made. It would be enacted that Plato might be read, but only in colleges annually licensed for that purpose; that every one wishing to read must have a general certificate signed by certain professors, and setting forth his object, also to be renewed every year; and that special certificates might be severally obtained for reading certain excepted dialogues, for copying from them, for publishing them, or, in rare cases, for translating them. However reasonably such a system might be administered, who can doubt the result would be a diminution of the number of scholars, and a check to the progress of learning? Now this is what legislation has done for physiological experiments. The Act (39 and 40 Victoria) was hastily drawn and hurriedly discussed; for noble lords and honourable gentlemen who had been taught from childhood to vivisect for unscientific purposes were eager to hurry off to their own merry vivisections, for which they were ready provided with licence and certificates. And it works as might be expected. Some shrink from seeing their names figure in disreputable newspapers, and receiving more or less savagely abusive anonymous letters. Others have no laboratories, and find difficulty in licensing their houses. Others are refused the certificates they require. In one case two thoroughly qualified men were anxious to carry out an important investigation on the treatment of snake-bites. They

procured venomous snakes from a distance, and applied for the special certificates necessary. Considerable delay ensued; various objections were raised, and set at rest; and at last all the certificates were obtained; but meantime the snakes had died."

In the Department of Botany Sir John Lubbock read a paper on "Seeds." He directed attention to the difference in seeds, some being large, some small, some covered with hooks, some provided with hairs, some smooth, some sticky, &c. Many seeds required protection from birds and insects, hence the shells or husks of the beech, Spanish chestnut, horse chestnut, walnut, &c. In some cases, as in the common herb, the calyx closed over the seeds when the flower faded, and opened when the seeds were ripe. In other cases the flower stalk changed its position. The modes of dispersion by means of which seeds secured a sort of natural rotation of crops, and were also in other cases enabled to rectify their frontiers, were also described. Some plants threw their seeds. Among the higher plants the seeds were transported by the wind. Many seeds were provided with a wing which caught the wind, and these and kindred aids in dispersion were as various as the plants themselves. The dispersion was often effected by the agency of animals. In some cases the action of animals was involuntary, and these might be divided into two classes, those in which the seeds adhered to animals by hooks and those in which this was effected by sticky glands. The seeds of a South African plant, provided with hooks more than an inch long, were said sometimes even to destroy lions. They rolled about on the sandy plains, and if a seed attached itself to the skin the wretched animal tried to tear it off, and getting it into his mouth was fatally injured. Sir John, in conclusion, called attention to mimicking seeds, such as the scorpiurus, the pods of which did not open, but looked so exactly like worms that birds were induced to peck at them, and thus the seeds were freed.

A paper on the effects of the frosts of 1860-61 and 1878-79 on vegetation was read by Mr. E. J. Lowe, F.R.S., from which it appeared that the greatest cold of 1860 exceeded that of last winter by 10 degrees, being 6 degrees below zero in 1860, whilst it was 4 degrees above zero during the late frost. Instead of the cold killing the slugs and various pests of plants they were never known to be so numerous. Many hardy plants in pots were killed when they escaped if planted in the ground.

Section F.

We must not omit a notice of two papers brought before the section of Economic Science and Statistics, the one by Dr. J. H. Gladstone "On Elementary National Science in Board Schools;" the other by Prof. Leone Levi "On the Scientific Societies in Relation to the Advancement of Science in the United Kingdom."

Dr. Gladstone pointed out that in elementary schools a knowledge of the facts of nature is generally given in two very different ways. In the infant department there usually lingers some remnants of that instruction by object lessons which was considered a valuable part of education before the Revised Code of 1861. In the higher standards of the boys' and girls' departments certain sciences may be taught as "specific subjects," and receive encouragement by a Government grant. At the present time out of 1074 male and 1790 female teachers, 888 males and 442 females hold advanced science certificates, varying in number from 1 to 23. Advanced object lessons, generally on natural history, are taught in many of the boys' and girls' departments, and there is little doubt that they will soon become much more general and systematic. Out of 248 boys', 218 girls', and 46 mixed schools, more than half include in their course of instruction scientific specific subjects. The cost of books and apparatus for the instruction in natural knowledge during the past twelvemonth was £834. This amount is rather less than one penny per child per annum.

Mr. J. F. Moss advocated the establishment of centres, at which this class of work could be done by selected teachers and under far more favourable conditions than at present existed. Branches of science more immediately bearing upon the industries of each district should have special prominence so as to aid in the training of intelligent artisans, foremen, and managers instead of depending so largely for the supply of educated men from other countries where greater attention had been given to technical education with such important results.

Prof. Silvanus Thompson advocated apprenticeship schools, but if such schools were established in England it should be by local rather than Imperial effort, for they succeeded better when they were not fettered by Imperial legislation.

The President (Mr. Mundella) believed that science teaching tended to redeem school life from its drudgery and monotony. In the science schools abroad the interest manifested by the children in a proper object lesson and the

facility with which they acquired knowledge had very much struck him.

In his Address on Scientific Societies, Prof. Leone Levi said that in the seventeenth century there were only two scientific societies in this country, but at the present time the calendar exhibited an amount of activity quite unknown at former periods. In 1878 the number of members for the Royal Society was 549; for the Royal Society of Edinburgh, 429; and for the Royal Irish Academy, 328. The societies devoted to physical and mathematical sciences had, in 1878, 5406 members; the natural history sciences, 16,534; the archæological and geographical societies, 5038; and the societies whose object is the study of the applied sciences, 21,947. The amount voted by the State in 1878 for education, science, and art amounted to £4,153,000. If, however, they eliminated from the total vote the amount expended for elementary education, the proportion left for science and art was considerably diminished, amounting only to £529,000. Government aid was principally given to physical and natural science, leaving a wide range of scientific exploration altogether unassisted. It was not to be desired that Science should be subsidised by the State, but the claim of Science had been fully recognised.

The Chairman (Mr. Shaw Lefevre, M.P.) agreed with the speakers that the multiplication of members of scientific societies did not necessarily indicate the growth of excellence in Science and an increase in the number of scientific men. They really required some standard, for there was no doubt that the members of some learned societies placing so many letters after their names was just an indication that they could subscribe a certain number of guineas in the year. The Royal Society had taken a step in the right direction in limiting the number of its Fellows. He always thought the letters "F.R.S." a much greater distinction than "M.P.," for it was impossible that that honour could be acquired except by the possession of great and solid attainments.

Section G.

The President of the Mechanical Science Section (Mr. J. Robinson) chose for the subject of his Address the "Development of the Use of Steel during the last Forty Years," considered in its Mechanical and Economic Aspects." Referring to the enormous reduction of price, and consequent more frequent and more economic application of steel, the President said that, following the initiation of Krupp, our

English engineers and men of science set themselves to work to discover and apply new processes for the production and manufacture of this most wonderful metal; and in the whole history of metallurgy, from the time of Tubal Cain downwards, there has been no such progress in invention and manufacture as has been realised by the aid of such men as Mushet, Krupp, Bessemer, Siemens, Whitworth, Martin, Vickers, Bell, Bauschinger, Styffe, and many others within the last forty years.

An important paper was read by Captain Bedford Pim, "On the proposed Canal across the Isthmus of Panama." He estimated that the canal would not cost more than ten millions.

A paper "On Leon Francq's Fireless Locomotive" was read by M. C. Bergeron. The idea of such a locomotive was originated at the time of the horse plague in New Orleans, in 1872. After the Franco-German war M. Francq, who was connected with the Paris tramways, gave his attention to discovering a motive power for his tram-cars. The object he had in view was in obtaining such heat from water as would supply the motive power he needed. The engine in question was the result of his investigations. It had a large cylindrical reservoir, surmounted by a steel dome, two cylinders acting with piston rods, and a crank carrying two driving wheels, which were connected with other two wheels. The reservoir holds more than 700 gallons of water, heated to such an extent as to produce a pressure of 224 lbs. to the square inch. It was claimed for the new engines that they were very economical in every respect. It was pointed out that the engine would be especially useful on the Metropolitan lines, as it neither gave off smoke nor deleterious gases. In a few weeks an engine of this character will be used on the tramway at Leeds and another at Liverpool Docks.

At the concluding General Meeting of the Association it was announced that the Meeting in 1880 will be held at Swansea, the President-Elect being Prof. Ramsay, LL.D., F.R.S.

NOTICES OF BOOKS.

A Manual of the Geology of India. Chiefly compiled from the Observations of the Geological Survey. By H. B. MEDLICOTT and W. T. BLANFORD. Calcutta: Geological Survey Office. London: Trübner and Co.

WE have had, from time to time, the pleasure of drawing the attention of our readers to the important results worked out by the Geological Survey of India. The memoirs and reports of the Survey have become too numerous and bulky for general consultation. Much information on the Geology of India is scattered in various learned journals, both Indian and European. Hence a digest or compendium, showing at least the more important facts observed and the chief conclusions arrived at, has become imperatively requisite, and we are glad to learn that the Government has seen this necessity, and has directed the present Manual to be drawn up.

The first volume is devoted to peninsular India, whilst the second treats of the extra-peninsular portions, such as Sind, the Punjab, the Himalayan and Sub-Himalayan regions, and British Burma. Concerning this extra-peninsular area it is remarked that it is "geologically an intrinsic portion of the Asiatic continent, whilst peninsular India is not."

The authors find ample evidence that India was affected by the cold of the Glacial epoch. The poverty of the living fauna as compared with the fossil fauna of the Siwaliks, as Mr. Wallace happily suggests, is best explained by a secular decrease of temperature. The eleven extinct elephants and mastodons are now represented by a solitary living form. Against fifty known fossil species of ungulates the same area affords only eighteen recent forms. The Tálchir formation displays the same kind of evidence by which the existence of a Glacial epoch is recognised in more northern regions.

As regards the former distribution of land and water, the authors conclude that in Eocene times peninsular India was part of a tract of land, perhaps even of a great continent connected with Africa. To the east and north-east was a sea, where now rise the Assam hills, whilst on the north-west another sea covered great part, if not the whole, of Persia, Baluchistan, the Indus plain, and part of the plain of the Upper Ganges. The recent fauna, they believe, speaks in favour of the connection of Southern India with the Malay Islands and Africa in the early Tertiary times.

The authors do not entirely agree with the system of anima

geography propounded by Mr. Wallace. On comparing the mammalian fauna of the Oriental region with those of the three neighbouring regions, they consider that the strongest affinities are with the most distant of the three—the Ethiopian. This, they hold, points to the existence of an older fauna once common to Africa and India, though now partly replaced in either country by newer types. It has been supposed that Ceylon, with the southern portion of the Indian peninsula, may have been united with the Malay countries later than with Africa. As far, however, as the sea-bottom between Ceylon and the Malay Islands is known, there is nothing to prove a recent extension of land in this direction. The affinities of the land-shells of India are rather with the Australian than with the African fauna. As these molluscs are supposed to be of very high antiquity, the resemblance may be due to a communication existing in the Mesozoic times.

The existence of a former continent, Lemuria, stretching from Madagascar to the Malay Islands, and connected at times with Africa, and at times again with Asia, still remains a speculative question. But that the interval between Africa and India was once bridged over, so as to admit of the easy interchange of mammalian species, scarcely admits of doubt.

As regards its soils, the authors state that India may be very roughly divided into the extra-tropical wheat-region ; the damper portion of tropical India—the rice, sugar, &c., country ; and the drier tropical parts—the so-called black-soil country, suitable for the cultivation of cotton. The wild floræ of these three regions are respectively as distinct as are the cultivated crops.

Pre-historic implements, bearing marks of human or anthropoid industry, have been found in India to a considerable extent. Chipped palæolithic weapons were first found near Madras by Messrs. Foote, King, and Oldham, and have been subsequently met with in Orissa, Bengal, and Assam. Knives made from agate, flint, or chert are possibly of more recent origin than the quartzite implements. Similar flakes are still in use among the natives of the Andamans. Neolithic stone implements—so-called "Celts"—with surfaces smoothed by grinding, have been noticed at Kirwi, Chutia Nagpur, and Kurg. A single bronze axe has been discovered near Jabalpur. Copper weapons are more numerous, but still scarce. Iron implements are abundant, and the authors suggest that the art of working this metal may have been discovered earlier in India than in more western regions. They consider that closer and more extended research in India will doubtless bring to light important evidence on the earlier career of the human species.

The work is illustrated with a judicious assortment of figures of Indian fossils of different epochs, and is further provided with an Appendix in the shape of a very large geological map, which forms a companion volume.

CORRESPONDENCE.

WEATHER-INDICATIONS.

To the Editor of the Monthly Journal of Science.

Sir,—Have any of your meteorological correspondents noticed a fact with which I have often been struck,—that is, that in a season of confirmed character the ordinary weather-indications become of no value? Thus in 1868 I often observed that lowering clouds, a red sunrise, the low flight of swallows, eddies of dust, &c., were not followed by rain. Conversely in the present season, a rising barometer, red sunsets, very lofty flights of swallows, and other indications of fair weather, are succeeded by rain.—I am, &c.,

Cloud-gazer.

ANTI-VIVISECTIONIST INCONSISTENCY.

To the Editor of the Monthly Journal of Science.

Sir,—In a recent magazine article a distinguished writer, after describing with great zest a day's trout-fishing which he enjoyed at Cheneys, enters upon the defence of "sport" in general, which he defines as killing for the sake of killing. He ventures on the unwarrantable statement that save "the dogs and the cats which have learnt from him," man is the only animal which indulges in such gratuitous slaughter. Waiving, however, any present discussion on this point, I cannot help remarking that to indulge in and to justify "sport" come with a very bad grace from one who, like the writer in question, is an avowed opponent of vivisection.

If the common infliction of pain and death upon animals for mere amusement, or for the exercise and display of skill, is to be tolerated without rebuke either from law or from public opinion, how can a much rarer infliction of pain for a high and important purpose be branded as a horrible crime?—I am, &c.,

J. W. Slater.

NOTES.

Biology.

M. E. Renon predicts a series of bad seasons until 1883, when the summer is to be warm. The winter of 1881-82 will be exceptionally severe. He assumes a period of 41 years, the beginning and end of which are characterised by bad weather. Referring to the fact that in the past July, as in July, 1758 and 1816, the temperature in Western Europe was exceptionally low, though accompanied by south and south-west winds, he supposes that the African current, in place of taking its usual course over Spain, France, &c., has been deflected to the east; thus accounting for the great heats experienced in Italy, Hungary, Bulgaria, &c.

In a paper on the "Production of Electricity by the Rays" M. Ch. Robin states that, as early as 1865, he demonstrated that the electric apparatus in the tail of the Ray acts in the same manner as those of the Torpedo and the Gymnotus, the differences between them being merely of a secondary nature. Quite recently he has verified anew the exactitude of his former observations.

"La Correspondance Scientifique" gives an interesting account of the museum founded at Cette by M. J. B. Doumet, a young French officer who, in 1815, was dismissed from active service on account of his suspected attachment to the Empire. It is exceedingly rich in the mollusks, crustaceans, fishes, and reptiles of the Mediterranean and of the adjacent countries. The collection of exotic Lepidoptera is also very fine. One of the greatest curiosities is the original herbarium of Michel Adanson, the celebrated botanist, the maternal grandfather of Doumet.

P. H. Reinsch has observed the occurrence of the mycelia of fungi in a hen's egg ("Botanische Zeitung," 1879, No. 3, pp. 37, 38).

M. A. Chaveau, in the course of experiments described in the "Comptes Rendus," observes that certain Algerian sheep were perfectly proof against the poison of carbuncle, even on repeated inoculation—a fact which raises a number of most important questions.

Professor K. Th. Liebe, in the "Transactions of the Imperial Academy of Sciences of Vienna," contends that during the second diluvial period the hills of southern Bohemia and Moravia were the starting-point from which the virgin forests invaded the great diluvial steppe of Central Europe to the north of the Alpine chain.

M. B. Schnetzler communicates to the Academy of Sciences certain observations on *Arum crinitum* and its relations to insects. Its carrion-like odour entices numbers of flies, such as *Musca Cæsar*, which lay their eggs at the bottom of the spathe where the resulting larvæ necessarily perish. Common flies and even Acarides are often found imprisoned among the hairs. Some flies, however, less eager to lay their eggs, are attracted by the glandulous hairs upon the spadix, which lead them, like the steps of a ladder, to the stamens. There, on alighting upon the anthers, they cause the pollen to be emitted, and fly away to deposit their eggs in another spathe, at the bottom of which they deposit upon the stigmata the pollen brought from the former flower. The dead flies, though upon a moist surface, appear dried up. It appears that the hairs which overspread the inner surface of the spathe secrete an acid which, like that exuding from the hairs of *Droscra*, contributes to the transformation of the nitrogenised parts of the insects into matter absorbable by the spathe. Hence the author, without denying that insects contribute to the fecundation of this flower, contends that they contribute to its nutriment, and holds that it was rightly named *Arum muscivorum* by the younger Linnæus.

According to Ernesto da Canto, the smoke of burning straw or brush-wood has been found very beneficial to a variety of plants, but especially to the pine-apple. The observations were made in the hot-houses used in the Azores for forwarding pine-apples in the cold season.

In the "Rivista Orticale" hemp-plants are recommended to be cultivated in vineyards, orchards, &c., for the banishment or destruction of noxious insects. The absence of insects in hemp-fields is said to be a well-known fact.

Worms have been observed in fresh hens' eggs at New York. The species was not determined, but it may probably be *Distoma ovatum*, which lives parasitically in the cloaca of poultry.

In a paper communicated to the Academy of Sciences M. Richet shows that muscles in a state of contraction are more excitable than when in a state of repose. The relaxation of a muscle is not abrupt, but gradual, and the true form of the muscular shock is masked by the weights which extend the muscle. For muscles extended by a weight there is a period of *latent contraction*, during which the muscle is most excitable.

M. Poincaré has ascertained that guinea-pigs are affected by the vapour of nitrobenzol much more seriously than is man. Workmen exposed to these fumes in the manufacture of aniline rarely experience more than a transient loss of consciousness, which is at once removed on exposure to the fresh air. All the guinea-pigs experimented upon perished. On examination the liver, kidneys, nervous centres, and lungs were all found strongly congested. The chief symptom observed during life was a difficulty of breathing.

M. Léon Fournol, in the "Journal d'Hygiène," gives an interesting summary of our present knowledge concerning poisonous fishes, which appear to be a source of grave danger. We may trace here four distinct cases. There are fishes perfectly harmless at some seasons of the year, but poisonous at others. This has been observed in Japan with the common salmon, the bonito, and the albicore. The symptoms generally resemble those caused by the ingestion of corrupted meat, and though alarming are rarely fatal. Other fishes, such as *Lethrinus nambo*, are wholesome when young, but become poisonous when they reach a certain growth. Other aquatic animals—the conger, the pike, the barbel, the prawn, and the mussel—are occasionally and capriciously poisonous. In one instance a number of persons who had partaken of the spawn of a large barbel experienced very severe symptoms of poisoning, whilst one of the party who had not eaten any of the spawn escaped. The liver and the head in several species of fish inhabiting different parts of the world are found exceptionally dangerous. The occasionally poisonous action of mussels, oysters, &c., is ascribed by eminent authorities to their having eaten the eggs of the star-fish, which are occasionally found in these mollusks on dissection, and which, when handled, produce violent cutaneous irritation. Other fishes appear to be poisonous at all times and under all circumstances. Prudence dictates that the roe, liver, &c., of all unknown fishes should be carefully avoided, and it is very doubtful whether aquatic species of animals too small to admit of the removal of the alimentary canal, with its possible contents, should be eaten at all. The entire question offers a splendid field for research, both biological and chemical.

M. J. Künckel has published in the "Comptes Rendus" the results of an extended series of researches on the nervous system of the Diptera. He recognises in some cases an absolute centralisation, or an extreme dispersion of the nervous centres, with the most varied intermediate arrangements, but each family has its nervous system constructed upon a peculiar and invariable plan. The number of nervous centres varies, however, gradually from one family to another.

M. E. Brandt has laid before the Academy of Sciences certain interesting results concerning the nervous system of insects. He finds that in the genera Rhizotrogus, Stylops, and Hydrometra the subœsophagian ganglion is not separated from the following ganglia, as has been supposed to be the case in all insects. Hence this character can no longer serve to distinguish the nervous system of insects from that of the other Arthropoda. The pedunculated bodies of Dujardin, or the cerebral circumvolutions, are not only met with in some insects, but in all. In some insects a difference in the development of these convolutions is found, even among different individuals of the same

species. Wagner's assertion, however, that in the male bee these convolutions are entirely wanting, is inexact. They are present in all male insects, though in the social species in a much lower degree. The development of that portion of the brain named the hemispheres is proportional to the grade of intelligence, though that of the entire brain is not. In the Lepidoptera, Coleoptera, Hymenoptera, and Neuroptera the first thoracic ganglion is simple, and the second compound. In others (such as several dipterous genera), both the first and second thoracic ganglia are compound.

According to M. Arloing, the formiate of soda lowers the temperature of the animal system. It neither affects the heart nor the kidneys as profoundly as does the salicylate.

MM. Brissaud and Richet, writing in the "Comptes Rendus," point out that there is a very intimate connection between the normal muscular tone, catalepsy, cramp, myo-reflex contraction, and the contraction of hemiplegia, and that forms of transition between these states will, doubtless, be traced.

From experiments upon *Scyllium canicula*, M. Cadiat concludes that digitalin, given in a toxic dose, is a heart-poison, acting directly upon that organ and producing a tetanisation of the ventricle and a diastole of the auricle. It has no action upon the nervous centres, the peripheric nerves, or the muscles.

According to M. de Brévans, flights of swallows are sometimes seen going northwards as late as October, returning again to the south when leaving Europe. The author—as quoted in "Les Mondes"—remarks that this year the swallows have returned later than usual. This does not agree with observations made in England.

M. E. Heckel has observed a well-marked case of trichinosis in a young hippopotamus which died in the Zoological Gardens at Marseilles. He asks if the pachydermata are especially liable to a spontaneous development of this terrible parasite?

M. Dareste, having placed eggs in warm water of a temperature proper for incubation, found that all of them presented marks of incipient development, though in every case the embryo had perished. In one case only the structure had escaped decomposition, and it was a monstrosity of the class which the author has described as omphalocephalous. The heart, clearly distinguishable, was placed above the head.

Dr. A. W. Lyte, in the "Medical Brief," recommends the oil of sassafras as an effectual antidote for the bite of the copper-head snake, and as destructive to all insect life.

The application of osmic acid to microscopical purposes forms the subject of a paper read before the Royal Microscopical Society on the 12th of March by T. J. Parker, B.Sc. It is valuable principally on account of its property of killing and

hardening protoplasmic structures with the minimum of shrinkage, and also staining fat of an intense black colour. The former property renders it of great value in embryology and in the study of infusoriæ and delicate animal tissues : the latter gives it its pre-eminence in bringing out the ramifications of the finer medullated nerves, and the structure of the adipose tissue. Some recently-hatched glass-crab (Phyllosoma) were immersed for ten minutes in a 1 per cent solution of osmic acid, and subsequently treated with alcohol of gradually-increasing strength, commencing with 50 per cent, and ending with absolute. The central nervous mass was stained jet black; the muscles, glands, &c., assumed a greyish brown, the striæ of the muscular fibres becoming remarkably distinct. It has been used with great success for the preparation of delicate vegetable tissues; the manipulation is similar, after staining and treatment with alcohol; soaking in oil of cloves follows; the sections are cut by imbedding in cocoa butter, and mounted in balsam. A mixture of osmic and chromic acids (chromic acid, 0·25 per cent, 9 parts; osmic acid, 1 per cent, 1 part) answers, in some respects, better even than osmic alone for vegetable preparations.

In the same journal (vol. II. p. 354) is a paper by George Hoggan, M.B., and Frances Elizabeth Hoggan, M.D., "On the Development and Retrogression of the Fat Cell." The mode of preparation of the specimens is minutely described, and the manipulation and effect of the various staining and other reagents given in detail.

The employment of osmic acid and other stains is opening a new field for the histologist. The following process is used by Dr. Richard Altmann, of Giessen, for the investigation of certain animal tissues under the microscope:—The tissue is either impregnated by soaking, or else its vessels are injected with olive or castor oil, and then placed in osmic acid until the oil is hardened and blackened; finally it is transferred to a solution of potassium or sodium hypochlorite ("Eau de Javelle"), which completely destroys the tissue, the fatty material being left, and retaining the form of the vessels or spaces into which it was injected. The solution of osmic acid is of 1 per cent strength. The process has been successfully employed for the investigation of medullated nerve, striped muscle, epithelium, cornea, choroid, and retina. Full details will be found in the "Archiv fur Mikroskopishe Anatomie." vol. xvi. (1879), p. 471; a translation in the "Journal of the Royal Microscopical Society" for August. The Royal Microscopical Society are doing good service by the carefully-selected translations of foreign papers on biological subjects.

Chemistry and Technology.

It would seem that restrictions upon the Chemical Arts, to whatever length they may be carried, must fail to prevent the

injuries to which farmers and landowners complain, for in a paper published in "Die Chemische Industrie" on the "Injury to Vegetation by Acid Gases," Herr R. Hasenclever admits that many causes, in addition to the fumes of chemical works, have had a deleterious action upon trees. He points out that the use of coal as fuel, whether in private houses or in mechanical manufactures, &c., has exerted a very devastating action upon vegetation. The memoir is accompanied by a coloured engraving of the leaves of various trees as affected by acid vapours. German observations confirm the view entertained in this country, that the plane-tree is distinguished by its power of resisting acids.

An exhaustive research on the action of ozone upon the colouring matters of plants has been conducted by Prof. A. R. Leeds. The colouring matters of both leaves and flowers of the species experimented upon were partly or wholly destroyed by ozone; but Prof. Leeds found that a considerable percentage of ozone is required to produce this result, or if such small amounts as are obtained in the customary methods of ozonising air by phosphorus are employed (1 to 3 m.grms. per litre), a large volume of ozonised air must be used, and a considerable interval elapse, before bleaching is effected.

The *Palmella cruenta* is a small alga much resembling clotted blood. Its colouring matter, *palmellin*, Dr. T. L. Phipson finds to closely resemble hæmoglobin. It is, like the latter, insoluble in alcohol, ether, benzol, sulphide of carbon, &c., but it dissolves in water. It is, further, dichroic; it is composed of red matter united to an albuminoid substance, and is coagulated by alcohol, acetic acid, and by the application of heat. Like the colouring matter of blood, it produces absorption-bands in the yellow part of the spectrum, though not exactly in the same position. Palmellin easily enters into putrefaction, giving off a strong ammoniacal odour, and, to complete its analogies with the colouring matter of blood, it contains iron.

M. Lami has investigated the comparative influence of milking twice or thrice daily. He considers the latter system, other conditions being the same, more favourable to the production of butter.

A correspondent of the "Chemical News" calls attention to the fact that during the past two years no less than eleven claimants for recognition as elementary substances have presented themselves in that journal, their names being Davyum, Neptunium, Lavoesium, Mosandrum, "New Earths," Philippium, Ytterbium, Decipium, Scandium, Norwegium, Uralium. While several names in this formidable list are, doubtless, destined to stand the test of time, the majority, it is thought, will have to join the noble army of "defunct elements." Probably only the "fittest" will survive. And the question is, what delay shall occur before mentioning these substances in the instruction of

advanced classes? What authority is weighty enough to settle the existence or non-existence of these bodies?

At a meeting of the Academy of Sciences, on September 1st, M. P. T. Cleve announced the discovery of two new elements in erbia, which he has named respectively *Thullium* and *Holmium*.

Herr Fleitmann conceived the idea that the brittleness of nickel and cobalt was due to absorption of carbonic oxide. He succeeded in removing it by the addition of small quantities of magnesium, obtaining both metals in a perfectly malleable and ductile condition. To prevent explosions, the magnesium is introduced through a hole in the lid of the crucible after the oxygen has been removed by the addition of fragments of charcoal.

A new method for the detection of arsenic has been devised by Prof. Selmi. The method is that of Schneider modified so as to incur no losses. The substance to be examined is treated with hot concentrated sulphuric acid, and during the same time is traversed by a current of hydrochloric acid gas, which carries with it all the arsenic in a state of chloride, separating it from the organic substances with which it was mixed. The arsenical liquid is then placed in a Marsh's apparatus and tested in the usual manner. Prof. Selmi has been thus able to obtain the metallic ring on operating upon 100 gms. of animal matter containing 1-400th of a milligrm. of arsenious anhydride.

Reichenbach's wood-tar colour Pittical has been resuscitated by A. Gratzel, and it is now an article of commerce at the price of £4 per kilo., under the name of "German-Imperial-Flower-Blue," with reference probably to the blue corn-flower, which is said to be the favourite cognizance of the German Emperor. The acetate is generally used for dyeing, dissolved in a little acetic acid diluted with water, and almost neutralised with ammonia. In this bath silk and wool take a fine reddish blue without the aid of any mordant. Cotton and other vegetable fibres are prepared with a solution of tannin, followed by a solution of tartar emetic. The colours produced are perfectly fast. From Reimann's "Farber Zeitung" we learn that if Pittical is separated from its alkaline salts, and heated with alcoholic ammonia in a sealed tube to 176°, a base is obtained closely resembling rosanilin in its composition. It dissolves in acetic acid with a corn-flower blue colour. The new colour is the first member of a new series of tinctorial products, which may prove of great importance.

Dr. Meusel, of Breslau, prepares colouring matter by treating coal-dust with nitric acid, or with a mixture of alkaline nitrates and sulphuric acid, or with other oxidising agents. A part of the coal is thus rendered soluble with a deep brown colour in alkaline hydrates or carbonates. The black residue serves as a body colour. The brown alkaline solution can (says

the "Die Allg. Polyt. Zeitung") either be used at once, or metallic salts of a black or dark brown colour may be precipitated from it, or the precipitate produced by the addition of acids may be used as a pigment.

According to the "Moniteur des Produits Chimiques," ostrich feathers are bleached in a bath of 10 grms. barium peroxide to 1 litre water, heated to 30°. In this they remain for forty-eight hours, and are then washed, treated with weak hydrochloric acid, and dried.

According to M. Moyret, 40,000 kilos. of copperas, pyrolignite of iron, and other iron mordants are used daily in the Lyon district for weighting black silks. For whites and bright shades sugar is used, to which the author proposes an addition of a decoction of quassia. Silk dresses thus prepared would serve for fly-papers! Stannic chloride, mysteriously spoken of in the trade as X, is employed in addition to sugar. Barium sulphate has also been proposed, but it deprives the silk of its lustre.

The following formula for preparing a mordant from lees of wine is taken from a recent number of "Chemiker Zeitung":—Fresh green lees, with the addition of two-fifths sodium tartrate, are evaporated down to one-sixth the original volume. 15 grms. of Cologne glue and 10 grms. tannic acid are added. The mass is pressed, rubbed over with alcohol and tannic acid, dried in the air, and powdered. For use it is to be further mixed with one-fifth per cent sodium tartrate. It is recommended for dying full shades in wool and silk, and is said to render the aniline colours permanent. In dying woollen cloth a decoction of Saponaria root is added both to the mordant and the dye-bath.

In a communication on the action of vinegar upon alloys of lead and tin Herr R. Weber shows that vinegar attacks pure tin as well as alloys with lead, the quantity of metal dissolved increasing with the proportion of lead present. Alloys of tin and lead, to which 4 per cent antimony had been added, were also attacked, and lead entered into solution.

Nickelising without electricity may be effected, says the "Moniteur Industriel," by introducing into a 5 or 10 per cent solution of zinc chloride as much of a salt of nickel as is sufficient to give it the ordinary colour of a nickel-bath. The articles to be coated, previously well cleaned, are laid in this solution, and the process is complete in from half an hour to an hour. Cobalt can be deposited in the same manner.

T. Troost has reported to the Société d'Encouragement pour l'Industrie Nationale on M. Gaiffe's Galvanic Deposits of cobalt. The metal is deposited from a solution of the double sulphate of cobalt and ammonia, and is superior to nickel at once in hardness, tenacity, and in beauty of colour. It is much less oxidisable than iron, but is very easily dissolved by acids.

From a report by M. Aimé Girard on M. Kuhlmann's (junior)

methods of conveying acids, we learn that M. Kuhlmann, in place of carboys, employs floating reservoirs in the form of an ordinary boat, fitted with air-chambers to give them sufficient buoyancy. For sulphuric acid of 60° B. and upwards these are constructed of sheet-iron, and have been in successful use for some years on the canals of the North. For hydrochloric acid he uses cylinders of hardened india-rubber, kept in their form by an external framework of wood. A modification of the structure serves for transport by rail.

The value of aluminium sulphate as a disinfecting agent is ascribed by A. Tedesco in "Die Chemische Industrie" to the following action:—The ammoniacal products of decomposition are fixed as ammonium sulphate; the liberated aluminium hydrate carries down all suspended particles, forming with them a solid precipitate. The organic cell, in contact with aluminous compounds, absorbs alumina with great avidity, losing thereby its vegetative power, and putting an end to the process of decomposition. Herr Tedesco considers bauxite and wochenite the best materials for the preparation of a sanitary sulphate of alumina. Kaolins are readily attacked by sulphuric acid, but are poor in alumina and comparatively costly.

The influence of the chemical composition of water in the preparation of raw silk is pointed out in a communication from the Instituto Technico Superiore of Milan. In silk are distinguished the soluble constituents, "varnish," or "gum" and colouring matters, and, on the other hand, the insoluble fibre. The soluble constituents give raw silk its brightness, colour, and strength, and should therefore be preserved as far as possible. For the purpose of unwinding the cocoons the natural gum should be softened, but not dissolved. According to Franceson, silk, if deprived of all its soluble constituents, loses at the same time its strength and elasticity. The authors of this paper find that though the loss of strength is proportional to the loss of soluble matter, the elasticity is but slightly diminished. In order to minimise the loss sustained in softening the cocoons hard waters are used, and soft waters are artificially modified by the addition of sulphate of lime and carbonate of soda. Silks which are to be dyed bright colours, however, should be spun out of soft water.

An improved device for testing petroleum oils, brought out by Mr. Holly, is described by "The Engineering and Mining Journal." With the common oil-tester it is extremely difficult, even for one skilled by long practice, to get reliable results except by taking the mean of a number of tests, since the approach of a jet of flame, however small, to the surface of the oil, as usually practised, destroys the accuracy of the test more or less, either by the local heating of the oil or by the production of disturbing air-currents. Mr. Holly modifies the apparatus usually employed by arranging the poles of a battery within

three-eighths of an inch from the surface of the oil, so that he is able to pass a spark between them, as the thermometer registers an increase of one degree in temperature. By this means the disturbing influences above noted are reduced to a minimum, and much greater accuracy in the results of such examinations is assured.

The Patents-Committee of the German Association for Promoting the Interests of Chemical Industry recently held a session at which an interesting communication from the scientific staff of the Baden Aniline Works was read. The authors point out that neither a novel substance nor a novel application of a substance obtained chemically can be patented in Germany. Concerning the preliminary investigation they doubt the possibility of deciding *à priori* on an invention.

Geology, Palæontology, &c.

The Memoirs of the Geological Survey, England and Wales, includes one on "The Geology of the N.W. part of Essex and the N.E. part of Herts, with parts of Cambridgeshire and Suffolk, by W. Whitaker, W. H. Penning, W. H. Dalton, and F. J. Bennett. The authors describe the cretaceous strata from the Gault to the Upper Chalk, the Eocene beds from the Thanet Sands to the London Clay, the Red Crag and the Glacial deposits of boulder-clay and gravel and sands. We notice that in a bed of so-called peat found in a cutting between Audley End and Saffron, in the valley of the Cam, two cart loads of large mammalian bones were taken from an area of 60 feet by 20. Some of them are the lower jaws of an ox, probably *Bos longifrons*. They bear artificially-made markings, and along with them was found a very fine horn of the Irisk elk, *Cervus Megaceros*.

The "Records of the Geological Survey of India" contain a translation of a paper on the geographical distribution of fossil organisms in India, read before the Imperial Academy of Sciences in Vienna by Dr. Waagen, formerly of the Geological Survey of India. The author considers that at the close of the Eocene epoch the sea retreated altogether from India; only in the neighbourhood of Karrachi and Arracan does it appear to have touched the Indian continent, so that the sea of the Miocene and Pliocene periods probably extended away to the southward, whilst a north-westerly connection of the continents probably took place with North Africa through Arabia. Blanford's supposition of an Indo-Oceanic continent uniting Africa, India, and Australia, and existing without important changes from the end of the palæozoic up to the miocene and pliocene periods, is thoroughly erroneous. A submerged forest has been discovered on Bombay Island, remarkable as showing that in recent or sub-recent times a depression must have taken place in the immediate neighbourhood of ground which appears to have been raised.

Another number of these valuable "Records" contains an interesting discussion on the origin and affinities of a fossil jaw, found in the Sivalik range, which is referred to a new species, *Palæopithecus sivalensis*. It was intermediate in size between the mias and the gorilla, but in dentition falls between the chimpanzee and man. The author considers that its occurrence is another piece of evidence in favour of the hypothetical sunken continent Lemuria, from which the anthropoids have been dispersed. He suggests that the unexplored tertiaries of Africa, Sumatra, Borneo, and Southern India may probably contain forms bridging the chasm between the highest-known apes and man. In the same district proof has been obtained of the former existence in India of an emeu double the size of the existing emeu of Australia. *Megaloscelornis sivalensis* was a wader probably allied to the adjutant crane, but in stoutness and length of limb approaching the ostrich. Colonel C. A. MacMahon contributes some interesting notes on the supposed glaciation of India. He finds the contour of the hills and valleys in the interior Himalayas sharp and angular, rounded outlines, where seen, being sufficiently explained by the action of sub-aërial forces on comparatively soft and friable rocks. The idea of an ice-cap is thus set aside, and there seems even no evidence that the existing glaciers had ever, within a reasonable geological period, extended lower than 11,000 feet above the sea-level. Mr. Lydekker, in his investigations in the valley of Kashmir, finds no evidence in favour of a former glaciation of the outer hills and the Upper Punjab.

From a memoir on the "Geology of the Salt-Range in the Punjab," by Mr. A. B. Wynne, we learn that the rocks of the salt-range comprise alternations of calcareous, earthy, and sandy deposits, which may be arranged under thirteen main divisions. Nine of these can be referred to as many of the main formations recognised by geologists, whilst the ages of the other four are not satisfactorily determined. The economic resources of the district have been exaggerated. The deposits of salt, indeed, are enormous, and the average annual receipts from their working reach the sum of £388,144. Still we must remember that the world's available supplies of this mineral are incalculably in excess of any conceivable demand for ages to come. In the so-called Mayo mine a pink-coloured salt has been found containing 61 per cent. of potassium chloride, but unfortunately it formed merely a local lenticular deposit, and Mr. Wynne has not been able to detect it in other parts of the range. The coal found in the region is pyritic, shaly, and not very abundant. Sulphur occurs in exceedingly small quantities. Petroleum wells are not copious, a gallon daily being considered a good flow. Sandstones, saturated with oil, are abundant, and might, doubtless, be profitably distilled. The stream-gold yields a scanty return, 4 annas' worth being the average per man for a day's washing.

Copper occurs as a mineralogical curiosity only. Galena is found in small disseminated crystals in the dolomite of Karangli. Iron ores of a first-rate kind are mentioned by Mr. Powell and Dr. Henderson as having been obtained from the Korana hills, but the cost of production would not be remunerative owing to the scarcity of fuel.

The Daltonganj and Aurunga coal fields may, according to the estimate of Mr. V. Ball, F.G.S., yield 30 million tons of coal of a good quality. In this estimate the Singra seams are not included, though it is remarked that they might be profitably worked for some years as open quarries. Concerning the Hutar fields, the author considers that any estimate would be premature, as the lateral extension of the seams is quite unknown. Concerning the Tatapani coal-field nothing definite appears to be known. Iron ores of different kinds, magnetite, carbonate, and carbonate altered into hæmatite appear to be found in considerable quantity, but under circumstances which make their utilisation a somewhat problematical question. The limestone of the region is, however, well adapted for use as a flux. Galena and copper ores occur, but, it would seem, not in quantity.

A recent issue of the "Geological Survey of Canada" contains a paper "On the Fossils of the Cretaceous Rocks of Vancouver and adjacent Islands in the Strait of Georgia," by J. F. Whiteaves, F.G.S. The palæontological researches of the author support the conclusion of Mr. Gabb, based upon an investigation of the cretaceous fauna of California, that a continuous land-barrier between the oceans of that period can scarcely be admitted. The respective faunæ of the Upper Cretaceous of Vancouver and of Texas are even more closely related than those of the same period in California and Texas.

The American Association for the Advancement of Science held its twenty-eighth meeting at Saratoga Springs, N.Y., beginning August 27th, 1879. The meeting was a memorable one, both on account of the large attendance and the great value of the papers presented. The Association was honoured by the presence of an unusually large number of its ex-Presidents, no less than nine being on the platform at one time. The presiding officer, Prof. George F. Barker, M.D., of the University of Pennsylvania, was very happy in conducting the business of the Society, his genial humour in no wise detracting from the dignity of the occasion, and serving as a pleasant refreshment. The other officers of the Association were as follows:—Vice-President of the Physical Section, Prof. S. P. Langley, of Alleghany, Pa.; Vice-President of the Natural History Section, Major J. W. Powell, of Washington, D.C.; Permanent Secretary, Prof. F. W. Putnam, of Cambridge, Mass. In the absence of Dr. George Little, of Atlanta, Ga., Dr. H. Carrington Bolton, of Hartford, who was General Secretary at the St. Louis Meeting (1878),

was continued in office. The Chairman of the Sub-section of Chemistry, Dr. Ira Remsen, of Baltimore, was, unfortunately, prevented from attending, and Prof. F. W. Clarke, of Cincinnati, took the position. One of the features of these meetings is the address of the Retiring President. On this occasion Prof. O. C. Marsh, of New Haven, had the duty to perform. His subject was "The History and Methods of Palæontological Discovery." Vice-President Langley gave an address on "Solar Physics," a subject to which he himself has materially contributed. And Major J. W. Powell gave an address on "Mythologic Philosophy, having special reference to the mythologies of the Indian tribes. Dr. Ira Remsen's address was a plea for the study of Organic Chemistry, a branch which he claims is too often neglected in the courses prescribed in colleges and scientific schools. These addresses will appear in full in the annual volume of Proceedings. Heartily welcomed by the meeting was the distinguished astronomer, Dr. Otto Struve, Director of the Pulkowa Observatory, Russia, who is in America for the purpose of securing a larger object lens for a refracting telescope than has yet been made in the world. The lens is to be made by Alvan Clark and Son, of Cambridge, Mass. The lion of the gathering was, undoubtedly, Dr. Thomas A. Edison, of Menlo Park, N.J. He exhibited on Saturday evening, to a delighted audience of 1500 persons, his recently-invented Electro-Chemical Telephone. Speech, music, &c., was transmitted from a distant room, and the sounds issuing from the telephone were heard by every individual in the hall. Perhaps the most remarkable discovery announced is one by Dr. Edison, which will necessitate an entire revision of the physical constants of all known metals. His paper was entitled, "The Phenomena of Heating Metals in Vacuo by means of an Electric Current," and demonstrated that platinum heated *in vacuo* by electricity becomes denser, harder, more infusible, and less liable to disintegration when heated in a flame. Iron treated in a similar manner becomes as hard as steel and just as elastic. Aluminium melts only at a white heat.

The Meeting of the French Association for the Advancement of Science for 1879 was held, in August, at Montpellier, under the presidency of M. Bardoux, the late Minister for Public Instruction. The speaker chose for his subject "Education," viewed chiefly from a literary and a moral point of view. Still he declared that "henceforth the memory must no longer be made the basis of education." He denounced the too great importance attached to words, and quoted Montaigne to the effect that the pupils of many pedagogues were simply "asses loaded with books." The financial statement showed that the capital of the Society amounts to about 300,000 fr. The grants for research last year amounted to 10,000 fr. One of the lectures was given by M. Denayrouze, on "The Progress of Electricity."

M. Barral, Perpetual Secretary of the National Society of Agricure, also delivered a lecture on "The Use of the Rhone Water for Irrigation." The meeting for 1880 will take place in Rheims, in the month of August, while it has been decided to hold the meeting of 1881 at Algiers, during the month of April. For this meeting M. Chauveaux, Director of the Veterinary School of Lyons, has been elected the President. Among the papers read before the Chemical and Physical Sections were: "On the Compound Ammonias," by M. Buisine; "On the Oxidising Action of Cupric Oxide," by M. Cazeneuve; "On the Determination of Methylic Alcohol in Ethylic Alcohol," by M. Caillol; "On the Calcareous Soils of Barcelona, from a Chemical Point of View," by M. Luis Cabello e Ibanez; "On the Emissive and Absorbent Powers of Flames, by M. Rosetti; "On a Compass for the Measurement of Powerful Currents," by M. Ducrétet; "On the Method of Measuring the Intensities of the Currents of Bunsen Batteries by means of Thomson's Galvanometer," by M. Mercadier; and "On Thermometric Measurements of High Temperatures," by M. Crova.

THE MONTHLY

JOURNAL OF SCIENCE.

NOVEMBER, 1879.

I. LEGENDS OF SEPULCHRAL AND PERPETUAL LAMPS.

By Prof. H. Carrington Bolton.

EVERY science and every art appears to have had, at some period in its history, aims and aspirations of an unphilosophical and superstitious character; every branch of learning has had its infatuated enthusiasts working at unachievable projects and striving to reach unattainable goals. In astronomy, clever and learned men endeavoured to foretell the fate of mankind by a study of the stars; in mechanics they sought for perpetual motion: in mathematics, for the quadrature of the circle; in medical art for a universal panacea and an elixir of life; and chemical science was for many centuries a fertile field for speculations no less chimerical and unsubstantial.

The extraordinary delusions of the alchemists have been summarised by Wiegleb, a German chemist of the last century, in the following words:—"At one time they turned their attention to the production of pearls and precious stones; at another to the fixation of ordinary mercury, and hence to its extraction from natural metals; some sought to transform water into vinegar, others eneavoured to make glass flexible and malleable. They sought also to prepare for the physician the much-needed elixir of life, to transform common salt into saltpetre, to prepare a universal solvent, *to discover the means of causing lamps to burn perpetually*, to reproduce plants and animals from their ashes, and even to effect the resurrection of the dead! Besides these chimerical pursuits, the favourite and most enticing of all was the transmutation of the common or base metals into silver and gold." *

* Wiegleb, Hist. Krit. Untersuch. d. Alchemie. Weimar, 1777. 12mo.

The belief in ever-burning lamps mentioned by Wiegleb is not unfrequently alluded to in prose and poetry, yet detailed and exact information on this point is to be had only by patient research. Many volumes have been written on the history of chemistry, and on alchemy, narrating the fanciful views and arduous labours of the would-be transmuters of the base metals, but they seldom make reference to the sepulchral and perpetual lamps.

During the sixteenth and seventeenth centuries a belief in the actual existence of ever-burning lamps seems to have been very real and widely prevailing. Many learned writers maintained that the ancients were acquainted with the preparation of a combustible fluid, which, while burning and giving out light, diminished not in quantity and potency. Lamps supplied with this marvellous liquid were placed in tombs by the ancient Romans, and continued burning until some ruthless explorer desecrated the subterranean places of burial and allowed air to enter, whereupon the flame flickered and shortly expired. These astonishing lamps "burned the most brightly where there was most want of air, and were always extinguished by the immission of external air."—(Dr. Plot.)

To discover the secret of preparing this combustible yet inconsumable liquid was one of the alchemist's dreams; in their figurative language they endeavoured to "concentrate the four elements, and to make therefrom a wonder-working essence."—(Kriegsmann.) The material upon which their operations were conducted, and in which they placed great reliance, was fine gold; this, they said, resolved by hermetic methods into a "clear liquid," supplied, by its "incombustible oiliness," the perpetual light. Some authors describe the liquid in question as an "extract of the elements prepared with great labour, and possessing the property, when provided with a suitable ferment of silver or gold, of forming the white or red tincture capable of transmuting the base metals." But without the ferment it nourished the ever-burning lamps, and on account of its exceeding costliness was consecrated to the mythological divinity Pluto." Such is the description of this liquid contained in a rare and quaint little 16mo., entitled "Taaut," written by one Wilhelm Christoph Kriegsmann, and published at Frankfurt in 1665 (page 65 of Notes).

The alchemists did not, however, always employ such enigmatical and obscure language when writing of this mysterious liquid. Trithemius—a learned abbot of the fifteenth century, who found leisure amid his Benedictine duties to

write works on theology, history, philosophy, and alchemy—gives two receipts for making the inexhaustible oil: one of these reads as follows:—

"Mix four ounces of sulphur and four ounces of alum, sublime them, and convert them into flowers. Take two and one-half ounces of these flowers, with half an ounce of borax and Venetian crystal, and pulverise the whole in a glass mortar. Put the powder into a phial, and, having poured into it spirit of wine four times rectified, cause it to digest. Pour off the spirit of wine, and, having added some new, repeat the operation three or four times, until the sulphur runs on hot plates of brass without smoke, like wax. You must then prepare a proper wick, which may be done in the following manner: take filaments of mineral flax of the length of the finger, form them into a packet half as thick as the finger, and tie them with a white silk thread. The wick being thus arranged, cover it with the sulphur prepared as before described, and immerse it in the sulphur in a vessel of Venetian glass. Place the whole upon a sand-bath for twenty-four hours, so that you may always see the sulphur boil, and the wick being by these means well penetrated and impregnated with that aliment, put it into a small glass with a wide mouth. Then fill this vessel with the prepared sulphur and place it in warm sand, that the sulphur may melt and surround the wick. If it be then kindled it will burn with a perpetual flame."*

It is hardly necessary to add that the process, described with so much care, would prove very unsatisfactory to any curious investigator.

The alchemists also claimed that an inconsumable oil could be obtained by treating gold with oil of vitriol, and they called the product *oleum vitrioli aurificatum*. Other receipts are given by the Jesuit Athanasius Kircher, and will be found by the curious in his "Mundus Subterraneus." †

The belief in perpetual lamps was sustained and diffused by the publication of many legends, which appear to have been freely accepted by a credulous people, notwithstanding their highly improbable character and the slender basis of truth on which they were founded. One of the most famous and oft-quoted legends establishing this superstitious belief was that of the tomb of Tulliola. In the pontificate of Paul III. (1534—1549), the same who convoked the Council of Trent, a tomb was opened, at Rome, which contained the

* Trithemius, Tractatus de Lapide Philosophorum. 1611, 8vo.
† Vol. ii., Lib. 8, Sect. 3, De Asbesto, &c.

body of a young woman in a remarkable state of preservation. Her flesh was uncorrupted,* and her tresses were still bound with a small plate of gold curiously chased and enamelled. A burning lamp hanging in the vault was extinguished as soon as the air entered. On the walls was carved the inscription "*Tulliola filiæ meæ*," which was thought to refer to Tulliola, the daughter of Cicero, whose death he laments in his letters to Servius Sulpicius.† One authority states that the lamp burned several hours after opening the tomb, though it must have been closed more than fifteen hundred years. This legend was the subject of much comment. Montfaucon,‡ the eminent French antiquary and philologist, repudiates the story, and maintains that it is merely the revival of an older tale concerning the discovery of an uncorrupted body, in March, 1485, related by Etienne de Infestura and Alexander ab Alexandro. The latter describes the marvellously perfect preservation of the remains, and adds—"To see the body one would have have thought it interred but a short time; there was no inscription, though Pomponius thought it to be the body of Tulliola, daughter of Cicero, or that of Priscilla, wife of Abascantius."

Another legend, which supplied partisans with one of their strongest arguments, was that of the Lamp of Olybius. In the year 1500 some peasants, digging the earth to a considerable depth, near Padua, discovered a tomb containing lighted lamps, one of silver and the other of gold; or, according to another authority, there was but one lamp, placed in an urn between two phials, one filled with liquid gold and the other with liquid silver, by virtue of which the light had been maintained since the days of the Romans. According to inscriptions§ on the urns the lamp had been prepared with great labour by Maximus Olybius. Representations of the two vases, with their inscriptions, are given in Kriegsmann's "Taaut," and in several less rare works.‖ Licetus,

* Perhaps converted into adipocere (?).

† Ad Familiares, iv., 6.

‡ L'Antiquité Expliquée. Paris, 1719.

§ *On the smaller urn*:—Abite hinc pessimi fures! Vos quid voltis vestris cum occulis emissitiis? Abite hinc vestro cum Mercurio petasato caduceatoque. Maximus maximo donum Plutoni, hoc sacrum facit.

On the larger urn;—

"Plutoni sacrum munus ne attingite fures!
Ignotum est vobis hoc quod in urna latet;
Namque elementa gravi clausit digesta labore
Vase sub hoc Modico Maximus Olybius.
Adsit fœcundo custos sibi copia cornu,
Ne pretium tanti depereat laticis.

‖ *See* Petri Afriani, Incriptionibus, p. 337; also Licetus, Ferrari, and Montfaucon.

of whom more hereafter, gives a letter of Maturantius, who tells his friend Alphenus that "Both the urns and the lamps and the phials have fallen into my hands, and are now in my possession. If you saw them you would be astonished. I would not part with them for one thousand crowns of gold." Two similar sepulchral lamps are to be seen in the Museum of Rarities at Leyden.

Many other equally credible narratives were current in the sixteenth and seventeenth centuries. At the time of the suppression of the monasteries by King Henry VIII. (1537) a tomb was opened containing a lamp which had been burning at least twelve hundred years: the authority of Cassidiorus was quoted, who says he himself made perpetual lamps for the use of the monks in his monastery at Viviers (about 500 A.D.). Reference was also made to the lamp of Pallas, son of Evander, whose brave deeds were sung by Virgil: this lamp was found at Rome about 800 A.D., and must have burned nearly 2000 years. Even St. Augustine, the most illustrious and holy Father of the Church, was claimed, by partisans of these traditions, as testifying to their truth. St. Augustine, mentioning the lamp in the Temple of Venus, says it "burned perpetually, and the flame adhered so strongly to the combustible matter that neither wind, rain, nor tempests could extinguish it, though continually exposed to the inclemency of the seasons." St. Augustine attempts to explain the marvel, and conjectures that the incombustible wick was made of asbestos, but that the inexhaustible aliment was the work of demons, who wrought the infernal miracle in order to blind the pagans more completely, and to attach them to the infamous deity worshipped in that temple.

The famous chemist Glauber refers to these miraculous lamps in his brief essay entitled "Book of Fires" (*Libellus Ignium*). He writes of a lamp "which, being closed up in a glass, may be made to burn continually by its own virtue, and give light without any other help." He explains the admitted fact that when the "secret fire" is "touched by the least air it extinguisheth and goeth out," by the suggestion that "the fire is appropriated to the Elemental Air," and hath its "own air in itself." Glauber, who was always ready to turn chemical discoveries to medical purposes, writes of this lamp that it may be "very available to those who, by reason of continual weakness, are forced to lie much upon their beds," and that it may be "kept continually burning in their bed-chamber, not only because of its clear shining light which doth neither smoak nor scent, like all

other combustible lights, which scent is very prejudicial to the sick as well as to the healthy." *

Even in the eighteenth century we find anecdotes of these ever-burning lamps, clothed with new adornments and skilfully adapted to the later epoch. One of the most entertaining of these legends is that of Rosencreutz, the founder of the mysterious fraternity known as the Rosicrucians. The narrative runs as follows:—A certain person having occasion to dig somewhat deep in the ground, where the philosopher Rosencreutz lay interred, met with a small door having a wall on each side of it. His curiosity, and the hope of finding some hidden treasure, soon prompted him to force open the door. He was immediately surprised by a sudden blaze of light, and discovered a very fair vault. At the upper end of it was a statue of a man in armour, sitting by a table and leaning on his left arm. He held a truncheon in his right hand, and had a lamp burning before him. The man had no sooner set one foot within the vault than the statue, erecting itself from its leaning posture, stood bolt upright, and, upon the fellow advancing another step, lifted up the truncheon in his right hand. The man still ventured a third step, when the statue with a furious blow broke the lamp into a thousand pieces, and left his guest in sudden darkness. Upon the report of this adventure the country people came with lights to the sepulchre, and discovered that the statue, which was made of brass, was nothing more than a piece of clockwork; that the floor of the vault was all loose and underlaid with several springs, which upon any man's treading naturally produced that which had happened. Rosicrucius, said his disciples, made use of this method to show to the world that he had re-invented the ever-burning lamps of the ancients, though he was resolved no one should reap any advantage from the discovery."†

The Comte de Résie, in his curious "Histoire et Traité des Sciences Occultes,"‡ gives a brief account of the sepulchral lamps, and adds the following narrative:—In the reign of St. Louis (1226—1270) there lived in Paris a certain Rabbi named Jechiel, who was regarded by the Jews as a saint and by Christians as a sorcerer. He possessed a lamp which gave out light equal to daylight in brilliancy, which required no oil, and burned unceasingly. But what is more singular still, when honest tradesmen or people of quality came at night to knock at the door, his lamp shone brightly

* GLAUBER'S Works. London, 1689. Part II., p. 216.
† Spectator, No. 379, May 15, 1712, written by EUSTACE BUDGELL.
‡ Vol. ii., p. 67, 1857.

as usual, and the Rabbi gave his friendly guests an entrance; but whenever impostors or persons of evil intent presented themselves at the door, the lamp grew visibly pale, and the Jew took care to fasten the door against the intruder.

The latter half of the seventeenth century witnessed a considerable controversy on the subject of perpetual lamps. On the affirmative side of the question we fine the names of the Abbot Trithemius, already mentioned; John Baptist Porta, the eminent Italian physicist, the same who invented the camera obscura ("Magiæ Naturalis," Libri xx., 1558); the celebrated Italian engraver and painter, Pietro Santi Bartoli ("Le antiche Lucerne, &c.," Roma, 1691, fol.); the German antiquary, Lorenz Beger ("Lucerne, vet. sep. Berol.," 1702); the learned Jesuit, Martin Antoine Delrio ("Essay on Magic," 1599), who attributed the lamps to magic skill; and Fortunius Licetus, the most credulous and erudite of all. On the negative side of the question are arrayed the names of Vigneul-Marville, a French Carthusian monk (1699); the learned Jesuit, Athanasius Kircher, whose "Mundus Subterraneus" has been already quoted; the Italian antiquary and Professor of Philosophy, Ottavio Ferrari ("Dissertatio de Veterum Lucernis," Padua, 1684, 4to., in Grævius's "Thesaurus Antiq. Ital."); the Italian ecclesiastic, Paolo Aresi, Bishop of Tortona ("Sacred Emblems," 1613); the English naturalist and antiquary, Robert Plot ("Phil. Trans.," Abr. III., p. 100, 1684; the German chemist, Libavius ("Comment. Alchem.," Part II., Lib. ii., Cap. x., 1595); Buonamici, an Italian *litterateur;* Fabricius; Montucla; and many others.

To detail all the points for and against the existence of perpetual lamps made by these writers does not come within the scope of this essay. One of the most earnest supporters of these fables, Fortunius Licetus, deserves a somewhat lengthier notice.

Fortunius Licetus was born in the State of Genoa, October 3rd, 1577. He was the son of a physician, and, having received a liberal education in medicine and philosophy, became professor of these departments of learning at the University of Padua. After holding these chairs for twenty-five years the rival University of Pisa secured his services by a tempting offer; but nine years later, a vacancy occurring at Padua, he returned to his former position, and held it until his death in 1657. Licetus was a voluminous author, writing fifty treatises on medical, moral, philosophical, antiquarian, and historical subjects, which were distinguished for their erudition, though he displayed little

acuteness in research or originality of conception. His treatise "De Monstrorum Causis" (1668) abounds with instances of his credulity and with the superstitions and fables of his predecessors. Of all his works none is more celebrated than his treatise on the Sepulchral and Perpetual Lamps of the Ancients ("De Lucernis Antiquorum reconditis," Venet., 1652). This work, of 640 pages in quarto, is written expressly to prove the truth of the traditions concerning sepulchral lamps. The author accumulates a great number of witnesses, ancient and modern, quoting legends of Merlin, Porta, Scardeonius, and many others. The work is abundantly illustrated with engravings of the perpetual lamps, and forms a notable example of misplaced erudition and credulity. In his first book Licetus mentions thirty instances of perpetual lamps, including those connected with the mysteries of the Delphic Oracle, the ceremonies of Jupiter Ammon, and of the Vestal Virgins. The lamp of Demosthenes, which burned in the Temple of Minerva at Athens, also furnished him with a proof of the possibility of an inextinguishable fire. Licetus also relates the testimony of Jacobonus, author of the "Book of the House of Cesi," who mentions several persons who had seen these lamps still burning. He confidently relies on the testimony of such an unreliable author as Pausanias (175 A.D.), who speaks of a golden lamp, in the Temple of Minerva, which, when once filled with oil, burned a whole year without replenishing—a marvel which he attributed to the nature of the wick. Licetus refers to the statement of Plutarch, who relates that Cleombrotus, the Lacedemonian, visited the Temple of Jupiter Ammon, and saw a lamp which the priests said burned continually without oil.

Vigneul-Marville, writing of Licetus's work, very justly remarks that in it "Licetus exhausts all the resources at his command, but after all does not inform us of what we most want to know, viz., the secret of these perpetual lamps.

Many endeavours have been made by modern authors to account for the persistence of this belief by reference to natural causes. Montfaucon remarks no one doubts that burning lamps were placed in tombs by the ancients, and gives the following inscription from a tomb at Salerno:—"Adieu Septima; may the earth lie lightly upon you; may a golden oil cover the ashes of him who placed in this tomb a burning lamp." *

* "Have Septima, sit tibi terra levis quisque huic tumulo posuit ardente lucernam illius cineres aurea terra tegat."

Certain writers suggest that the imperishable wicks may have been made of asbestos, or "salamander's wool" as it was called, but they encounter great difficulty in the matter of the "indestructible aliment." Vigneul-Marville ascribes the flames to the "fat and gross vapours engendered by the corruption of dead bodies and enkindled by the torches used in opening a tomb." Dr. Robert Plot, "Director of Experiments to the Philosophical Society of Oxford," read a paper before the Society, in 1684, in which he narrates experiments made to test the value of asbestos for lamp-wicks. He concludes that this material may have been used in the sepulchral lamps, and to account for the inexhaustible oil discourses on a spring of liquid bitumen, or naphtha, such as occurs in Shropshire : a similar suggestion was made by Athanasius Kircher as early as 1665 ("Mundus Subt.," *loc. cit.*). Other writers conjecture that the lights found in the tombs were of phosphorus—meaning thereby not the modern chemical element of this name, but the so-called "Bologna stone," which shines in the dark. The elder Disraeli briefly notices these remarkable legends in his "Curiosities of Literature," and points out the necessity of the oxygen of the atmosphere to ordinary combustion ; he considers this fact in itself a sufficient refutation of these singular traditions.

II. NATIONAL SCIENTIFIC APPOINTMENTS.

MANY well-informed people, including not a few of those who hover round the outskirts of the scientific world, have but very vague notions concerning the manner in which many of our scientific officials are selected. We have heard the question asked whether appointments of this nature were obtained by influence, by examination, or, *rebus gestis*, by discoveries made and researches carried out. In consideration of our national cram-worship, it need scarcely have been doubted that we should, with rare exceptions, hold fast to the worst of all possible methods, that of competitive examination—a system which is year by year lowering our national position in the scientific world.

Facts have, indeed, lately come to light which might render all further argument needless if "cram," like many other absurdities, had not ninetyfold the "nine lives" which the popular proverb assigns to cats. The "Chemical News," in its "Students' Number" for the session 1879-80, informs us, on the authority of Prof. Huxley, that "not so very long ago a certain student, who had never handled a dissecting-knife, carried off the B.A. prize as well as honours in Animal Physiology by dint of an excellent memory, and is now a Government clerk, whilst a contemporary of his who in the same year carried off prizes and honours in Chemistry, without having ever cleaned a test-tube in his life, is now one of our leading musical composers and critics." What man of ordinary common sense can fail to see the absurdities which in these two cases lie piled up, layer upon layer? Or what unprejudiced mind can require further evidence for the necessity of a great and a total change?

An official document has lately been issued, announcing that "open competitive examinations" will shortly be held for two scientific appointments, viz., that of Assistant-Keeper in the South Kensington Museum (Science Branch), and that of Assistant Naturalist in the Natural-History Department of the Dublin Museum of Science and Art. The process consists of two parts. There is a preliminary scrutiny in hand-writing, orthography, arithmetic, English composition, and translation from Latin, French, or German into English. Without satisfactory proficiency be shown in all these subjects the candidate is allowed to proceed no farther. If successful, he is then admitted to the competitive ordeal itself, consisting of an "obligatory" and an "optional" branch. The former comprises in any case elementary mathematics, in which 500 marks are obtainable, and "any two of the seven following subjects: inorganic chemistry, which may count for 500 marks; organic chemistry, for 250; physics, for 750; zoology, 500; botany, 500; geology and mineralogy, 500; physiology, 500; and drawing, 500."

The "optional" department embraces any of the above seven subjects which the candidate has not selected as obligatory, and in addition any of the following:—"Higher mathematics (pure), 1000; theoretical mechanics, 500; applied mechanics, 500; French, 500; German, 500; Latin, 400; Italian, 400; Greek, 400; and English History, 400."

Having expressed our gratification that the examinees are not required to be "up" in heraldry, international law, book-keeping by double entry, Mexican archæo-

logy, &c., we may meditate sadly not so much on the mere "stiffness" of the examination as on its irrelevant character. From the remark that "Candidates for the situation of Assistant-Naturalist must pass in Zoology," we may safely infer that the duties of the post will mainly consist in the application of a thorough knowledge of the animal kingdom, and consequently that the man wanted must be above all things a zoologist. Yet the highest number of marks possible in this science is a poor 500 out of a maximum total of 9100, or not quite 6 per cent! Hence there is absolutely no safeguard that the best zoologist will be appointed! Neither botany nor even physiology is, by some strange oversight, demanded as essential. Hence it is quite conceivable that a man ignorant of these two branches of science, and endowed with a mere smattering of zoology, may still—if well versed in mathematics, physics, and in languages—completely swamp his rivals and win the prize. On the other hand, a biologist, "pure simple," a thorough specialist who has concentrated his whole attention and devoted his whole time to the study of organic life, is almost of necessity excluded. Suppose such a one selects as his two obligatory subjects zoology and physiology, and as an optional subject botany, and that in all three—as well as in elementary mathematics, which is inevitable—he gains the maximum number possible, or 500 marks in each; his total number will then be only 2000. On the other hand, let a competitor take 500 marks in elementary mathematics, 750 in physics, 250 in zoology, 1000 in pure mathematics, 500 each in theoretical and in applied mechanics, and 400 each in Greek and Latin, he achieves a total of 4300, and wins in honours. The true biologist is beaten in virtue, we had almost said, of his very superiority in his own department, or at least in virtue of that very concentration of thought and attention without which true greatness in any science is out of the question.

Another peculiar feature of these regulations is that the "optional" list, if taken in the gross, might possibly lead to a greater number of marks than the subjects in the "obligatory" list, even if the whole of them were taken up. To us it seems self-contradictory to admit that a science or a branch of knowledge is non-essential by placing it in an optional list, and yet to reward proficiency in it more highly than in the branches recognised as obligatory.

We may further point out certain omissions: the history of science, and especially of organic science, would be

assuredly more to the purpose than English history. Animal geography, based as a matter of course upon physical geography, would also have been a useful feature. But these subjects seem to have escaped the notice of the Science and Art Department, or to have been crowded out by less relevant matter.

In what manner the examination will be conducted it does not, of course, appear. It is possible that the practical knowledge of candidates will be carefully weighed, and will stand them in more stead than any amount of mere words —that, *e.g.*, in zoology they will be called upon to name, or at least to refer to the correct order and family, specimens placed before them assigning grounds for such classification. But it is, on the other hand, possible that the whole affair will resolve itself into a question of verbal memory, and that the candidate who from his own mental emptiness most eagerly absorbs and most glibly reproduces the views of others will win the day.

These considerations naturally remind us that the "Regulations" hold out neither inducement nor reward for research. We might reasonably expect that in such a case original observations or experimental results might, according to their value, count for hundreds, or even thousands of marks, and if of pre-eminent merit might be held to decide the contest irrespective of all other points. But not so; investigations, however successful, and discoveries, however brilliant, count here absolutely for nothing, whether in the obligatory or in the optional department. Their very possibility is not taken into account, and their production would, we fear, be looked upon as little better than contempt of court. How many of the world's greatest naturalists would escape being ignominiously "plucked" if pitted, under such regulations, against candidates possessing an excellent verbal memory and accustomed to the "cramming" process? We have met with eminent men of science who admit that they would utterly fail if examined in their own published researches against men of this stamp.

The very root of the cramming system lies in the strange circumstance that the division of labour, so widely and so usefully recognised in the industrial arts, should still be deemed inapplicable in the sciences, and should even, in some influential quarters, be regarded as rank heresy. Ages ago the wise saw "Jack of all trades and master of none" was understood and acted upon in the practical world. But we still will not recognise a man to be master of any science unless he can be, or seem to be, "Jack" of all. Suppose

we require a good ship-carpenter; we do not deem it needful to test his abilities in Turkey-red dyeing, or in glass-blowing, or in shoe-making. We are content that he shall be a specialist; we believe that all those powers of body or of mind which are required in his art can be best trained and developed in the acquirement and exercise of that very art. We do not fancy that a blacksmith requires to exercise his muscles by preliminary practice as an oarsman; we do not dream that a railway official or a pilot, in order to appreciate coloured signal-flags or fires, ought to serve an initiatory training in a dye-house. We go still further: we are apt to suspect the man who lays claim to proficiency in several arts or trades. We know that, however able and industrious a man may be, life is too short for the attainment of excellence in many different directions. We prefer the man whose whole time and whose individual energies have been concentrated on the subject we want, and, provided he gives us full satisfaction therein, we care little as to his "general culture." Even professional men are apt to suffer in the confidence of the public if it is known that they study any subject not strictly within the sphere of their more immediate duties. We have heard of a physician who had given much time and attention to the study of geology, and to the formation of a geological museum in the town where he lived. No one was, indeed, prepared to say that he had ever neglected a patient, or that he was out of the way if called for; still the people argued, roughly, but not in the main inaccurately, that he would have been a more able and successful physician had he not been a geologist, and he suffered in their opinion in consequence.

In scientific education, and in the preparation for certain professional careers, we do the very reverse of all that has just been described. We despise and reject the specialist, and call out for the "good man all round"—the man whose time and attention have been equally distributed. We forget that in these days each science has become so immense in extent that to keep up with the progress of discovery in one only branch is a task of no trifling magnitude, and that the Polyhistor of the day is a mere superficial trifler. To act on our present principle is not only to waste time, but to run the risk of acquiring habits of thought foreign to our immediate purpose. If a student has to learn to distinguish nicely between peculiarities of form, of texture, of colour, odour, and the like, he will not be greatly aided by turning his attention to words and abstractions.

So long as we insist upon general culture as obligatory,

"cram" will flourish, denounce it as we may. The only way to its extinction, and consequently to thoroughness, lies through specialism.

III. HABITS OF ANIMALS IN RELATION TO THE WEATHER.

By the Rev. S. Barber, F.M.S.

THE habits of animals, and notably of the insect world, have for ages been admitted into the category of indicators of weather change. Much difficulty, however, has been found in interpreting these habits and movements, —a difficulty only to be overcome by long-continued, patient, and close observation of particular species. It is indeed somewhat remarkable that popular attention has been arrested, in this respect, by the habits of various species to which in a general way but little credit is given for intelligence, sagacity, or sensitiveness.

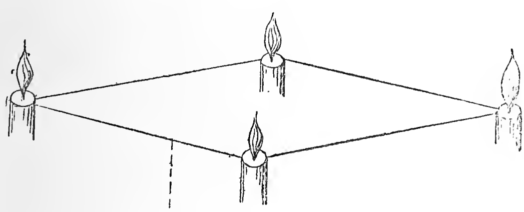
To associate the movements of ducks and geese (especially in a wild state) with weather changes in regard to rain and floods, which affect their habitations and supply of food, seems natural enough; but we find also such animals as the ram, the pig, and other thick-skinned creatures recognised as prophetical in weather lore and popular tradition. There is something taking in the notion of an astronomer gaining information of the approaching storm from the wagging of a ram's tail; and those whose occupation entails respect for, and attention to, the animal world, are often disposed to attribute to dogs or swine faculties denied to themselves, —as, for example, that the former are cognizant of spiritual phenomena, and that the latter can see the wind. The researches of modern zoologists* have certainly brought to light an amount of intelligence among the lower orders of animals that we little suspected in former times; and the attempt to elicit and develop, by careful training, such intelligence, has become to many an interesting pastime. We notice, also, that an exceptional regard to the habits of particular species has not only found an embodiment in the

* Recently the researches of Dr. M'Cook in regard to the ant deserve especial attention.

popular superstitions of various nations, but has even crept into their sacred writings. This result is perhaps partly due to the fabulists and moralists who have utilised familiar objects in order to convey moral and religious truths more pleasantly to the people. But the more rigidly scientific and exact methods of modern enquiry have tended to reveal more fully, to reflective minds, the *harmony* of Nature, the perfection of the various parts of her machinery, and their adaptation to one another; and also to show how the great Author of Nature has adjusted the springs with unerring hand, making precise compensation in the balance, and distributing with infinite wisdom both intelligence and force.

Certainly there is much more scope for enquiry among the lower creatures than many persons are aware of, as a careful study of the ant, the bee, or the spider will reveal, and has, in fact, revealed in recent times.

The following incident may serve as an illustration both of the mechanical dexterity and audacity, and also of the occasionally *social instinct*, of the last-mentioned creature; and possibly of the same law under which birds become tame upon the approach of severe weather:—In the autumn of 1878, upon a table on which the writer was dining, four candles were placed in an elongated diamond-form. They had not been lighted long when a tiny spider, scarcely larger than a pin-head, was observed to have hung his lines after this fashion (see figure):—



Spider's Web attached to Summits of Candles.

The summits of the candles were connected by a single line, rigidly strung, and uniting the very cups formed by the heat of the flame rising from their centres. It seems absurd to suppose that such a delicate thread could have been floated into this symmetrical form by atmospheric agency, in a room where the currents of air would necessarily be much disturbed; especially if we consider the fact of the flames being less than half an inch from each point of attachment to the candles. It would, again, be equally absurd to imagine that the tiny creature had ascended each candle sepa-

rately, drawing the line after it, as the table must have presented numerous obstacles to this method of construction. The probable method in such cases seems to be that the lines are projected with precision from one point of adherence to another; possibly they are weighted for the purpose. This would appear to be the only way of placing a delicate filament in so extraordinary a position; and that it is the actual method used, I had, soon after the above-mentioned incident took place, a strong proof.

A small spider appeared suddenly upon the sheet of paper on which I was writing. I advanced to the window, holding the sheet in my hand, to inspect the stranger. In the other hand I held another sheet of paper, the edge of which I placed within about a foot of that on which Sir Spinner was disporting himself. Having ascended to the edge, he at once darted over the intervening space with such rapidity as almost to elude vision. It seemed that he had projected a line, with great precision, to the opposite side of the gulf, the very moment he gained the "point of vantage," and then had sped across with lightning rapidity, drawing up the line as he advanced.

Speaking of *Araneæ*, I may mention in connexion with the severe winter, 1878-79, that, shortly before the frost began, I noticed several of the smaller species in a half-torpid state upon lines dropped from the old-fashioned high mantel-shelf in my room. They remained motionless for a considerable time in mid-air, apparently enjoying the warmth.

Here we have probably an instance of that tameness which finds another expression in the approach and entrance of small birds into human dwellings when intense cold is near at hand. Whether this change of habit arises from a vague anticipation of misfortune and a consequent craving for sympathy, or merely from the failure of the food-supply depending on the existence of creatures weaker than themselves, it seems evident enough that there is a consciousness of the fact that man is to a certain extent the controller of Nature's resources and the almoner of her bounties.

The Meteorological Society, in publishing its "Phenological" Observations, has fully recognised the importance of noting first and last appearances; and in the Entomological Report Mr. Griffith puts in a claim for the spiders. He alludes to their supposed remarkable perception of the *probable future condition* of the atmosphere; and notes the opinion expressed by M. De Tijonval, that their first appearance should be proclaimed with the "sound of trumpet."

A connexion between the movements of the animal world and the revolution of the seasons has been observed since the earliest dawn of Science, and to explain those movements may still furnish problems for the acutest of naturalists; but it would appear to be inferring too much to conclude, from their ordinary migrations, a possession of prophetic *insight* into the course of the weather. It seems more rational to believe, with regard to most of these appearances, that it is by virtue of their highly-developed senses and peculiar nervous organisation that some species become predictive *in their habits.* Whether the supposed presentiment which induces them to deviate from their normal line of action arises simply from a vague feeling of disquietude, or results from an actual perception and connotation of various phenomena which escape the human observer, is very difficult for us to decide.

To observe with exactness and to compare notes in a certain fashion are arts in which many insignificant creatures excel, and human beings are very apt to underrate both the power of observation and of "co-relating" in animals. The very conditions of their existence give them indeed an advantage as compared with the more artificial life of man, inasmuch as they are more *en rapport* with the physical forces of Nature, and more in harmony, therefore, with their onward movement.

It has been said that, in regard to weather prediction, the conditon of the barometer cannot, in any direct way, be regarded as prophetical, as it simply registers the *present* condition of the superincumbent air; yet by virtue of the association of phenomena, it must be allowed to be *practically* prophetical, inasmuch as it registers conditions which, in a general way, have a certain sequence.

And it appears that this simple law of the association of phenomena applies also, and with perhaps greater force, to the movements of the animal world: for it can scarcely be doubted that animals are in many cases highly sensitive recorders of atmospheric influences, and that they often register the action of those more subtle forces to which the movements of the barometer are less intimately related.

IV. THE TEMPERATURE OF THE SUN.

By W. Mattieu Williams.

IN the last number of the "Monthly Journal of Science" Prof. Langley compares the radiations from the sun with those from a surface of molten metal which presents to the thermopile a disk of equal angular diameter, and bases his estimate of an inconceivably high solar temperature upon this experiment.

If we had any reasons for concluding that the surface of the sun is a heated solid, or liquid as athermanous as molten iron, his method might justify his conclusions; but all we know of the sun leads to the conclusion that the photosphere is composed of flaming or incandescent gaseous matter, the outer or superficial strata of which are permeable by the radiations from the incandescent matter below or within. If so, it is evident that the effective radiations from such a body must be increased, not merely by the extent of its surface, but also by the thickness or depth of the diathermanous incandescent matter; and a comparison between a disk of this and another similar disk of an athermanous body that radiates only from its surface must be quite delusive when applied for comparing their *temperatures*. This may be illustrated by simply presenting a plate of heated iron to a distant thermometer or thermopile, and then carefully placing behind this another plate of equal or less diameter, and equally heated. If direct radiation from the second is prevented, the thermometer will not rise when it is placed behind the first. Now try a similar experiment with two gas flames, and the effect of the radiation of the second flame *through* the first will be seen at once, though the surface of flame presented and the thermometer remain the same, and the temperature of the first or front flame is not sensibly raised.

In Chapters 7 and 8 of "The Fuel of the Sun" I have described a series of experiments made for the purpose of determining the transparency and diathermancy of flame to its own special radiations. The experiments on transparency were conducted with the advantages of the best attainable apparatus, — the photometer and dark chamber of the Official Gas Examiner of the Sheffield Corporation,—and the results showed that with proper precautions, there described in detail, the light received upon the screen of the photometer from a given number of gas-jets, each burning

an equal quantity of gas per minute, was the same whether the jets were arranged with all their surfaces presented to the screen, or with the surface of only one presented, the others being ranged behind that one and at equal mean distance to the larger surface; or, in other words, that thickness or depth of flame is a factor of equal value to that of surface area. The flame itself was found to be transparent, but the products of its combustion not so. The diathermancy of flames was similarly tested, but with inferior apparatus. The results showed that every additional flame placed behind others increased the thermal radiations from a given restricted area facing the thermometer; but the increase was not so strictly proportionate to the quantity of gas consumed as in the case of the luminous radiations. The variations, however, were not greater than the limits of error due to the rude experimental appliances.

Further investigations are demanded for reliable quantitative determination of the amount of thermal radiations which will pass through a flame or flames of given thicknesses. My experiments *indicate* (though not sufficiently delicate to *prove*) that a flame offers no resistance at all to the passage of the thermal radiations from a flame similar to itself; and if I am right, the vast energy of the solar rays demand no preter-terrestrial *intensity* of temperature in the sun, but merely a great *quantity* of heat, due to a great depth of flame of no higher temperature than is produced here on earth by the re-combination on the photosphere of those elements which we are justified in regarding as dissociated in the interior or lower regions of the solar envelope, and re-combining in the photosphere.

As Prof. Langley is familiar with the phenomena of the Bessemer process, I will direct his attention to a magnificent illustration of the effect of body or thickness of flame which it affords. Every chemist knows how feeble is the luminosity of the blue flame which accompanies the combustion of carbonic oxide. When the spiegeleisen is poured into the Bessemer converter some of the carbon of the spiegel combines with the oxygen of the fused oxide of the blown iron, and a huge blue flame of unmistakable character emerges from the mouth of the converter. Its intrinsic luminosity is visibly small, comparable to that of the flame we produce in the lecture experiment of decomposing oxalic acid in a test-tube, and lighting the carbonic acid at the tube's mouth. The radiant energy of this body of pale flame (which has no hollow centre like common flames receiving their oxygen from the outside atmosphere) is nevertheless wonderfully

great, as may be seen by observing its illumination of distant objects. The Bessemer shop at Sir John Brown and Co.'s Works, Sheffield, had window-openings at the sides and roof; and standing on Grimesthorpe Hill, half a mile away, I have frequently, on misty nights, seen beams of light pouring through these openings, and stretching to the low clouds, on which the image of the opening was projected as on a screen. In all weathers the night illumination of these windows by the carbonic oxide flame was magnificent.

It appears to me that this subject is of great philosophical interest. If the sun has a temperature approaching that assigned to it by Prof. Langley and others, all hopes of studying solar physics by comparison with terrestrial phenomena are at an end. The revelations of the spectroscope must be but snares and delusions, since the activities of gaseous molecules or atoms incandescent at such temperatures should set their wave-lengths and refrangibilities at such variance with those in our laboratories that we must either fail to recognise them or remodel our accepted theories of radiation. If, on the other hand, they have temperatures similar to those produced by the combinations of terrestrial elements, we may venture to accept the conclusions of the spectroscopist concerning their identity, and reason upon possible effects of their dissociation and re-combination, our reasoning being then based on the only data we possess, viz., terrestrial experience.

V. LONGEVITY, OR THE NATURAL DURATION OF LIFE.

THE subject of Dr. Richardson's Opening Address at the Croydon Congress of the Sanitary Institute of Great Britain, recalls to our mind an admirable paper "On the Longevity of Brain-Workers," read at a Meeting of the American Public Health Association, in 1874, by Dr. George Beard, of New York.* Dr. Beard wished to prove the error of the accepted teachings in regard to the effect of mental labour. He therefore obtained statistics on the general

* Quarterly Journal of Science, vol. v. (N.S.), p. 430.

subject of the relation of occupation to health and longevity from the registration reports of England and America, and he also studied the lives of many prominent brain-workers. So far from confirming the generally-received theory that the mind can only be used at the expense of the body, his researches led him to conclude—

1. That the brain-working classes—clergymen, lawyers, physicians, merchants, scientists, and men of letters—live very much longer than the muscle-working classes.
2. That those who follow occupations that call both muscle and brain into exercise are longer lived than those who live in occupations that are purely manual.
3. That the greatest and hardest brain-workers of history have lived longer on the average than brain-workers of ordinary ability and industry.
4. That clergyman are longer lived than any other great class of brain-workers.
5. That longevity increases very greatly with the advance of civilisation, and that this increase is too marked to be explained merely by improved sanitary knowledge.
6. That although nervous diseases increase with the increase of culture, and although the unequal and excessive excitements and anxieties attendant on mental occupations of a high civilisation are so far both prejudicial to health and longevity, yet these incidental evils are more than counterbalanced by the fact that fatal inflammatory diseases have diminished in frequency and violence in proportion as nervous diseases have increased; and also that brain-work is, *per se*, healthful and conducive to longevity.

The title of Dr. Richardson's Address was "Salutland; an Ideal of a Healthy People." About three months ago Dr. Richardson and Mr. Chadwick discussed with Professor Owen the question of longevity and the natural duration of life of different classes of animals.

"With his usual scientific accuracy and industrious research, Owen had estimated, from various data he had collected, the natural term of life of that curious animal, the hippopotamus. He had learned that its term of life is thirty years. He explained to us the mode by which he had arrived at that fact; how into the calculation it had been necessary to take into account the dentition of the animal; the stages of development; the natural wearing out of the

teeth; the period of gestation; the development of the skeleton into the perfection of a bony fabric, with particular reference to the combination of the epiphyses or loose ends of the bones to the shafts of the bones; and lastly, the pathological or diseased condition of the dead animal of the species that had arrived at its full longevity, in order to determine whether or not there was evidence of cause of death from disease of some particular organ, or whether there was no such evidence, but simply a history of general decay from old age pure and simple.

"We were told that in a hippopotamus which had recently died, and which was known to have just turned thirty years of age, the two sets of teeth had fulfilled their allotted duty; that the bones of the skeleton were duly consolidated; and that the organs of the body were equally degenerated; so that death had occurred not from failure of any particular organ, but from failure of the organic parts altogether. In a sentence, the animal had died a natural death, and the constant of the term of life of it and its family was set down at thirty years, a constant to which all the facts that could be collated in respect to this species of animal definitely pointed.

"From this line of facts in respect to one type of animal life we were led to others, and the rule laid down by the distinguished Flourens—by which the determination of natural old age is calculated on the basis of perfected maturity—was brought under review. The skeleton is perfected when the epiphyses or loose terminal parts of long bones are firmly united with the shaft of the bone. When the date of such perfection of development is known in the mammalian class of animals, the simple process of multiplying the age at that date by five gives the natural anatomical life of the animal. The elephant came before us as an example. A young elephant, whose history has been related in the "Philosophical Transactions," died at the age of thirty years. At that age the epiphyses of its bones were not completely united with the shafts. It was nearly, but not quite, matured. Multiply thirty by five, and one hundred and fifty years stand as the natural estimate of the life of the elephant; so that really an elephant might exist which had itself carried all the Governors-General of our Indian Empire. Moving from this animal of long life, we turned to the camel, to find full maturity at eight years, full life at forty. We turned to the horse, to find full maturity at five years, full life at twenty-five. We turned to the lion and the ox, to find full maturity at four years, full life at

twenty. We turned to the dog, to find full maturity at two years of age, full life at ten. We turned to the cat, to find full maturity at eighteen months, full life at seven and a half years. We turned to the rabbit, to find full maturity at one year, full life at five.

"From these contemplations our minds very naturally reverted to the animal, man, to the members of the human family. Man, we learned, follows the same rule as the rest of living beings. Judged by the same test, his full maturity and full age may be calculated with equal precision. His maturity—perhaps not quite the full maturity—is twenty years. His full age, therefore, is one hundred years. This is the anatomical estimate of human life, the surest and by far the best of all that can be supplied, since it defines a law irrespective of and over-riding all those accidental circumstances of social and physical storm and strife which may interfere, and indeed do interfere, with every estimate based on the career of life itself, as it is shown in the ephemera by and through whom it is phenomenally demonstrated."

"This lesson," Dr. Richardson continues, "struck Mr. Chadwick and myself with singular force. On a surer basis than we ever trod, it corroborated a view we had ourselves promulgated from entirely different stand-points: and it further corroborated a similar view which had been advanced by our eminent friend, Dr. William Farr. We were led, in a word, once again, to the inevitable conclusion that man, even in this stage of his probation on the planet, is naturally destined to walk upon it, endowed with sensibilities of life and intelligence, for a period of one hundred years, and that until he realises this destiny practically he is—in value of physical life—actually degraded far below his earth-mates whom he designates the brute creation, and over whom he presumes to exercise his, to them, almighty will.

"To certain parts of the scheme of natural life there is a boundary. The period of maturity of development has its boundary of twenty years,—when the body, as Flourens says, ceases to grow; but if it ceases, in the ordinary sense of the term, to grow, it does not cease to increase; its nutrition improves and perfects for twenty years more at least, and then only has reached its completed physical condition. It should never from that period gain in weight, and for a long time it should not lose. It goes on now through a third period, which Flourens admirably calls the period of invigoration, during which all its parts become firmer, all its functions more certain, all its organisation more perfect;

and this period covers thirty years. At seventy old age begins; the *first* old age, in which naturally the fruits of wisdom are most bountifully developed, and which lasts from fifteen years to twenty, to mellow down to a period of *ripe* old age, commencing at eighty-five years and lasting fifteen years more, *i.e.*, until the constant is attained.

"And yet there need not now be death; for though, as Lord Bacon has said, old men are like ruined towers, and though, as Flourens has quoted, youths live in a double sense, with forces in reserve and forces in action, *vires in posse et vires in actu*, the *radical* forces and *acting* forces of Barthez, while old men live only on the forces in action, '*vires in actu*,' possessing no reserve, it is wonderful how the forces in action will continue after the reserve is withdrawn. This kind of half-life has continued unquestionably many years beyond the fulness of age, both in man and lower animals, and to give it twenty years beyond the natural hundred is to be just without being in any extreme sense generous.

"What we call death is gravitation; what we call disease is some accidental shot inflicted, it may be, while still the self-resistance to gravitation is in operation; what we call natural death is the gradual overweighting, at different periods, of the natural powers, reserve and acting, by the persistent force that bears us down. We cease to grow at a certain stage of our life, because of the resistance of this downward force; we cease to increase in size from the same cause; we consolidate in structure from the same cause; we bend in old age from the same cause; and we die from the same cause. Every step has practically been a death from the same cause.

"If the civilised world would continue in the ascendant, it must learn to live. An average life of forty-one, and under favourable circumstances of forty-nine years, with a world of disease and death up to that period, and a scattered struggle of the fittest for an exceptional existence into ripe old age, cannot maintain the relative efficiency of any nation, except in a world universally and equally bad."

As an answer to the question how civilised man should live in order that the natural term may be found, Dr. Richardson created an ideal people, having an ordinary term of life of one hundred, and prospective term of one hundred and twenty years.

This ideal people, believing that all cities have an equal right and an equal importance, possessed no capital city, no special seat of government, no professed politicians, no

army, no lawyers, and no paid physicians, every inhabitant possessing a knowledge of the laws of life and health. Animals are, however, used by this model people. Their fleeces are used for clothing, their milk for food, and many of them are made to work. The elephant works with an intelligence and skill that is almost human, and with a power that is superhuman, so that he is one of the most useful and faithful, and best-beloved of all the lower animals in the land. He is the rival of the horse, which is also much cared for, and is bred in a state of great perfection for bearing the rider, to which duty he is mainly consigned.

The roads leading from one part of the country to the other are maintained in the most perfect efficiency, smooth in all parts, and dry as our best asphalte of to-day. Transit along these highways on horseback and by velocipede has supplanted most other modes of personal conveyance. The cost of coal has rendered steam locomotive power very limited, while aërial locomotion has replaced steam-propelled carriages in a marked degree. But that which has effected the greatest change in respect to locomotion has been the facility with which persons in all parts of the Commonwealth can converse by telephone at any distance from each other, the act of journeying at a pace above 40 miles an hour being considered an unnecessary expenditure of means and physical energy.

In the course of scientific development the philosophers of Salutland were led slowly to the demonstration that, in every case, crime and insanity are synonymous psychological conditions. Descending from the highest insanities to the lower and the lowest, they traced out perfect analogy; they detected that on matters of crime many men and women might be mad, while on other matters they were sane, and even capable of performing good and useful works. Thus, analysing natural facts, they became in time bold enough to act on what they had learned. The man who can commit a crime is insane, and must be treated accordingly. To punish such a man in the ordinary meaning of that term is to try to cure one crime by committing another, which is absurd, and would be an indication of general insanity. So the insane man from crime is put with the other insane. He is moved with them to a separate colony, where, according to the nature of his offence and the character of his affection, whether it be deeply hereditary or not hereditary, and so on, he is subjected to a seclusion in which he can do no one any harm, and to such supervision and improvement

as may render him fitted to re-enter society. His banishment is softened by the permitted visits of friends, and when recovery is completed he is free. But in confirmed cases where the criminality is incurable, the law is inexorable: incurable mad men and mad women, treated with all imaginable care and consideration, are retained apart to the end of their lives. They must not corrupt others. Most of all, they must not be the fathers and mothers of a new progeny, corrupted by that most silent and potent of all corrupting influences, hereditary taint.

The education of this ideal people is never forced. Hard examinations, prizes, rewards for work,—all these stimulants would be held as mentally poisonous, mere excitants of local emulation, to the exclusion of the general. Learning is universally appreciated, and the office of teacher is amongst the very highest in the social scale.

It is, further, a part of the whole economy of the Saluts that they never dream either of killing themselves or injuring themselves by work on the one hand, or by retirement from work on the other. They hold that a human being is constructed to perform a certain amount of work. His heart is born to deliver a certain number of beats,—say, in one hundred years of natural life, three billions six hundred and fifty millions of beats. He is constituted to develop in the various organs of his body so many trillions of active cells, which make up his molecular organism, and which, duly supplied with force derived from without, are capable of performing so much work as they live and die. More force of heart, more development of cells, more life in short, no man or woman can ever possess than that with which they are primitively endowed, as far, at all events, as are yet known, and all the free will in the world cannot change this one fundamental fact. At the same time free will can do this much: it can use its own as it likes; it can wear out its cell life altogether equally and sedately, and so live the whole allotted time, keeping a good margin; or it can wear out its cell life altogether and rapidly, leaving no margin; or it can wear out the cell life of one particular organ—brain, heart, stomach, liver, kidney—by excessive use and exhaustion of the cells of such organ, and so can kill the whole organisation by the death of one organ, the rest of the organs being still in condition, perchance, for years of activity. He only should retire from active work from whom work retires, is another idea and practice so faithfully followed that every town yields many workers who, like Titian the Great, are doing their full quantity at the centenaries of

their births. For all necessary purposes—such is the easy and equal distribution of labour, and so comparatively light are the tasks of labour—from three to four hours in the twenty-four are sufficient for everything that needs to be done, by the busiest of the busy, to keep the social machine in perfect order.

Thus, in Salutland, ample time is left for the pursuit of every useful, healthy, and ennobling occupation. Its happy people cultivate every beautiful and refined art, every branch of natural science; their literature, chiefly of Nature and of Life, and History of Life, while it has lost none of the brilliancy and point of the present rapid method, is deeper and richer and newer. The age of criticism has passed away. To visit all the planet, make the grand tour of the earth; to know all history by biography, so that no man or woman who has helped humanity a hair's-breadth on its way may escape their appreciative and correct knowledge; to compare all artistic existence with the nature from which it sprang; to read men through the languages they have spoken; to study the physical directions of mental phenomena, and from the repetitions of history to forecast even history: these are the studies of their learned men, and the texts from which the learned impart their stores of erudition. To perfect these studies every means is offered by which, without prize-giving or other false stimulus, the choicest rivalry may be naturally imported. Music is most perfected. The stage maintains its reputation, and is utilised to the grandest purposes. Art in the form of sculpture is encouraged with the greatest care. Architecture is another profession which vies in splendour. Here, too, the true painter has found a home. Science is the unembodied Nestor of the teacher and the taught. To know is to exist; and Science is knowing, existing. For the museums of Science, the collections of the works of the Universal Father, the architect expends his best designing energies, the builder his finest work, the mechanist his choicest skill. Astronomy still heads the line of the pure sciences, chemistry follows, and meteorology, geology, natural history, anthropology, mechanics, and engineering, and other sciences find all their true places. Health science stands alone; it includes all the rest. *Salus salutis; scientia scientiæ.*

While thus, in systematised order, the gentle and refining arts and sciences are cultivated as exercises for the mind, the physical health is tended with equal care. Out-door life is the first thought, and out-door exercises of a skilful and useful character are to the fullest extent encouraged.

All the young are taught to swim, to row, to ride, to skate, to walk with ease and stateliness, to climb, to play at invigorating games, to dance, to speak in public, and to become efficient in the gymnasium. The daily ablution in the bath, the daily exercises of muscle and limb, are made as distinctive necessities as the taking of meals; and, withal, the dress in which the body of both sexes is clothed is made so loose and obedient to every movement that no deformity of body from dress is possible.

With regard to the health of the Saluts they have mastered the pestilential diseases. An epidemic from pollution of air, of water, of food, is with them impossible. The hereditary tendencies to disease are either lost altogether or are so nearly eradicated as to be practically removed. The diseases incident to poverty are stamped out by the removal of their cause. The diseases incident to intemperance and luxury are stamped out by the removal of their causes. The diseases incident to occupation are stamped out by the careful and easy expunging of everything that is injurious in occupations. The diseases incident to worry are stamped out by the abolition of maddening, exhausting, and useless strifes and ambitions. In a word, this people contends only with the natural elements,—the heat of the sun, the flash of the lightning, the changes of atmosphere,—from the fatal effect of which they rarely suffer; and with the one destroying inevitable power, the gravitation of the earth, which brings old age and death.

Thus, with the fewest accidental exceptions, the men and women attain the sacred age. Their death-rate is normal and constant, at eight in the thousand per year, and death itself—painless, final sleep—is hardly more than departure to rest when the day of work is done.

Referring to the simple means by which these results were achieved, Dr. Richardson says these settlers had become indoctrinated in their own land with the elementary truths relating to public health. They had learned the lesson of physiology; they had acquired a certain knowledge of what were and what were not healthy places. They had learned the history of the diseases produced by uncleanliness, had become practised in the useful and innocuous distribution of sewage, and had seen the dangers that arise from pitching dwellings in damp localities, and in building dwellings with materials that absorb and hold water. In accord in spirit, with the best information on these points, they carried out the spirit to the letter in their practice, and so began on a new and sound foundation. Ignoring all thought of false

economy, destroying by fire all carriers and sources of contagion, and providing for the instant isolation of every case of contagious or infectious disease, they stamped out the communicable diseases wholesale, with a success and readiness which were surprises even to the most sanguine preventionists, and which gained for them ten years of life.

The science of medicine, which in its true and honest position is always in the front rank of advancement, was somewhat changed. The doctors continued to keep a correct history of diseases, of the course of diseases, and of the causes of diseases, but they added an equal knowledge of prevention, particular and general, and valued that knowledge most. Dismissing all special modes of cure by particular systems or assumed specifics, they determined to know once and for ever what diseases would not get well without the aid of medicines of any kind, the general conditions for recovery being rendered as perfect as was possible. This discovery of the triumph of preventive art did not, however, satisfy altogether. It left on record the fact that Nature never goes out of her path to cure, and that what has been called the *vis medicatrix naturæ* was as much a myth as any other of the past myths of physic. It left on record, also, that under the happiest apparent external conditions some diseases will run their fatal course as decidedly without medicines as with them.

The diseases which so progressed were, in turn, discovered to be diseases of what we call constitutional type, depending upon heredity. They were four in number: scrofula, with its attendant, pulmonary consumption; cancer; specific disease; and insanity. The majority of the physicians, seeing the results I have named, began at once to teach that, as these diseases were obviously diseases of descent, and were maintained by the intermarriages of persons subject to them, there was only one sure and certain mode of removing them, and that was a common-sense rule that such intermarriages should not be tolerated.

The physiologists, dealing with the two questions of digestion and food for digestion, were led to the conclusion that a considerable shortening of life was induced by the excess of work which was put on the digestive organs. They bore in mind that many persons die from the wearing out of one particular organ, the rest of the organs being still healthy. Of all organs they agreed the stomach is most exposed to this danger. They found, on inquiry, that the stomach was distressed both by quantity and quality of food. Following a suggestion thrown out by Flourens, they

decided, on anatomical grounds, that man was neither herbivorous nor carnivorous, but a frugivorous, or fruit-eating animal. Next they estimated the precise amount of food and of drink that was necessary to support the reserve and the active life in the varied stages of life. Again, they determined the reduction of food that is required when the reserve life is withdrawn, and when the active life being left alone, it is the more requisite that no additional surplus of tissue or fluid, fat or water, should encumber the body; that no excess of force should be supplied to the digestive organs to the deprivation of other organs equally important, and that no over-taxation should be cast on the digestive organs themselves. Step by step, they were led hereupon to the introduction of an entire change of food and feeding. Animals were given up as sources of sustenance; fruits became greatly in demand; the bread tree competed with wheat grain; the banana and the grape were called largely into use; the juices of fruits almost entirely superseded water as beverages; while chemistry, coming in always to the assistance of man, easily transmuted many vegetable substances into the most perfectly digestible of foods for every variety of age and constitution. Of purely animal substances, milk only, and the products of it, butter and cheese, retained full sway. Of the vegetable kingdom not frugivorous, cereals, pulses, tomatoes, potatoes, and other fresh vegetables, with the edible Fungi, retained their useful place; and in respect to quantity of food and drink, not more than half by weight began to be consumed compared with what had been consumed before.

The last of the later advancements in order of time, and the final in order of complete accomplishment of obedience to natural design, had relation to sleep and rest. When the sun became the fellow-workman of the people of Salutland, the redemption of their bodies from premature death was carried out with the fullest success. The people saved millions in money, but this was nothing to the other saving. That nervous system of theirs—that system which takes in the outer universe, which is stirred by its waves, and sleeps, if it be permitted, when the waves sleep—found at last its natural time for work and for rest. All Salutland laid down like one vast living world to enter oblivion, and to wake from it filled with another spell of life, ready and happy to greet another day.

If there were any hope of Dr. Richardson's city and people having any other than an ideal existence, we might

dismiss from our minds the arguments put forward by Dr. Beard in support of his theory; but as there is no likelihood of such a state of perfection being reached, at all events in the present generation, it will be well to consider the conditions which favour the brain-worker, with the view of extending some of them to the muscle-worker, or at any rate of improving his social and intellectual condition. The causes to which Dr. Beard attributes the greater longevity of the brain-working classes are—1. The inherent and essential healthfulness of brain-work. 2. Brain-workers have less worry, and more comfort and happiness, than muscle-workers. 3. Brain-workers live under better sanitary conditions than muscle-workers. 4. The nervous temperament, which usually predominates in brain-workers, is antagonistic to fatal, acute, inflammatory disease, and favourable to long life. 5. Brain-workers can adapt their labour to their moods, and hours and period of greatest capacity for labour, better than muscle-workers.

The study of vital statistics led Dr. Beard, moreover, to the conclusion that, other conditions being the same, the greater and richer the brain the greater the longevity; and he shows that the greatest men of the world have lived longer, on the average, than men of ordinary ability in the different occupations by fourteen years; six years longer than physicians and lawyers; nineteen or twenty years longer than mechanics and day labourers; from two to three years longer than farmers; and a fraction of a year longer than clergymen, who are the longest-lived class in our modern society. The causes of this exceptional longevity are—1. Great men usually come from healthy, long-lived ancestors. 2. A good constitution usually accompanies a good brain. 3. Great men who are permanently successful have correspondingly greater will than common men, and force of will is a potent element in determining longevity. 4. Great men work more easily than ordinary men.

VI. ATLANTIS NOT A MYTH.

By EDWARD H. THOMPSON.

OUR sturdy worker in the copper mine of Lake Superior, finding both himself and his vein of copper growing poorer day by day, determines to seek some more paying claim in the as yet unexplored portion of the copper country. He gathers his kit of tools together and starts, and, after many a hard hour's travel over the wild and rugged country, finds a region with abundant signs of copper, and where seemingly no human foot has trod since creation's dawn.

He strikes a rich vein, and goes steadily to work digging and blasting his way to the richer portions, when suddenly, right in the richest part, he finds his lead cut off by what looks to his experienced eye marvellously like a mining shaft. Amazedly he begins to clear out of the pit the fallen earth and the *débris* of ages, and the daylight thus let in reveals to his astonished gaze an immense mass of copper raised some distance from the original bottom of the pit on a platform of logs, while at his feet lie a number of strange stone and copper implements,—some thin and sharp like knives and hatchets, others huge and blunt like mauls and hammers,—all being left in such a manner as though the workman had but just gone to dinner and might be expected back at any moment. Bewildered, he ascends to the surface again and looks about him. He sees mounds that from their positions are evidently formed from the refuse of the pit, but these mounds are covered with gigantic trees, evidently the growth of centuries; and, looking still closer, he sees that these trees are fed from the decayed ruins of trees still older—trees that have sprung up, flourished, grown old, and died since this pit was dug or these mounds were raised. The more he thinks of the vast ages that have elapsed since this pit was dug, that mass of copper quarried and raised, the more confused he becomes; his mind cannot grasp this immensity of time.

"Who were these miners? When did they live, and where did they come from?" are the questions he asks himself, but gets no answer. However, one fact is patent to him—that, whoever they were, they will not now trouble his claim; and, consoled by this reflection, he goes to work again.

The traveller in wandering through the dense and almost

impenetrable forests of Central and South America suddenly finds himself upon a broad and well-paved road, but a road over which in places there have grown trees centuries old. Curiously following this road he sees before him, as though brought thither by some Aladdin's lamp, a vast city, a city built of stone; buildings that look at a distance like our large New England factories; splendid palaces and aqueducts, all constructed with such massiveness and grandeur as to compel a cry of astonishment from the surprised traveller; an immense but deserted city, whose magnificent palaces and beautiful sculpturing are inhabited and viewed only by the iguana and centipede. The roads and paths to the aqueducts, once so much travelled as to have worn hollows in the hard stone, are now trodden only by the ignorant mestizo or simple Indian. Of this deserted home of a lost race the traveller asks the same question as the miner, and the only answer he gets from the semi-civilised Indian is a laconic "*Quien sabe?*" And who does know?

The curious and scientific world, however, are not so easily answered, and various are the theories and conjectures as to these diggers of mines and builders of mounds and strange cities. One of the most plausible of these—one believed by many scientists to be the true theory—is this: Ages ago the Americans presented a very different appearance from what they now do. Then an immense peninsula extended itself from Mexico, Central America, and New Granada, so far into the Atlantic that Madeira, the Azores, and the West India Islands are now fragments of it. This peninsula was a fair and fertile country inhabited by rich and civilised nations, a people versed in the arts of war and civilisation—a country covered with large cities and magnificent palaces; their rulers, according to tradition, reigning not only on the Atlantic Continent, but over islands far and near, even into Europe and Asia. Suddenly, without warning, this whole fair land was engulfed by the sea, in a mighty convulsion of nature.

Now, this catastrophe is not impossible or even improbable. Instances are not wanting of large tracts of land, several hundred miles in extent, disappearing in a like manner. The island of Ferdinandea sudddenly appeared, and after a while as suddenly disappeared. In 1819, during an earthquake in India, an immense tract of land near the River Indus sank from view, and a lake now occupies its place.

The whole bed of the Atlantic, where Atlantis is said to have been situated, consists of extinct volcanoes. The

terrible Lisbon earthquake of 1755, and the later American shock, created a commotion throughout the whole Atlantic area.

That Atlantis possessed great facilities for making a sudden exit cannot be doubted. Its very situation gives good colour to the narratives of ancient Grecian historians and Toltecian traditions, that "it disappeared by earthquakes and inundations."

Not only is it within the bounds of possibility that it might have occurred, but if traditions so clear and distinct as to be almost authentic history are to be believed, then it did occur. Listen to what one of the most cautious of ancient writers, Plato, says:—"Among the great deeds of Athens, of which recollection is preserved in our books, there is one that should be placed above all others. Our book tells us that the Athenians destroyed an army that came across the Atlantic Seas, and insolently invaded Europe and Asia, for this sea was then navigable; and beyond the straits where you place the Pillars of Hercules was an immense island, larger than Asia and Libya combined. From this island one could pass easily to the other islands, and from these to the continent beyond. The sea on this side of the straits resembled a harbour with a narrow entrance; but there is a veritable sea, and the land which surrounds it is a veritable continent. On this island of Atlantis there reigned three kings with great and marvellous power. They had under their domain the whole of Atlantis, several of the other islands, and part of the continent. At one time their power extended into Europe as far as Tyrrhenia, and uniting their whole force they sought to destroy our country at a blow, but their defeat stopped the invasion and gave entire freedom to the countries this side of the Pillars of Hercules. Afterward, in one day and one fatal night, there came mighty earthquakes and inundations, that engulfed that warlike people. Atlantis disappeared, and then that sea became inaccessible, on account of the vast quantities of mud that the engulfed island left in its place." It is possible that the *débris* said to have been left by this catastrophe might be identical with, or the nuclei of, the *sargazo* fields that, many centuries later, Columbus found almost impenetrable. Again, Plato, in an extract from Proclus, speaks of an island in the Atlantic whose inhabitants preserved knowledge from their ancestors of a large island in the Atlantic, which had dominion over all other islands of this sea.

Plutarch, in his Life of the philosopher Solon, Herodotus, and other ancient writers, speak of this island as a known

fact; and it is impossible to believe otherwise than that Seneca thought of Atlantis when he writes in his tragedy of "Medea"—"Late centuries will appear, when the ocean's veil will lift to open a vast country. New worlds will Thetsys unveil. Ultima Thule" (Iceland) "will not remain the earth's boundary." He evidently believed in the unknown island and continent, and knew it would not remain for ever unknown.

Diodorus Siculus says that "opposite to Africa lies an island which, on account of its magnitude, is worthy to be mentioned. It is several days distant from Africa. It has a fertile soil, many mountains, and not a few plains, unexcelled in their beauty. It is watered by many navigable rivers, and there are to be found estates in abundance adorned with fine buildings." Again he says, "Indeed it appears, on account of the abundance of its charms, as though it were the abode of gods and not of men."

The situation, the description of the country, in fact every particular, agrees precisely with our idea of Atlantis; and what other land now in existence agrees in any way with this description—what islands of magnitude that contain navigable rivers, large fertile plains, and mountains?

Turning from our well-known ancient writers, we find, in all the traditions and books of the ancient Central Americans and Mexicans, a continual recurrence to the fact of an awful catastrophe, similar to that mentioned by Plato and others.

Now, what are we to believe? This, that either the traditions and narratives of these ancient writers and historians of both lands are but a tissue of fabrications, evolved from their own brains, with perhaps a small thread of fact, or else that they are truths, and truths proving that the Americas, instead of being the youngest habitation of man, are among the oldest, if not, as De Bourbourg affirms, the oldest.

Brasseur de Bourbourg, who Baldwin says has studied the monuments, writings, and traditions left by this civilisation more carefully and thoroughly than any man living, is an advocate of this theory, and to him we are indebted for most of our translations of the traditions and histories of the ancient Americans.

To the imaginative and lovers of the marvellous this theory is peculiarly fascinating, and the fact that there is plausible evidence of its truth adds to the effect. With their mind's eye they can see the dreadful events, as recorded by Plato, as in a panorama. They see the fair and fertile

country, filled with people, prosperous and happy; the sound of busy life from man and beast fills the air. Comfort and prosperity abound. The sun shines clear overhead, and the huge mountains look down upon the cities and villages at their feet, like a mother upon her babes: all is a picture of peacefulness. Suddenly, in a second, all is changed. The protecting angels become destroying fiends, vomiting fire and liquid hell upon the devoted cities at their feet, burning, scorching, strangling their wretched inhabitants. The earth rocks horribly; palaces, temples, all crashing down, crushing their human victims, flocked together like so many ants. Vast rents open at their very feet, licking with huge flaming tongues the terrified people into their yawning mouths. And then the inundations. Mighty waves sweep over the land. The fierce enemies, Fire and Water, join hands to effect the destruction of a mighty nation.

How they hiss and surge, rattle and seethe! How the steam rises, mingled with the black smoke, looking like a mourning-veil, that it is, and, when that veil is lifted, all is still, the quiet of annihilation! Of all that populous land naught remains save fuming, seething mud. It is not to be supposed that all perished in that calamity. Long before this they had spread over the portion of the Americas contiguous to the peninsula, building cities, palaces, roads, and aqueducts, like those of their native homes; and adventurous pioneers continually spreading north, east, and westward, their constant increase of numbers from their former homes enabling them to overcome the resistance offered to their progress by both natives and nature, till at last they reached and discovered the copper country of Lake Superior. That they appreciated this discovery is evinced by the innumerable evidences of their works and of their skill in discovering the richest and most promising veins. Wherever our miners of the present day go, they find their ancient fellow-craftsmen have been before them, worked the richest veins, and gathered the best copper; and it is supposed that they continued thus till the terrible blotting out of their native country cut short all this, and left this advancing civilisation to wither and die like a vine severed from the parent stem.

Having no further accession to their numbers, and being continually decimated by savages and disease, they slowly retreated before the ever-advancing hordes. Gradually, and contesting every step, as is shown by their numerous defensive works along their path, they were forced back to their cities on this continent that had been spared them

from the universal destruction of their country, where the dense and almost impassable forests afforded them their last refuge from their enemies, and where—reduced by war, pestilence, and other causes, to a feeble band—their total extinction was only a matter of time. Such is probably the history of this lost civilisation, and such would have been the history of our civilisation had we in our infant growth been cut off from receiving the nourishment of the mother countries.

Within the last twenty-five years all sciences relating to the past and present of man have been enormously developed. Old, worn-out, useless theories have been discarded, new facts have taken their places, discoveries have followed discoveries, each discovery helping to form, link by link, the chain of human history.

We are beginning to perceive that we are but yet young in the knowledge of human history,—that we have as yet picked up but a bright pebble of thought or glittering shell of theory, while before us lies the whole vast sea of human history unexplored. That we are beginning to acknowledge this is a good sign, for, when a man or mankind acknowledge their ignorance, they have at least a sure foundation to build upon.

Again, the spirit of bigotry—the spirit that told men to scorn and deride Galileo and Columbus—is fast passing away, and in its stead comes the spirit of rationality, a spirit that tells men to look upon a new idea or theory, even if it does run outside of the accustomed rut, with a reasoning if not favourable eye. And we have faith, as science grows to grander proportions and dispels some of the mist that now envelopes it, that some day not far distant will bring forward an historic Edison that shall bring together the faint voice of the prehistoric past and the bright clear voice of the present; that some future Champollion will discover, among the ruined cities of the Americas, an American Rosetta-stone that will complete the chain of human history. "The noblest study of mankind is man."—*The Popular Science Monthly.*

NOTICES OF BOOKS.

Spon's Encyclopædia of the Industrial Arts, Manufactures, and Commercial Products. Edited by G. G. Andre, F.G.S. Division I. London: E. and F. N. Spon.

We have here the first section of what promises to be a voluminous, but at the same time a useful work. The arrangement adopted is to some extent like that of Muspratt's "Dictionary of Chemistry,"—that is, we have a series of what might almost be called distinct treatises arranged in alphabetical order. Thus the first half of the present volume is devoted to acids, from the acetic to the tartaric inclusive, the sulphuric being, however, placed before the hydrochloric, nitric, oxalic, &c. Such groupings will render a good index absolutely necessary for purposes of reference. In the meantime it is difficult to say whether certain substances are duly dealt with or not, since, if not found in what would seem their natural place, they may turn up afterwards under some general heading. This is the possible reason for the omission of the arsenic, phosphoric, &c., acids, under the section "Acids."

Under the head "Carbolic Acid" we find mention of a curious fact, biological rather than technological, but well worth putting on record. Even very brief contact of the strong acid with any considerable surface of the lower part of the body is usually fatal, whilst the arms and upper parts may be thus wetted with comparative impunity. "In one instance a man employed at a carbolic acid works, who often had his entire arms covered with the acid, died from the effects of some of the same acid spilt upon his leg." The enquiry is at once here suggested whether this fact is unique, or whether the same rule applies to any other poisonous agencies.

Picric acid is described under its elder synonym, carbazotic acid, the modern scientific name—trinitrophenol—not being given. A full list of the synonyms of every substance is, we think, greatly to be desired in a work like the present.

In treating of arsenic the authors inform us that its price is ruled to a great extent by "rings." In the first six months of 1878 a combination of merchants drove up the price from £7 to £12 per ton in three weeks, all surplus stocks being got rid of by consignment to the United States. "The combination, however, broke down, and the price fell, almost as rapidly as it had risen, to £8 per ton. Such combinations are readily carried out,

because the make is small—only about 8000 tons per annum—and is in very few hands." The supply being so limited it may perhaps be deemed singular that, in addition to its more legitimate uses, arsenic should crop up as it does in "violet powder," and even in French chalk, as has recently happened with tragic result. Such being the case we trust that the authors are mistaken when they say "there is reason to believe that it (the manufacture of arsenious acid) has yet to see its best days both in this country and on the Continent." On the contrary, it seems to us that, in deference to public opinion, very stringent regulations will be adopted at home and abroad concerning the industrial use of arsenical compounds.

In the notice of asphalte, as applied to road-making purposes, we find another interesting case, showing how industrial operations are often frustrated by the operations of speculators. "As often happens to new industrial schemes carried on on so gigantic a scale, it fell into the hands of speculators whose main object was not the successful working of the mine," but gambling in shares. "A ring was formed, which in a few months raised the price of the shares from 500 to 13,000 francs. This did not last, and in a short time the 13,000 franc shares were being offered at 25 francs each. Asphalte, however, was destined to overcome these difficulties, and, though it remained some time in the hands of speculators, it eventually took its proper place as an important and profitable industry."

Under the section on hydrochloric acid we find a somewhat elaborate notice of the noxious vapours question, including the chief provisions of the Supplementary Alkali Act of 1874 (37 and 38 Vict., cap. 43) and the somewhat formidable "recommendations" of the Commission which has recently completed its labours. The definition of the term "noxious gas," in the above-mentioned statute, borders upon the facetious. It is made to include sulphurous acid *except* that arising from the combustion of coals." This is the old—we cannot say good—rule: punish the small offender, and let the great sinner go free.

The recommendations sin by default in overlooking the relative fitness of places. One code is to be laid down for all parts of the kingdom. Any man who should erect an alkali works, say in the Isle of Wight or on the banks of Derwentwater, would encounter no more stringent regulations than if he had gone to work in South Lancashire, in the Black Country, or in the outskirts of Glasgow. We would "recommend," on the contrary, the absolute prohibition, in certain districts, of all works capable of generating nuisances, whether solid, liquid, or gaseous, and in other districts the concession of very great latitude.

It is of course somewhat premature to pass judgment upon a work of which so small a portion only has appeared. It seems to us, however, that whilst the arrangement leaves something to be desired, the matter may upon the whole be pronounced

valuable. Most readers will be able to find here useful information for which they might have elsewhere to make a long and tedious search.

Elementary Lessons on Sound. By Dr. W. H. Stone. London: Macmillan and Co.

The purpose of this little work, as the author informs us, is to "furnish information intermediate between Acoustics and Music proper, supplementary to both"—a task which appears to be fairly fulfilled. In addition, and we suppose as a matter of course in the present day, it gives "a concise outline of subjects required for examination.

Papers, Proceedings, and Report of the Royal Society of Tasmania for 1877. Tasmania: Mercury Steam-Press Offic, Hobart Town.

The Royal Society of Tasmania is active, and is doing the rigt kind of work,—that is, work which can be better done in Tasmania than in any other part of the world. One point, howevr, we cannot help noticing with regret. Of the eleven papers published, seven—and certainly not the least valuable—are dueto two of the Fellows of the Society, the Revs. J. E. Tenisn-Woods and W. W. Spicer. The death or removal of eithe of these gentlemen would leave the Society, judging from presnt appearances, in a very unsatisfactory condition. We have geat pleasure in putting on record the active and useful part plaed by the Governor and the Bishop.

One of Mr. Spicer's papers, "Plants as Insect-Destroyes," gives a very complete summary of phenomena not geneally known. The fungoid genus *Cordiceps*, the species of whichare parasitical on various beetles, wasps, and moths, is particuarly described. Among unscientific observers the opinion stillprevails that the insect, which of course ultimately perishes, is being gradually metamorphosed into a plant. There is, howevr, a rumour that a phanerogamous plant springs, in an apparntly similar manner, from the decaying body of an insect. Accoding to M. le Compte d'Ursel, during his travels in various pais of South America he met with an insect which he describes and figures as a thick, distinctly articulated grub, hard to the tuch. When about to die it buries itself some centimetres deep in the earth, and expands till it assumes the appearance of a ptato,

though still retaining its original shape. A stem is then produced, which shoots up and bears a crop of blue flowers. The author admits that the origin of the plant may be a seed contained in the body of the insect, but he evidently leans to the theory of direct metamorphosis. We wish this insect would have presented itself to Mr. Wallace or Mr. Bates during their abode in South America, so that this strange phenomenon might have been thoroughly examined. The discoverer does not seem to be either a botanist or an entomologist.

A paper on "New Britain and New Ireland," by the Rev. G. Brown, contains some interesting facts in animal geography. Thus the cuscus is found in New Guinea and New Ireland, but not in the intervening island of New Britain. The wallaby has hitherto been found in New Ireland alone. The only marsupial obtained in New Britain was a small flying squirrel. Strange reports had reached Mr. Brown of a race of men with tails said to live in the interior of New Britain. In consequence he arranged an expedition to the locality. Unfortunately one of the party, a sailor, got access to a bottle of gin, and was the cause of so serious an accident that the party had to return to the ship with the wounded man, and circumstances did not allow of a second attempt. This disappointment is the more to be regretted as the discovery of an ape in the islands south-eastward of New Guinea would have been hardly less interesting than the occurrence of tailed men.

In the discussion following upon the reading of a paper by the Rev. W. W. Spicer, on the "Foreign Plants which have been naturalised in Tasmania," particular mention was made of the blackberry and sweet briar, both of which thrive in Tasmania with a luxuriance quite unparalleled in England. The question was raised whether birds ever feed upon the blackberry or raspberry. With Mr. Spicer we think that they do not in Europe, but we are by no means certain.

The same author, in an interesting paper on "Silk and Silk-Producers," gives a satirical account of the medicinal uses to which insects were formerly put. Thus the yellow matter which exudes from the joints of the bilbeetle [? oil-beetle, genera *Proscarabæus* and *Meloe*] was held to be as efficacious in dropsy or rheumatism as in hydrophobia, and no doubt was so." That many of the medicinal virtues ascribed to insects were merely imaginary may well be admitted, but we must guard against supposing that if taken internally they would prove inert. The yellow matter above mentioned would very probably be found rich in cantharidin.

Mr. Morton Allport contributes a paper on the "Acclimatisation of Salmon in Tasmania." He appears to take a hopeful view of the results of the experiment. It may be mentioned that amongst the fishes found off the coasts some are absolutely identical with well-known inhabitants of the British seas, such

as the conger, the sprat, the horse-mackerel and the John Dorey.

We trust that the Royal Society of Tasmania will persevere in the course that it has evidently taken, working out the biology, geology, mineralogy, and climatology of the island.

Colour-Blindness; its Dangers and its Detection. By B. Joy Jeffries, M.D. Boston: Houghton, Osgood, and Co. London: Trübner and Co.

Dr. Jeffries had, it appears, originally intended to produce an English version of the work of Prof. Holmgren, "Colour-Blindness and its Relations to Railroads and the Marine." Being, however, anticipated by an abridged translation which appeared in the Reports of the Smithsonian Institute, he was led to draw up an independent treatise, covering, however, essentially the same ground. It must therefore be distinctly understood that the present volume is not, and does not profess to be, an exhaustive monograph of colour-blindness. The author's aim is essentially practical. He seeks to point out the dangers to life and property which may arise from the employment on railways or on shipboard of men defective in the colour-sense, and to explain in detail the best means for their detection. These dangers are undoubtedly real and serious. If one out of every twenty-five of the male population of civilised Europe and America is more or less unable to distinguish colours,—a truth which appears established,—there is a certain element of probability that the engine-driver or the look-out man, upon whose judgment our personal safety depends, may mistake the colour of a signal and rush headlong to his own and our destruction.

The more speculative, and to many men of Science the more interesting, phase of the subject, is but slightly touched upon. Little new light is thrown upon the ultimate causes of colour-blindness; upon the laws of its production by accident, disease, or immoderate narcotism; upon the *rationale* of its predominating frequency in the male sex, its relative occurrence in different races and nations, and its possible extension among the lower animals. The comparative delicacy of the colour-sense among the two sexes in birds, &c., becomes a very important point. If, as among mankind, the females are the most sensitive to colour, whilst the males are admittedly the most highly coloured, we have a strong confirmation of the hypothesis of sexual selection.

Nor does Dr. Jeffries discuss the alleged recent development of the colour-sense, nor the influence which its varying degrees of perfection may have upon decorative art or upon personal

ornamentation in different ages and in different nations. It would be, however, highly unfair to blame him for omitting subjects which he has intentionally excluded from his plan. What he has undertaken he has executed thoroughly and satisfactorily, and his work is hence an invaluable manual for the higher officials of railways, for the authorities of the navy, and for the proprietors of trading- and passenger-vessels. All such persons will here find what, in this respect, is their duty to the public, and how it may best be performed. To the medical profession it will prove an indispensable guide in the examination of supposed cases of colour-blindness.

The Music of the Bible; with an account of the Development of Modern Musical Instruments from Ancient Types. By JOHN STAINER, M.A., Mus. Doc., Magd. Coll., Oxon. London: Novello, Ewer, and Co., and Cassell, Petter, and Galpin.

THE author of this little book has carefully weighed the abundant though discordant evidences as to the identity of biblical musical instruments. The many sieges which Jerusalem has undergone have destroyed every monumental trace; a few coins are the only Hebrew source of information. Fortunately nations with whom the Jews were in constant intercourse, the Egyptians and Assyrians, supply abundance of material for the determination of the musical instruments in use by them, which may fairly be considered to have greatly influenced the musical resources of the Hebrews.

With respect to the origin of stringed instruments, Dr. Stainer is of opinion that the common hunting-bow is the parent of the whole family; and the series on page 72 show how easily the violin, guitar, dulcimer, and even the pianoforte itself, may have been evolved by a progressive series of alterations. Indeed the comparative anatomy, if it may be so called, of musical instruments seems to have been diligently studied by the author in his endeavour to show the probable nature of the resources of the Jewish orchestra. That the instruments varied at different periods of their history there can be little doubt. For instance, at page 97, Dr. Stainer remarks, "We have the same name (organ) for the single row of about fifty pipes, placed perhaps in a little room, and the mighty instrument of five thousand pipes, occupying as much space as an ordinary dwelling-house, and requiring the daily attention of a qualified workman to keep its marvellous complications properly adjusted; yet each is an organ. May it not have been the case that the '*ugab*,' which in Gen. iv., 21, is mentioned as the simply constructed *wind* instrument, in contrast to the simple *stringed* instrument, the

'*kinnor*,' was a greatly inferior instrument to that which in Ps. cl. is thought worthy of mention by the side of a term for the whole string power." It is only natural to suppose that instruments bearing the same name must have been greatly improved in the course of two thousand years or more.

The modern bell is shown (p. 142) to have been derived by a simple succession of developmental changes from the cymbal. The bell of ancient times could hardly be looked upon as a musical instrument, but was a mere ornamental appendage,—either little cymbals like the jingles on the modern tambourine, or like toy bells placed on dog-collars, &c. The bell proper appears not to have existed until the Middle Ages, and to have originated in Europe.

The last chapter is devoted to the consideration of Jewish vocal music: here the accents appended to the Hebrew text furnish some evidence, but explanations are conflicting, and modern traditions among the Jews of little value.

Dr. Stainer certainly deserves the thanks of musicians, as well as of biblical students, for his painstaking and elaborate researches on one of the most difficult subjects within the range of Archæology.

CORRESPONDENCE.

HEREDITY.

To the Editor of the Monthly Journal of Science.

Sir,—Your article on "The Criminal Law of the Future" is very interesting, and doubtless true; although if Oliver Cromwell had known of it he would probably have said that he had been a fool to stop short with only massacreing the inhabitants of Drogheda, in Ireland, and that Irish proclivities to murder and rebellion "proves only the more convincingly the necessity for their elimination" (p. 594). Furthermore, it would warrant the extermination of Zulus and other tribes.

But the question which struck me was, how is it that the principle of Heredity does not apply to genius? The learned Count de Maistre remarked of France—"A considerable portion of the literary glory of the French, particularly in the great century, belongs to the clergy. Science being generally contrary to the propagation of families and of names. Hence arises the ancient prejudice as to the incompatibility of science with nobility—a prejudice founded, like all prejudices, on some hidden cause. No learned man of the first class has been able to found a house. Already even the names of the sixteenth century that were celebrated in literature and science no longer exist."—I am, &c.,

Willis Nevins.

Cheltenham.

SEA-SERPENTS.

To the Editor of the Monthly Journal of Science.

Sir,—Several alleged appearances of large sea-serpents have been lately recorded, but unfortunately they leave the question of the existence of such creatures precisely where it was. The being which struck and sunk the Norwegian barque *Columbia*, on September 4th, is described as "a fish or some other sea-monster," and may probably have been a large whale. The

creature seen by Captain J. F. Cox, of the *Privateer*, on August 5th, must have been a large ophidian if his description is trustworthy. But he will probably be at once proclaimed unworthy of credit. We are by no means sure that if such a serpent had been seen by the entire scientific staff of the *Challenger* their united testimony would have been accepted.—I am, &c.,

SERPENT-HUNTER.

THE ENDOWMENT OF RESEARCH.

To the Editor of the Monthly Journal of Science.

SIR,—In the able Address delivered by Dr. Pye-Smith, in the Anatomical and Physiological Department, Section D, of the British Association, as reported in your October number, there is one passage which seems to require comment. The learned speaker declared that he "should be sorry to see the endowment of research in biology," and it is to be presumed in other sciences also, "undertaken by Government funds." But those who advocate the endowment of research for the most part make no demands upon Government funds. They urge, first, that the present endowment of "cram" should cease, and that the positions and emoluments now handed over to successful examinees should be conferred upon discoverers; secondly, they demand that the fellowships of our old national universities should be made, as they were intended by their founders, the incentives and the rewards of original thought and research." "Local energy and unofficial zeal" have indeed done great things for England; but what have they done in this particular sphere? What would they even be permitted to do? Suppose a number of wealthy gentlemen agreed to found and endow a college where the highest honours should be attainable by research alone, would it receive official recognition and be permitted to grant any kind of degrees? We think not.—I am, &c.,

SCRUTATOR.

NOTES

Biology.

In a communication to the Academy of Sciences, M. L. Vaillant remarks that the fecundity of axolots being no longer contested, we are compelled to regard them not as a pathological modification,—an opinion still admitted by certain foreign *savants*,—but as a normal metamorphosis conformable to the cycle habitually known among the *Urodeles*. These animals, under certain conditions not yet determined, appear to reproduce themselves in two states—as larvæ and when completely developed. This is a fact not unexampled among the inferior Vertebrates and certain Articulates, as remarked by M. Blanchard in 1868 (*see* "Comptes Rendus de la Reunion de la Soc. Helvetique").

M. Galtier has undertaken a series of experimental studies on rabies, and has recorded the results in the "Comptes Rendus." He finds that rabies may be transferred from the dog to the rabbit, and from one rabbit to another, the predominant symptoms being paralysis and convulsions. It is impossible to say whether the virus of the rabbit has the same intensity of action as that of the dog. The period of "incubation" in the rabbit is shorter than in other animals, not exceeding eighteen days. Salicylic acid injected hypodermically every day for a fortnight, beginning with the fiftieth hour after inoculation, has not been found to interfere with the development of the disease. The saliva of a mad dog, taken from the living animal and kept in water, retains its virulence for twenty-four hours. (It is often asserted that the deaths of human subjects after being bitten by a rabid dog, ascribed to hydrophobia, are really due to a morbid imagination. We wonder if this plea will serve to explain the deaths of the rabbits experimented upon as above.)

According to MM. Callol de Poncy and Ch. Livon, in cases of chronic arsenical poisoning this element takes the place of the phosphorus normally present in the lecithin in the form of phospho-glyceric acid.

Prof. Balbiani gives an account, in "Leçons sur la Génération des Vertébrés," of the embryogenous cellule or vesicule which has been detected by many observers in the ova of various animal groups, vertebrate and invertebrate. He considers it as analogous to a seminal cellule which must exert upon the ovum an action similar to that of a spermatozoid. In many animals, it must be remembered, these elements have not the filamental form and are devoid of mobility. This is the case with most of

the Crustaceans, with the chilognathic Myriapods, the nematoid worms, &c. The germ in the female ovule is formed under the influence of a sort of fecundation exercised by this embryogenous cellule which represents the male element. It is always around this element that the plastic granulations are deposited. This cellule being a primordial male element, we understand that, with certain beings and in certain cases, its action is not limited to the formation of the germ. It may determine, in a manner more or less complete, either merely the first phases of the development of the ovum or even this development in its entirety, producing a perfect animal which constitutes parthenogenesis. Known facts prove that in various animal species—even Vertebrates—unfecundated eggs have undergone a more or less complete development, which, however, in no vertebrate species has been found to lead to the development of a perfect animal. Among Invertebrates—such as certain Lepidoptera and Hymenoptera (*Cynips*, species)—parthenogenesis is far from uncommon, and among the Aphides it has become the rule rather than the exception.

An account of the Cephalic Ganglia of Insects has been presented to the Academy of Sciences by M. N. Wagner. The sub-œsophagian ganglion governs principally the appendages of the mouth, and differs little in its histological structure from the other nuclei of the ganglionic chain. As for the cerebroid or super-œsophagian ganglia, they are the seat of nearly all the functions of the brain among the Vertebrates. Here are the organs of perception, of memory, intelligence, &c. Hence the histological structure is more complicated. Towards the centre of the ganglion are found three groups of small cellules, disposed in stages one above another. The greater or less development of these parts of the nervous system coincides with the intellectual development, and is more manifest in the working ants and bees than in the queens, and still more in the males where there exist mere rudiments of these organs.

M. Bacchi has examined the action of sodium phenate in the treatment of bacterihæmic disease. A drop of blood from a frog which had perished of this disease was injected under the skin of each of two healthy frogs. A day or two afterwards phenate of soda was injected under the skin of one, whilst the other remained without treatment. The former in every case recovered, whilst the latter died of bacterian blood-disease.

M. J. Renaut recommends for histological uses a solution of eosin and hæmatoxylin mixed with glycerin. He places in a test-glass 1 part by measure of neutral glycerin and the same volume of a saturated solution of eosin (aqueous or alcoholic), and then drops in hæmatoxylin prepared according to Boehmer's formula, until the green fluorescence becomes almost impercep-

tible. The violet liquid is then filtered. Its action is found very satisfactory.

According to M. Jolly the iron present in the blood globules exists exclusively in the state of phosphate.

MM. Marat and Ortille have observed that in uræmia the respiratory power of the blood is not strikingly changed until death is imminent. The respective proportions of oxygen and carbonic acid present in the arterial blood is also little altered. Ammonium carbonate is detected in the stomach, and ultimately in the blood also.

At a meeting of the Anthropological Society of Paris, M. Broca gave an account of certain observations made with a young Barbary ape. If shown an uncoloured likeness of any ape it at once gave signs of recognition. On seeing the coloured picture of a Macacus it attempted to search for fleas—the usual mark of friendship among apes. Before the figure of an orang-outang, whether plain or coloured, it expressed at once fear and curiosity; and before that of a sloth, irritation.

M. Dareste has in some instances found the amnios entirely wanting in embryo chickens.

M. Vulpian has been examining the action of certain "heart-poisons" upon the escargot (*Helix pomatia*). The alcoholic extract of the seeds of *Strophantus hispidus* and *muscarin* arrested the action of the heart in this snail in manner analogous to what is observed in frogs and mammiferous animals. Upon the heart of Crustaceans neither of these substances produced any perceptible effect.

The influence of a change of climate upon plants is pointed out by MM. Naudin, of Collioure (Pyrenées Orientales), and Radlkofer, of the Botanical Gardens, Munich, who have carried out a joint series of experiments on this subject. The mean annual temperature of the former station is 14·9°; that of the latter only 5·79°. They conclude that the more northern origin of a seed does not necessarily imply greater precocity in the resulting plant than if it had ripened in a warmer climate, inferences deduced from observations on cereals not being generally applicable. Seeds from a warmer locality may grow more rapidly and strongly in one and the same climate than those obtained from a colder source, as *Sonchus oleraceus*, *Solanum nigrum*, &c. In others, as *Calendula arvensis*, *Malva rotundifolia*, &c., the converse holds good.

Dr. Bureau has communicated to the Zoological Society of France a set of plates showing the transformations of the beak of certain birds of the family Mormonides. The beak has hitherto been considered an organ available for the demarcation of genera. The author, however, has shown that after the

breeding season the beak of *Fratercula arctica* falls off in nine portions. Hence this species has two forms of beak—one in winter, small, and covered at the base with a membrane; the other in summer, thick, large, robust, horny, and trowel-shaped; marvellously well adapted for digging the burrows in which this bird nests. A similar phenomenon is observed in all the birds of the North Pacific comprised in the genera *Fratercula*, *Lunda*, *Sagmatorrhina*, *Ceratorhynca*, and *Simorhyncus*. Hence many species which have been founded merely on the form of the beak will require revision.

MM. Couty and De Lacerda have made a series of experiments on the poison of the Brazilian serpent (*Bothrops jararacussu*). The animals bitten or inoculated with the poison, whether in its natural state or diluted with water, all perished in from two to ten minutes. The first action perceived was one of excitement, variable in its seat, as if the action of the poison was localised sometimes in one organ and sometimes in another. Death was always preceded by a complete paralysis of the brain and spinal chord, with relaxation of the limbs and acceleration of the heart.

M. J. Lichtenstein has carefully studied the metamorphoses of *Lytta vesicatoria* (the Spanish fly), which are exceedingly curious. The larva—like those of the nearly allied genera *Meloe*, *Sitaris*, and *Epicanta*—is piratical, infesting the nests of certain earth-bees, such as *Halictus* and *Andrena*, preying first on the eggs and young larvæ, and afterwards on the store of honey.

According to the very decided testimony of a Persian now resident in Paris, fair-haired persons are not unknown in his country.

M. Percy has communicated to the Société des Sciences Physiques et Naturelles de Bordeaux, a paper on the effects of the parasitism of the *Stylops* upon bees of the genus *Andrena*. Certain species of this genus were formerly supposed to be invariably beset with this parasite, but on closer examination the author discovered that they were merely abnormal forms of other species modified by the presence of the *Stylops*. In *Andrenæ* thus attacked each sex loses more or less its characteristic structure and colouration, and tends to acquire those of the opposite. In a Stylopised female *Andrena* the ovarian tubes are completely arrested in their development, inducing complete sterility. In the male a similar atrophy is confined to one side. These results are merely due to the pressure caused by the presence of the parasite.

A paper on the "Mutual Influence of Graft and Stock" is given in the "Zeitung für Land wirthe" (1878, No. 62, p. 353). E. A. Carrière grafted tomatoes upon common nightshade (*Solanum dulcamara*). Many of the plants reached the height of 3 metres, and yielded abundant fruit, differing little in appearance

from the ordinary kind, but much sweeter and containing fewer seeds. Jerusalem artichokes grafted upon the sunflower attained a gigantic height, whilst the roots of the stock in two places had developed tubers something like those of the dahlia.

At one of the last sittings of the Academy of Medicine Dr. Jollivet reported that sixteen persons, in the Department of Seine et Oise, have been seriously attacked with trichinosis. This is the first outbreak of the disease in France.

M. Fredericq has continued his researches on the blood of the octopus and the lobster. That of the former contains merely a single albuminoid species, the two great functions of the blood, respiration and nutrition, devolving upon one and the same chemical compound, hæmocyanin. In the blood of the lobster, in addition to the blue colouring-matter, hæmocyanin, there is found a rose coloured principle, soluble in alcohol. The blood of certain Gasteropods contains hæmocyanin, but it is not found in that of the Lamellibranchiæ.

Among the Primates the olfactory apparatus, the great limbic lobe, loses its importance. Among the apes there exists always a slight furrow, which extends as far as the fissure of Sylvius. M. Broca formerly considered this limbic furrow as characteristic of the apes, but he has subsequently detected it in all the brains belonging to the lower human races which he has been able to examine.

Abrassin oil is, according to the "Œst. Landwith. Wochenblatt," obtained in China from the seeds of *Elæococca cordata*, and is said to be an excellent protective against noxious insects. The tree, which belongs to the family of the Euphorbiaceæ, prospers in the South of France, where its culture is recommended, in the hope of the oil proving a specific for the *Phylloxera* and the *Oidium*.

The influence of quinine upon silkworms has been satisfactorily proved by C. Le Doux. Broods of caterpillars suffering from *flacquerie* were speedily restored by sprinkling their food with quinine sulphate. The same treatment proved successful in cases of "pebrine" with open wounds.

Cases of poisoning by caterpillars have been observed both in cows and ducks. The former experienced gastric symptoms, diarrhœa, loss of appetite, &c., but were all restored by means of mucilaginous drinks and a diet of boiled potatoes and bran. Ninety ducks were turned into a cabbage-field infested with the caterpillars of the common white (*Pieris brassicæ*). In one afternoon the field was almost clear, but two ducks died after an hour. The next morning twenty were found dead, and altogether fifty-three perished. The flesh of the dead, on examination, was found to resemble that of cattle which die of gangrene, indicating true poisoning.

Chemistry and Technology.

In the International Exhibition, now being held at Sydney, a collection of soils, manures, and agricultural products is shown by the Imperial College of Agriculture in Tokio, Japan. Accompanying the collection is a descriptive catalogue, compiled by Mr. Kinch, the Professor of Chemistry at the College, in which a short account is given of the various products exhibited, with about eighty chemical analyses. The catalogue opens with some analyses of soil. Then follow analyses of manures, including lime, wood ashes, nitre, waste vegetable substances, and residues from various manufactures, fish manure, bone, superphosphate, birds' dung, and hair. Next in order come analyses of foods. Then a summary of the dye-stuffs, and also of the various oils and resins.

In a paper on "Measures for Disinfecting," read at a Meeting of the German Public Health Association, at Stuttgart, reported in the "Sanitary Record," Prof. Hofmann said that disinfection can only be said to have been carried out when the following conditions have been fulfilled ;—(*a.*) By considering the object or poisonous germ which is to be destroyed. (*b.*) By considering the place or object where the poisonous germs may be found or must exist. (*c.*) By a thorough knowledge of the mode of action and the qualities of the disinfectant that is being used.

At the Manchester Meeting of the Social Science Congress Mr. C. T. Kingzett read a paper on "The Eucalyptus and the Pine considered in relation to their Sanitary Properties." The action of the eucalyptus is stated to be of a positive type, and, like the pine tree, its properties are of a healthful nature, upon whatever soil or in whatever climate it may grow. The *Eucalyptus amygdalina* is the most abundant oil-giving tree, 100 lbs. of the leaves giving from 3 to 6 lbs. of the oil. This oil is practically identical in composition with the oil of turpentine derived from pine trees, and with most of the so-called essential oils or perfumes. By the investigations of Mr. Kingzett it has been ascertained that all these oils, when subjected to the action of atmospheric oxygen and moisture, produce peroxide of hydrogen and a number of camphoraceous substances having marked antiseptic powers. Taking New South Wales and South Australia alone, Mr. Kingzett calculates that the eucalyptus forests contain at any given moment sufficient oil in the leaves to form by contact with the atmosphere no less than 92,785,023 tons of pure peroxide of hydrogen, and 507,587,945 tons of camphoraceous principles. If it be remembered that in Nature all matters of animal and vegetable origin are oxidised by the atmosphere, which is thus kept free from the pernicious products of putrefaction, and that peroxide of hydrogen is a much more powerful oxidant than ordinary oxygen, and if it be also borne in mind

that the camphoraceous products above referred to are also powerful antiseptic agents, then the healthful influences of the eucalyptus can neither be wondered at nor be longer open to any doubt. What is true of the eucalyptus is also true of the pine, and on an immensely larger scale; and the oil of turpentine, which is a natural product of these trees, undergoes the same chemical changes in the atmosphere as oil of eucalyptus. By imitating this natural process of oxidation, Mr. Kingzett has, as is well known, succeeded in obtaining and rendering available in commerce the antiseptic and oxidising principles to which pine and eucalyptus forests owe their hygienic influences.

The adulteration of olive oil has become so prevalent that the Minister of Agriculture and Commerce has requested the Academy of Sciences to ascertain the most trustworthy method for the detection of such frauds. Among the procedures at present under examination by a special committeee is the use of the diagometer, an instrument devised by Prof. Luigi Palmieri, founded on the difference of the electric conductivity of oils. Seed oils are as a class better conductors than olive oil. At the same time every oil conducts the better the greater are its impurities. Linseed and cotton seed oil are among the best conductors, whilst the oils of pine seeds and of hazel nuts are almost as feebly conductive as the purest olive oil, known in commerce as virgin oil. Fortunately these two oils are too rare and costly to be used in the adulteration of olive oil. The use of the diagometer requires considerable manipulative skill.

At a recent meeting of the Berlin Dyer's Association the new colour, "Puteaux blue," manufactured by MM. Patry, of Puteaux, came under discussion. It was considered to be in all probability an indulin, but was pronounced not well adapted for wool-dyeing. For mixed colours upon silks, and as a ground for blues, it was said to be applicable. Dr. Reimann, in speaking of "pittacall blue," considered that at no very distant period the products of beech tar would be utilised for the production of artificial tannin.

The system of "weighting" of silks is, remarks M. Marius Moyret, ruinous to the silk-growing departments of France. Their high-class products are no longer in demand, as inferior and cheaper foreign silks serve equally well for loaded tissues. Hence these districts, already suffering from the *Phylloxera* and from the loss of the madder trade, are in the utmost distress. The author proposes that in the sale of silks, as in that of gold and silver, the proportions of the real article and of weighting matters should be exactly specified, and that a central office for the cheap and rapid assay of silks should be opened in Lyon. He states that the most excessive weighting has been carried out by a New York firm.

In connection with the general Congress of German Apothe-

caries, held at Hanover on September 4, was exhibited a collection of pharmaceutical antiquities, including a specimen of genuine oriental bezoar, a goblet of metallic antimony, formerly used for imparting a purgative effect to wine or beer, a venerable sample of "Album Græcum," a selection of chemical apparatus inscribed with alchymistic symbols, a gaily-adorned jar for containing "Mithridate," &c.

Instead of linseed-meal as a lute for distillatory apparatus, Thanisch proposes the use of strips of brown paper smeared over with bookbinders' paste to which one-eighth of glycerin has been added.

The chemical purification of waste waters has been studied by Jean de Mollins. The author, who has dealt with the waste waters from the woollen mills of Roubaix, recommends milk of lime and sulphate of alumina, together or separately. He considers that the action of the aluminous hydrate upon the organic impurities of water is quite analogous to its behaviour with dissolved colouring-matters which it throws down in the form of lakes. He also points out that clay, if diffused in water, becomes coagulated by the presence of certain salts, and carries the organic impurities down with it.

A new method of producing varnish is proposed by Dr. E. Schrader. He causes ozone to act upon linseed oil: it is at once perfectly bleached and brought to the proper consistence without the aid of fire.

From a report by M. de Luynes (Société d'Encouragement pour l'Industrie Nationale) on M. C. Lorilleux's manufacture of printing and lithographic inks, we find that two kinds of varnish are employed in the manufacture of printing inks; the one obtained by boiling linseed oils, and the other from a mixture of resin and resin oil, the latter being chiefly used for newspapers where rapid drying is of importance. M. Lorilleux allows his linseed oil to rest for two years at a constant temperature. It is then boiled by means of hot air, at a distance from the furnaces so as to remove every risk of fire. A mass of 2500 kilos. is boiled from twenty-four to fifty-six hours, and is stirred by a mechanical agitator. The varnish thus obtained is limpid and flows well. The lamp-black is produced either by means of specially constructed lamps or by the decomposition of naphthalin oils, which fall by drops into a heated retort. The gaseous products are carried off by tubes, at the end of which they are burnt under sheet-iron bells, whilst the black is carried off by a current of air into large chambers. It is afterwards submitted to calcination. All the inks, lithographic or typographic, are submitted to a practical trial before being sent out.

Some researches on damage to the soil and the crops by the waste waters and the gases from manufactures have been insti-

tuted by Dr. J. König. These researches relate, in the first place, to the waters from certain mines of zinc-blende in Westphalia, which pass into certain streams used for irrigation, and seriously injure the productiveness of the soil. The presence of zinc oxide in the earth is indicated by the presence of *Viola calaminaria*, which contains in its ash as much as 21 per cent zinc oxide. The author has likewise examined the waste waters from a dye works, a wire works, and from pyrites washing. The two latter he considers as directly poisonous to plants on account of their percentage of ferrous sulphate. The dye water may, by reason of the organic matters which it holds in solution or suspension, gradually overload the soil with humus, and render it boggy.

M. de la Bastie is said to have made great improvements both in the method of tempering glass and in the quality of the product. Among the articles shown to the French Société d'Encouragement are mortars with their pestles for chemical use. At the Liverpool Meeting of the Iron and Steel Institute Mr. C. Wood described the process of Mr. Frederick Siemens, of Dresden, for the employment of toughened glass for sleepers and chairs for railways and tramways. Mr. Siemens tempers and moulds the glass into various forms to suit the different requirements. The cooling of the glass is so regulated that the radiation from each point of the surface corresponds to the thickness of the glass, thus enabling the casting to be equally affected throughout when undergoing the tempering or hardening process. The regulation of the radiation or absorption of the heat in the thicker parts of the casting is done by having hollow iron moulds, and by circulating cold water or cool air at those points where the glass is thickest, so that the casting cools equally all over. The toughened glass is found to be almost as strong as iron, and it possesses greater durability. Mr. Bucknall, C.E., proposes to make the sleepers out of blast-furnace slag, under Mr. Buckley Bullan's process combined with the toughening process of Mr. Siemens.

At the Meeting of the Academy of Sciences on October 13th, M. Berthelot explained the plan of his new work "Essay on Chemical Mechanics founded upon Thermo-Chemistry." The first volume of this work is devoted to calorimetry. The second volume comprises the general study of chemical composition and decomposition. The remainder of the work is devoted to chemical statics.

A new manure is proposed by M. de Molon. Finely-ground phosphate of lime is mixed with sea-weeds, especially varec, the mass being allowed to ferment for six to eight weeks.

A statement is quoted in "Les Mondes" of October 2nd, from the New York "Popular Science Monthly," that a Boston chemist found in a sample of cream of tartar 75 per cent of *terra*

alba, and that at Chicago, a chemist, in want of antimony sulphide, could find in the shops merely marble-dust blackened with soot.

Metallurgy, Mineralogy, Mining, &c

In a recent report of the proceedings of the Manchester Literary and Philosophical Society there is a description, by Charles A. Burghardt, Ph.D., of a precious garnet (almandine) from Ramsbottom, Lancashire. Dr. Burghardt was asked to examine certain red granules present in a small specimen of rock, in order to ascertain whether they were garnets or not. The rock itself is a conglomerate of milky quartz grains, cemented together with silica and calcium carbonate; and disseminated throughout this conglomerate are the garnet grains in question, the whole constituting a 14-feet "fault" in the coal-seam known as the Sand-rock or Feather-edge Seam in the "Shipperbottom Mine," near Grant's Tower at Ramsbottom. A microscopical examination of the rock showed the garnets to be very irregular in form and to vary considerably in size, the largest attaining a width of about 2·25 m.m., and the smallest about 0·75 m.m. Both the garnets and the quartz grains could be extracted from the matrix, a cast of their forms being left behind. The red grains fused easily and quietly before the blowpipe to a black bead, which was not, however, magnetic: they were somewhat attacked by hydrochloric acid, with a slight separation of powdery silica; the presence of iron was also detected. The hardness of the mineral was 7·5, and its specific gravity 4·09 at 9° C. Dr. Burghardt hopes shortly to determine the chemical composition. A microscopical examination of the garnet grains showed them to be crystalline, exhibiting certain marked peculiarities, and after a careful examination of many of them he came to the conclusion that the form in which they crystallised was the rhombic dodecahedron. It is somewhat difficult to draw a conclusion as to the origin of these garnets in such a locality. From the microscopical examination of the rock it would appear that the cement and the garnets were *both* in a soft and pasty condition, the latter being probably formed whilst in the pasty matrix, and prevented from attaining a normal development. The quartz grains were probably formed previously to the garnets, as they appear to be waterworn pebbles, and were most likely carried along with the semi-paste-like mass which contained the constituents of garnet and calcite in its substance. When the paste hardened, the quartz grains and the garnets were enclosed in the manner exhibited on the specimen of rock from Ramsbottom. All the constituents of garnet were at hand, namely, aluminous shale, which would furnish the alumina and silica, and iron pyrites, which would furnish the iron, whilst water impregnated with calcium carbonate, acting upon the shale

and iron pyrites, would bring about the necessary chemical changes, calcium sulphate being formed and carried away, and the garnet solution could then crystallise out. Another explanation of the occurrence of garnet in the conglomerate may be as follows:—Garnet is a very common accessory of crystalline rocks, occurring mostly in slates of various kinds, such as talc slate, mica slate, chloritic slate, and aluminous slate; it also occurs in gneiss, granite, porphyry, serpentine, and granular limestone. One or more of the above-mentioned rocks containing crystallised garnets already formed were disintegrated by the action of water, and the resulting *débris* (consisting of grains or pebbles of quartz and garnets) was carried away and deposited in a semi-fluid mass of silica and calcite, and thus cemented together.

Mr. F. P. Venable, of the University of Virginia, has analysed an alloy known as "Tungsten-Manganese Bronze." A fragment cut from a small block of so-called tungsten-manganese bronze, of light gold-yellow colour, pretty close grain, and with a fine polish upon one side of the specimen (sp. gr. = 8·64), was carefully tested qualitatively, and the proportion of the constituents found were thus determined. No manganese whatever was present, and but an insignificant amount of tungsten. The alloy consisted of—Cu, 86·51; Sn, 9·04; Zn, 3·47; Fe, 0·26; W, 0·23; total, 99·51. It is therefore but an ordinary gun-metal, with part of the tin replaced by zinc. Of course manganese may have been added in its production, as well as a larger proportion of tungsten; but if so, they have failed to be taken up, or have been burnt out before the alloy was cast; and manganese so removed may have served in a measure to improve the compactness and homogeneity of the mass by carrying off with it oxygen from the remaining metals, as in the use of phosphorus in making "phosphor-bronze." But the name under which the alloy is sold is calculated to mislead purchasers.

Barff's process for the prevention of corrosion on iron surfaces is being applied on a very large scale. The process, shortly, consists of passing superheated steam over the iron goods to be treated whilst at a red-heat, and can be applied to all kinds of iron-work, rendering it absolutely rustless at a less cost than galvanising, so substituting an absolute protection for one which confessedly is but partial in its action and easily removed.

From the Annual Report by the Keeper of Mining Records we learn that the production of coal in the United Kingdom during 1878 was estimated at 132,607,866 tons, valued at £46,412,753. The quantity of iron ore is given as 15,726,370 tons, valued at £5,609,507. There were produced 29,867 tons of iron pyrites, worth £19,099. Of pig-iron there were 6,381,051 tons produced from ores obtained in the United Kingdom, valued at £16,154,992. The total decrease in the production of coal in the year 1878, as

compared with that of 1877, was 1,955,876 tons. The production of pig-iron in Germany during 1878 was 2,124,444 metr. tons; zinc, 94,954; lead, 84,372; copper, 9541; tin, 831; antimony, 1245; coal, 39,429,308; lignite, 10,971,117; asphalte, 47,329. The total amount of pig-iron produced in France in 1878 was, according to "Engineering," 1,508,246 tons, most of which came from the Meurthe-et-Moselle. Pig produced with the aid of coke is greatly in demand, and is driving out charcoal pig and mixed sorts.

An attempt is contemplated to work coal-mines in the neighbourhood of Ching-men-Chow, not far from Tchang. Boring operations were commenced late last autumn. The coal-producing country appears to cover an extent of 758 square English miles, 15 long by 5 broad. There are two layers of coal, one above the other. It is supposed that 1,200,000 tons of coal can be raised from Wotzukow, and 800,000 from San-li-kang, at the rate of 40,000 tons a year. The supply thus would last at least forty years. It is highly probable that further explorations will bring to light fresh beds, as these discoveries are the result of merely the first investigations. Specimens of all the native and foreign coal procurable in China have been analysed together, and the new coal has shown itself superior to all for smelting purposes.

Requiring large quantities of platinum for his electro-chemical telephone, Mr. Edison issued, some few weeks ago, a circular letter of enquiry with regard to the possible occurrence of platinum in various parts of the United States. From the "Scientific American" we learn that Mr. Edison received some three thousand replies to his letter. Platinum, instead of being an extremely rare metal, seems to be widely distributed, and to occur in considerable abundance. It is found where gold occurs, and Mr. Edison thinks he can get 3000 lbs. a year from Chinese miners in one locality. One gravel-heap is mentioned from which a million ounces of platinum are expected!

Physics.

Since the Lontin electric light was first used at the Gaiety Theatre several important improvements have been effected. The improved machine possesses three advantages, viz.—the low speed at which it works, thus reducing wear and tear, risk of accidents, and overheating; the ready division of the light; and the production of alternate currents. The number of different circuits which one machine can generate, and the number of separate lights obtainable from each circuit, are, it is said, only limited by the size of the machinery. The machines now in operation at the Aldersgate Street Station of the Metropolitan

Railway have been constructed by Messrs. Harding and Co., of Paris. The generator works at a speed of 400 revolutions a minute, and the inductor at half that number. The motive power is supplied by a Fowler 25 horse-power high-pressure compound engine, capable of working up to 60 horse-power, which will probably also be used for producing electric lights in the Metropolitan Meat-market and at other stations on the Metropolitan Railway. The cost of coal and carbons is reckoned at less than 3d. per hour for a lamp equal in lighting-power to 600 candles, with an expenditure of from 1 to 1¼ horse-power. The lamp employed is the Serrin-Lontin, and by the most recent improvement the carbons will only require renewing once in fifteen hours. A great advantage is that the lamps can be lighted or extinguished independently of each other. The experiments at the Aldersgate Street Station are being carried out by the Electric Generator and Light Company. The first evening this light was exhibited, Mr. Harding, of Paris, explained the progress and capabilities of the Lontin system. "The most interesting news on this subject is,"—remarks the "Telegraphic Journal,"—"the statement made by Mr. Harding, at the exhibition of the Lontin light, to the effect that Mr. Crookes is engaged on some experiments with the electric light *in vacuo*, which promise to work an entire revolution in the prospects of the light. Carbons would be dispensed with, and the lamp would consist of a luminous globe, giving the light of (say) three candles. Thirty of these lamps would, it was thought, be maintained by an expenditure of 1¼ horse-power, at a cost of less than a penny per hour. We have always believed that there was something to be made by some one, in the way of electric lighting, out of Mr. Crookes's researches *in vacuo*, and we are gratified to hear that that eminent physicist is so near success himself. Mr. Edison and he are trenching upon the same ground with vacuo; but whereas the former uses metal electropyres, the latter uses none. Both are on the proper track, it seems to us, namely, the production of a perfectly steady light without any wasting of carbons or other perishable parts."

The Siemens electric light is now employed in the Reading Room of the British Museum. The main difference between the method adopted by Messrs. Siemens and that experimented upon some time ago by the Société Générale d'Electricité consists in the use of four very powerful lamps hung high over the heads of the readers, instead of a number of less powerful lights at a lower elevation. These four lights are produced by continuous currents passing through wires carried over the roof of the Reading Room and through the top of the glass lantern in the centre of the dome. The electricity is supplied by five dynamo-electric machines placed in a shed at the back of the Museum buildings at about 200 yards distance, and worked from two 8-horse-power semi-stationary engines, supplied by Messrs. Wallis and

Steevens, of Basingstoke. The regulators are the invention of Messrs. Siemens and Halske, of Berlin. They each contain 19 inches of double carbon, which are consumed at the rate of 3 inches an hour. The lights consequently will burn for a little more than six hours without the regulators being interfered with. Seven other lights, of which two are placed in the Court Yard, one under the portico, one in the Hall, one in the passage leading to the Reading Room, and the remaining two in the engine house, are worked by two alternate current machines, and the regulators are actuated by two coils acting on a differential principle, one of them forming part of the main circuit and tending to separate the carbons, the other tending to bring them into contact. The position of the carbons depends therefore not upon the strength of the current, but upon the relative amount of electricity passing through each coil. The carbons burn for about five hours, and may be renewed in half a minute. The wires supplying the current for these regulators are laid partly in the basement of the building, and partly in pipes under the ground. The light given out by each of these lamps is equal to 600 candles, and their steadiness says much for the excellence of the principle upon which they are constructed. The powers of the light were severely tested by several of the gentlemen present. Mr. George Bullen, Keeper of the Printed Books, put it to several practical working tests in different parts of the room, and found that at the desks which are farthest from the centre there was a decided lack of light for those who, like Mr. Bullen, have somewhat impaired their sight by incessant literary labour, while at the catalogue desks and in the centre of the room, the light was much more than sufficient to read even the smallest print. The diameter of the Reading Room it will be remembered is exactly 140 feet, the half of which is 70 feet. As at present placed the three outer lamps hang at a distance of about 35 feet from the centre, and of course at an equal distance from the wall. A moment's consideration will show that according to this arrangement the central portion of the room gets an overplus of light, while the part near the wall is underlighted. Mr. Bullen consequently suggested that the three outer lamps should be hung at a distance from the centre of two-thirds of the radius of the circle, by which means each third of the line between the centre and the circumference would be equally illuminated. By moving the outer lamps nearer the wall the outer row of the catalogues will be taken out of the deep shadow in which they were thrown by the arrangement adopted on Monday. Dr. Carter Blake submitted the purity of the light to some very searching colour-tests, and found that the most delicate tints were as easily distinguishable as in bright daylight.

In his excellent lecture on "Electricity as a Motive Power," delivered to the working men at Sheffield during the Meeting of the British Association, Prof. W. E. Ayrton demonstrated that a

dynamo-electric machine, with a separate exciter, driven very fast with a steam-engine, or with a stream of water, at high or low pressure, and sending, by even quite a fine wire, a small current to a distant electro-motor, also running very fast and magnetised by a separate exciter, is an economic arrangement for the transmission of power. As an illustration of what had been done by electricity as a motive power, Prof. Ayrton referred to the experiments of MM. Chrétien and Felix, who last year at Sermaize (Marne) ploughed fields by electricity, the electric current being produced by two of Gramme's dynamo-electric machines. These machines were usually worked with a steam-engine at some convenient place 300 or 400 yards away, in an adjoining road. The electro-motors were also two Gramme machines, one on each side of the field, with their coils revolving backwards. Through one of these the electric current was sent alternately, so that motion was given to one or other of two large windlasses, one on each of the waggons containing the electro-motors. In this way the plough, which could be used going in either direction, was first pulled across the field, making a furrow, and then back again, making another parallel furrow. M. Chrétien's electric crane has also been successfully employed for several months at Sermaize, in the harbour there, and it is considered that a saving of about 30 per cent has been effected of the expense formerly incurred for unloading the sugar-barrels out of the boats. Calculation showed that if electric currents—generated by very large steam-engines at certain points, and by turbines driven by the falling water on the hill-sides around Sheffield—were substituted for the use of coal for motive power, smelting, heating and lighting buildings, that a saving of something like £400,000 a year might be anticipated for that town. During the lecture electric lights, fed from a distance, were used. The current from a dynamo-machine at the works of Messrs. Walker and Hall, a considerable distance from the lecture-room, also set in motion an electro-motor on the platform. This in turn caused another dynamo-machine to rotate rapidly and produce a current with which the lecturer gilded a piece of plate. It was also shown that the Sheffield Water Company had a considerable surplus supply of water which could be used for producing motive power; and as an illustration of such a use of the water-power, a 2-inch board was sawn on the platform by a circular saw driven by an electric current generated by a water-engine in the yard of the Water Works, and conveyed to the hall by wires crossing the streets. In referring to the fact that electricity can produce not only motive power, but also light and heat, Prof. Ayrton said that Dr. Siemens had been able to effect a saving of about 30s. an hour, at the Royal Albert Hall, by replacing the old gas jets by electric lamps. The science of hanging a brilliant light high up had been luckily allowed to ride over the precedent of putting a number of feeble glimmers all over the building.

A new thermo-electric battery has been invented by M. Clamond for the production of the electric light. With a consumption of 9½ kilos. coke, a current is produced capable of maintaining four lamps, each equal to twenty-five Carcel burners.

In the American "Quarterly Microscopical Journal" Mr. F. H. Wenham gives some useful information relating to practical methods of obtaining parabolic forms. The mode of forming the template is simple and ingenious :—A cone of hard wood or metal is turned, removed from its centre, and fastened by one of its sides on a suitable chuck ; a section is made in the lathe parallel to the attached side ; a piece of accurately flattened brass, somewhat larger than the section, is fixed to the portion of the cone by screws, and above that a piece of similar material to that from which the cone was turned ; it is then returned to its centre, and the cone turned down to its original form : the metal plate has its edges finished by holding to it a "dead smooth file." The cone is unscrewed, and the brass plate comes out a true parabola. The block of glass is turned with an old saw file and plenty of turpentine until it approximately fits the template ; the paraboloid is then turned with a keen edge until the template, smeared with reddle and oil, marks it evenly all over ; the marks are ground out with a small block of brass and emery of graduated fineness, until all scratches disappear. The glass is polished with a buff stick and crocus and water, and finally a piece of hard bees' wax is held against it, with finer crocus, to obtain the last degree of polish. If the paraboloid be a non-immersion one, with a cupped top, it may be turned flat on the end until the required thickness is arrived at, and the hemispherical cavity roughly turned out to a half-circle template till the centre is brought to the focus : the cavity is then finished in the same way as a concave lens. Finally, when rotatory in the lathe, the paraboloid is perforated through the axis with a steel drill and turpentine.

A new "Catalogue of Electric Apparatus" has been issued by Mr. E. Paterson, Bedford Court, Covent Garden. This Catalogue has the double merit of being copious and well illustrated with nearly 160 woodcuts. The first part contains a priced list of apparatus used for instruction and demonstration, while the second deals entirely with practical matter or applied electricity. In the second part a clear description is given of the construction of electrical apparatus intended for house signalling, bells, telegraphing short distances, &c. The telephone and electric lamp are also described. The directions given for fixing electric house-bells are sufficiently clear and devoid of technicalities to be of service to any ordinarily intelligent builder or bellhanger.

THE MONTHLY

JOURNAL OF SCIENCE.

DECEMBER, 1879.

I. A VISIT TO THE ANCIENT OBSERVATORY AT DELHI, LOCALLY KNOWN AS THE "JUNTER MUNTER."

By H. A. Tracey, Major R.A.

DURING the visit of H.R.H. the Prince of Wales to Delhi, in the winter of 1875-76, my battery, among many others, was temporarily stationed there, and after H.R.H.'s departure I employed our few leisure days in visiting the interesting remains of ancient grandeur that dot the country for 10 or 12 miles to the south of the modern city.

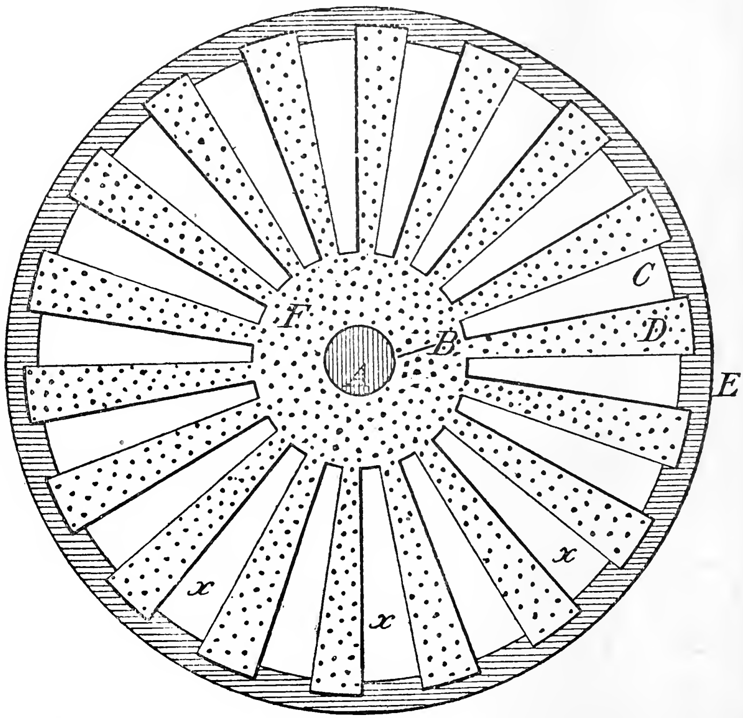
One of the most conspicuous is the great Sun-dial, erected by Rajah Jey Sing, about A.D. 1720. Its gnomon is a very remarkable object, rising stiff and angular some 60 feet above the plain. Those who mount the narrow stairs that lead to its summit find it crowned by a "Lingam," sure sign of a worship of "Baal" as descended to these days. There were the yellow flowers and grains of rice on and about it, showing that it is a "high place," still used by the neighbouring peasantry.

This Sun-dial, though the most prominent, is not the most important part of the remains of the Observatory. Two low towers, about 24 feet high and 60 feet in diameter, attract one's attention when on the gnomon, and lie due south of it. As seen from that more elevated spot their cylindrical exterior is at once noticed, and the observer has also the opportunity of remarking that the tower is open to the heavens; that a central pillar rises in each to the same height as the walls, which have deep vertical recesses on

their inner surface, so that, while the exterior plan of each tower would be represented by a circle, a cogged wheel would show the shape of the floor inside.

From a certain spot on the flight of steps running up the gnomon, the central pillar of each tower can be observed as being truly level with its surrounding wall. From this spot another peculiarity can be seen:—The projecting teeth of

FIG. 1.—GROUND PLAN OF TOWERS. $\frac{1}{200}$ Scale (Natural).
Shaded portions brickwork; dotted parts rank grass and filth.



A. Centre of pillar. A B. Radius of pillar, 32·5 inches. A C. Radius of interior of tower, 325 inches. A D. Radius of cogs, 350 inches. A E. Radius of exterior of tower, about 360 inches. A F. Unoccupied interval, about 130 inches. *x x*, &c. Raised sectors with "ivory" cement covering.

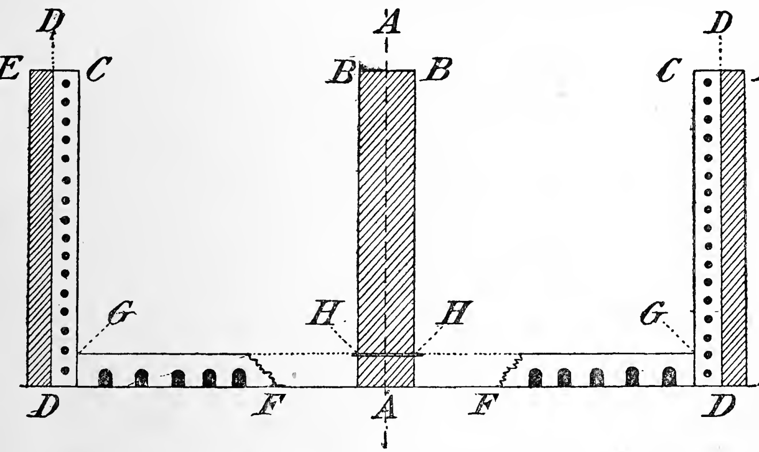
the imagined cog-wheel are apparently the same as the intervals between them. Both the towers seem in every respect alike, except that the nearest of the two towers has its northern cog in a line with us and its central pillar, while the furthest tower has one of the intervals between

the cogs in the same line, as if indeed the southern tower had been turned in azimuth through the space of one cog. With this exception the towers seemed similar.

The towers are well built of brick, and plastered. A small doorway in each led to the inner floor, 5 or 6 feet below the present level of the adjacent ground.

In the interior the first rough plaster is covered with a fine hard one everywhere; and that again on the portions marked with lines, &c., for observation, bore a still finer plaster, that, wherever I remarked it, had a surface, touch, and colour that can fairly be compared to ivory.

FIG. 2.—VERTICAL SECTION THROUGH CENTRE. $\frac{1}{200}$ Scale (Natural).



A A. Centre of pillar. A B. Radius of pillar, 32·5 inches. A C. Radius of interior of tower, 325 inches. A D. Radius of cogs, 350 inches. A E. Radius of exterior of tower, about 360 inches. A F. About 130 inches. C G. 292·5 inches. C B G H is a square, and the angle C G B = 45°. The dots along C G are the pigeon-holes for the feet.

A zealous "Sub" helped me in making the measures from which the accompanying plan and section of the towers are drawn.

To commence with the plan, Fig. 1.

The floor may roughly be described as a cart-wheel in shape, the eighteen spokes of which are raised from the ground (see Fig. 2) on five—I think, but not recorded in my notes—low arches. The intervals between the spokes are of equal breadth with the raised parts, and are at present covered with the exuviæ of the human and animal population of the neighbourhood. These portions of the floor each extend about 2 feet radially into the thickness of the

wall, and, carried up vertically, give the cog-wheel-like appearance to the inner plan of the towers, as noticed when looking down on them from the gnomon. On each side of these recesses are pigeon-holes, of a convenient size for the feet, so that, by straddling across from side to side, an observer can easily mount, resting his back against the wall, up to the top, and bring his eye with ease to the edge on either side, on which are marked the angles of depression from the edge of the flat top of the centre pillar. Along these vertical edges the degrees are numbered in Hindoo (Sanscrit), not Arabic figures, the Zero being at the top, and the 45th degree at the angle where the vertical and horizontal portions of fine work meet (see the angle marked G in Fig. 2). The Zero point (C, Fig. 2) was in every case removed. (Every fifth degree was marked in figures, the others by a line only.) I walked round the summit of each tower to assure myself of this, as I was anxious to find one *in situ*. It had apparently been marked by the edge of a stone about a foot square, and so would be a convenient size for grinding curry-stuff on. Such stones are in daily use by all the inhabitants of the country, and being ready cut and levelled would naturally be soon appropriated. And the flight of pigeon-holes led directly up to them.

Each of the observing recesses had three windows, lancet-shaped, together forming three rows of windows round each tower: they were of no apparent value, and were draughty and dangerous for an observer. Some were bricked up, and I regret I did not record the position of these, as it might have given an idea of the direction from which observations were most frequently taken.

The eighteen spokes being of the same width as the intervals between them, it follows that the circular floor was divided into thirty-six equal divisions of 10 degrees each. And now we learn the value of the *two* towers—as the raised "spokes," or "sectors" as we shall prefer to call them, in one occupied the place of the unmarked floor in the other. Each of these raised sectors, and the vertical wall adjoining them, had been once covered with the fine "ivory" cement I have spoken of. But little of it remains on the broad surfaces, as it seemed to have suffered from the weather and the frosts not uncommon here in January and February; though in the salient angles formed by the recesses I have mentioned it was more perfect than elsewhere, and the lines and figures on it were generally legible. They had apparently been drawn by some blunt fine-pointed instrument before the cement was quite dry. Each sector

was divided radially into six divisions by five deep lines, and each division into five subdivisions by finer lines. So each large division represented 10° divided by 6, or—

$$\left(\frac{10^\circ \times 60'}{6}\right),$$

100 minutes of azimuth angle; and consequently each finely-marked subdivision equalled 20′ of angle.

I am not able to say positively that these finer subdivisions were continued on the vertical walls. I saw some parts where the fine cement appeared intact, but not within reach, where I *thought* the finer lines apppeared, but I was not certain. They did *not* exist on the index edges, where the written numbers of the degrees were marked. The inner ends of the sectors were much damaged, the open space between them and the centre pillar being anything from 6 to 8 feet.

The centre pillar has a ring of well-cut stones, eighteen in number, on a level with and corresponding to the eighteen sectors. They are of the fine red freestone of the district, and project from the pillar about 2 inches (not exactly recorded in my notes). Their upper surface is level and finely dressed, and divided equally by radial lines into six parts. The space between each stone—equal in width to the stone itself—is occupied by a red stripe running vertically the whole length of the pillar. The pillar itself is divided above the level of the projecting stones, by clearly-marked red rings, into five horizontal blocks, each block being subdivided by finer horizontal lines into nine smaller blocks.

Judging by eye, and with the assistance of my Lieutenant on the opposite side of the tower, it was clear that the middle of each of the bolder red rings was intended to be (and indeed was) on the same level as the 36°, 27°, &c., marked on the index edges of the observing recesses. And the marks were as closely accurate as we were capable of discerning with the unaided eye. The level stone on the summit of each central pillar was in its place, with the edge angle very fine dressed.

In the northern tower, which is the one nearest the Sun-dial, the prolongation of the gnomon's western face is marked by a window, unlike the others in the tower. It is of lancet-shape, but extends from second to third row of windows counting from the ground; its *apex* marks the prolongation of the north and south line, while the same line on the opposite side of the tower falls exactly on the

edge of another window, signalised by being splayed out on that particular side into an exceptional shape, and leaving the edge required for observation dressed with the fine "ivory" cement. The N. and S. line only just clears the centre pillar. The same line is carried through the southern tower, falling on the other side of its centre pillar (I think so at least, though I did not record it in my notes made on the spot), and its position is again marked by fine edges of the "ivory" cement, though the windows are not of such exceptional shape as in the northern tower.

All the above lineal measures were made with a folding rule of box-wood, by Adie, of Edinburgh, and specially tested at the Observatory of that city for accuracy. It is 25 inches in length. One side of the rule was marked to tenths, and the other to half-tenths of an inch.

The measures on the walls and sectors we considered trustworthy; those connecting the walls with the centre pillar were carried out with difficulty. A plumb-line was dropped from some well-marked position at the edge of a sector, and then, by means of two boards alternately moved, the measure was carried step by step across the filth that covered the ground. A tight string gave a straight line, but no special means were taken for levelling. Direct measures from wall to pillar were also made in the same manner.

The general result of our measures were—

	British inches.
Radius of the interior of the tower	325·0
" " centre pillar	32·5
Height of finely-cemented portion of tower, inside above the sectors: mean of several measures	293·8

In this last measure, which theory demands should be 292·5 inches, it must be remembered the original index edge had been removed for the vertical, and we were guided by eye alone, taking the centre pillar's top and the opposite side of the tower as our guide.

I noticed that the index markings for vertical angle were not too truly marked; several had double lines. Those in azimuth were not too accurate. The box-wood rule allowed of its being bent against the inner circumference of the wall, and the width of the divisions in azimuth were carefully recorded.

	Inches.
8 are each recorded as	5·75
1 is recorded as	5·70
1 " "	5·72
Mean	5·74

Each of these divisions represent 100 minutes of horizontal angle.

Having completed, as well as time permitted, our measures of the towers, we devoted our spare time on the second day to a closer inspection of the great Sun-dial.

It is built on the edge of a natural, or perhaps artificial, depression in the ground, so that, though it only stands some 60 feet above the general level of the country, its perpendicular face really has a front of about 90 feet, the excavation at its foot giving it that additional height. From each side of its foot, and (in plan) at rather less than a right angle, sweep up the great arcs on which the shadow falls.

The Western one is quite ruined : a large badger appeared its only tenant, and watched us. The Eastern one had enough remaining to enable us to follow the shadow over the few existing legible markings, by mounting a very narrow and hazardous flight of steps which followed the sweep of the arc. As I mounted the ruined steps, spare remnants of the "ivory" cement called my attention, and it was clear that the crest of the parapet between me and the gnomon was the dial on which the time was marked.

Waiting till the sun was low I was able to watch the shadow moving over some of the more perfect portions of the scale. The workmanship was kindred to that of the towers—brickwork, rough plaster, finer plaster, and for the inscribed portions the "ivory" cement. I was not able to measure the angular divisions of the circle on this arc, but I could see the markings were finer than could be read, or, in other words, the penumbra on the edge of the shadow was wider than the space between each of the finer marks, so that its fall on each could not be noted accurately by my unpractised eye. These smallest divisions were arranged in groups of five. A blank concentric space of the same angular value marked each block of five, and an inner curve again gave larger divisions, but there was no portion of plaster left large enough to count how the enumeration was commenced.

An eye accustomed to watch the record of this great dial could no doubt read it to a fraction of a minute ; but with such a broad penumbra no two observers could, I think, give similar independent readings.

On our two visits to the top of the gnomon we took frequent measures of its angle ; they all lay between—

$$28^\circ\ 15' \text{ and } 28^\circ\ 45' \text{ (mean } 28^\circ\ 30'),$$

or close on the real latitude of the spot.

The substructures and architectural arrangements for the Eastern arc of the Dial were made subservient to many purposes, chief of which we noticed a narrow gallery or chasm running due N. and S. I hardly know how to describe it. In ground plan it would be a narrow parallelogram, about 6 feet wide; in side elevation, wedge-shaped—its central line in the direction of the mean angular midday elevation of the sun; the vertical height of this wedge being that of the arc, or about 60 feet. The apex of this wedge is a well-cut horizontal slit, about 2 inches wide, in a finely-dressed stone, and not to be noticed at first in the ruined condition of the outside; but inside the curious vault at midday its meaning is evident, as the line of sunlight falls across an arc, ready marked, recording the sun's vertical height at noon during the year.

The visit was an interesting one, but I left the "Junter Munter" with the idea that it was only the scientific toy of a rich man, with yearnings maybe ahead of his time; and when "Jey Sing" was gathered to his fathers, I imagine the "Junter Munter" soon fell into a respectable disuse.

II. THE ACTION OF LIGHT ON PLANTS.

IN his Presidential Address at the Sheffield Meeting of the British Association, Dr. Allman remarked that in by far the greater number of plants the protoplasm of most of the cells which are exposed to the sunlight undergoes a curious and important differentiation, part of it becoming separated from the remainder in the form usually of green granules, known as chlorophyll granules. To the presence of chlorophyll is due one of the most striking aspects of external nature—the green colour of the vegetation which clothes the surface of the earth; and with its formation is introduced a function of fundamental importance in the economy of plants, for it is on the cells which contain this substance that devolves the faculty of decomposing carbonic acid. On this depends the assi-

milation of plants, a process which becomes manifest externally by the exhalation of oxygen. Now, it is under the influence of light on the chlorophyll-containing cells that this evolution of oxygen is brought about. The recent observations of Draper and Pfeffer have shown that in this action the solar spectrum is not equally effective in all its parts; that the yellow and least refrangible rays are those which act with most intensity; that the violet and other refrangible rays of the visible spectrum take but a very subordinate part in assimilation; and that the invisible rays which lie beyond the violet are wholly inoperative.

In thus acknowledging the labours of Draper and Pfeffer, Dr. Allman forgot the previous researches of Mr. Robert Hunt, F.R.S., who investigated the subject forty years ago. Dr. Allman, however, in a letter to Mr. Hunt, observes, "I have been refreshing my memory of your researches by reading your published account of them, and their completeness and conclusiveness render my regret the greater that I had not given them the recognition they so well merit." The results of Mr. Hunt's "Researches on the Influence of the Solar Rays on the Growth of Plants" were described to the British Association in 1842 and 1846, and are given in his "Researches on Light."* In his history of the progress of the inquiry, he refers to Dr. Priestley's experiments in 1779. These researches, which showed that carbonic acid was absorbed by the plant, that under the influence of light it was decomposed, and that its oxygen was again liberated, were confirmed by the experiments of several naturalists and chemists, including Senebier, Ingenhousz, DeCandolle, Saussure, and Ritter. The general result of the investigations on the chemistry of vegetation up to this point was, Dr. Hunt observes, that light was essential to healthful vegetation, but that the decomposition of the carbonic acid by the plant took place more decidedly under the influence of the most refrangible range of the spectrum than of those which possessed superior illuminating power. In 1801 Labillardière communicated to the Philomathic Society his discovery that light was necessary to the development of pores in plants; and about the same time Victor Michellotti announced that light has a decided action on those germs which are exposed to it,—that this action is prejudicial to them, and it manifests its action by retarding their expansion if the light be weak or a deflected light, or by total

* Researches on Light in its Chemical Relations, embracing a consideration of all the Photographic Processes. By Robert Hunt, F.R.S. Second Edition. London: Longmans. 1854.

extinction of their life if it be very intense, as that which comes directly from the sun. M. Macaire Prinseps observed that sheltering leaves from the action of light prevents their change of colour in the autumn; that if the entire leaf was placed in the dark, it fell off green; if only a part, the rest of the parenchyma changed colour, and the covered portion retained its original colour.

Mr. Hunt's own experiments extended over seven years; they were made at every season of the year, and the localities in which they were carried on were changed from the south-western extremity of the kingdom to the neighbourhood of the metropolis. The problem he wished to solve was the proportion and kind of influence exerted by light, heat, and actinism in the various stages of vegetable growth. The experiments were made under the action of those radiations which had permeated variously coloured media, such as tinted glass and coloured transparent fluids. We should like, did space permit, to refer to the many precautions taken in order to render the experiments exact and conclusive. We can, however, only give the results, referring our readers to Mr. Hunt's own account for further particulars of his observations. These results are as follow:—

1. Light prevents the germination of seed.
2. Actinism quickens germination.
3. Light acts to effect the decomposition of carbonic acid by the growing plant.
4. Actinism and light are essential to the formation of the colouring-matter of leaves.
5. Light and actinism, independent of the calorific rays, prevent the development of the reproductive organs of plants.
6. The heat radiations corresponding with the extreme red rays of the spectrum facilitate the flowering of plants and the perfecting of their reproductive principles.

In order to ascertain the exact conditions in which the luminous actinic and calorific principles exist, Mr. Hunt made frequent examinations of the condition of the solar radiations. He found that in the spring the actinic principle is most active, and as compared with light and heat it is considerably in excess. As the summer advances the quantity of light and heat increases relatively to the actinic principle in a very great degree. In the autumn light and actinism both diminish, and the calorific radiations are relatively to them by far the most extensive.

The term *light* is here used to express all those rays of he spectrum which are visible to a perfectly-formed human eye; by *actinic principle* is meant the principle to which the phenomenon of chemical change under solar influence belongs; and by *calorific radiations*, not merely those effects which are traceable by any thermometric instruments, but also those which we can detect by the protection from change produced by a class of rays existing near the point of maximum heat in the spectrum. In the spring, when seeds germinate and young vegetation awakes for the repose of winter, we find an excess of that principle which imparts the required stimulus; in the summer this exciting agent is counterbalanced by another possessing different powers, upon the exercise of which the structural formation of the plant depends; and in the autumnal season these are checked by a mysterious agency, which we can scarcely recognise as heat, although connected with thermic manifestations upon which appears to depend the development of the flower and the perfection of the seed."

The phenomena which the prolonged action of sunlight produces on vegetation in high latitudes are recorded by M. J. A. Broch, in a work recently published.*

The farther we go eastward from the Gulf Stream the more severe is the climate, even though the degree of latitude be the same. Thus Scandinavia and Finland possess an exceptionally mild climate, considering their high polar altitude. Indeed barley and oats will ripen in the most northern districts of Norway, Sweden, and Finland, and immense forests are met with; whilst in Iceland, Greenland, and the Polar confines of Russia and America, the earth is bare and sterile, and there are eternal snows. The cause of these advantageous climatic conditions is to be attributed to the enormous mass of warm water and hot air which the Gulf Stream brings down from the Equatorial region to the coast of Norway, which coast it approaches between 60° and 61° of latitude. This circumstance, together with the difference in the geological formation of the various northern countries of Europe, naturally lead to certain dissimilarities in the respective climates of these countries. Comparing Norway and Sweden, for instance, in the former the sun is moist, cloudy, and the quantity of rain considerable, the winters mild and the summer cold; while in Sweden the sun is brighter, the

* Le Royaume de Norvège et le Peuple Norvègien, par J. A. Broch, Ancien Ministre de Norvège.

air drier, the quantity of rain less, the winters are cold and the summers hot. The isothermic line passing through the places whose mean temperature is zero,—skirting in Norway the chain of mountains and the sea coast from the North Cape, embracing also the central part of that country between the 60th and 63rd parallels—begins in Finland at the 66th degree of latitude, and rises rapidly to the north, forming a curve which encloses the elevated lands of the interior between the Gulf of Bothnia and the Arctic Sea, so that not only the countries situated south of that parallel, but also those which slope towards the Arctic Ocean and are submitted to the salutary influence of the Gulf Stream, have a mean temperature above zero. Of all the countries situated in the same latitude as Finland, the Scandinavian peninsula alone enjoys a milder climate. European Russia is much colder, and the climate of Asiatic Russia still severer. The neighbourhood of the sea and the abundance of lakes—in the number of which no country in Europe, or perhaps in the world, can compare with Finland—cause a tolerable quantity of rain to fall, and render the climate somewhat humid.

With regard to the action of prolonged solar light on the vegetation common to all those countries, Dr. Schübeler, of the University of Christiana, has demonstrated that the seed of corn or other plants obtained from the northern regions ripens more quickly than that produced in the more southern countries. In the regions of the extreme north, where grain crops are uncertain in their yield, owing partly to the elevation of the land above the level of the sea, the seed corn of the north is always used in preference to any other. It is not less true that the various kinds of grain and vegetables cultivated in the northern regions yield better and are much richer in carbo-hydrates than the varieties cultivated more to the south. The colour, moreover, is deeper—a phenomenon which applies also to all trees and plants. Foreign botanists visiting Norway, and the other countries of the extreme north, in summer, are astonished at the fresh dark green of the foliage and the bright colours of those flowers which grow both in northern and southern climes; and as this richness of colour increases regularly with the latitude, trees and plants have at first been considered as new varieties. The leaves of trees grown in the north are larger even when the seed has been brought from more southern countries. M. Schübeler has likewise proved that the aroma of all kinds of plants and fruits, both wild and cultivated, increases as the north is

approached. Ordinary vegetables and herbs grown in high latitudes have a far more aromatic taste than those grown in more southern countries. The meadow cumin (*Carum Carvi*) is an example of this fact: grown at Christiana it contains 5·8 per cent of volatile oil, whilst that cultivated in Germany and Central Russia contains only from 4·0 to 4·8 per cent. But this large development of aromatic essence is not always considered an advantage; for instance, the tobacco plant grown in Norway or other northern countries contains, it is said, too much nicotine. In proportion, however, as the aroma increases with the latitude the saccharine substances diminish: the berries and fruits of the north are less sweet than those which are cultivated or grown wild in the more southern parts of those countries. Consequently whilst Norway as well as Sweden, and even Finland, produce the most delicious apples, the pears are not sufficiently sweet.

These facts, as well as the rapid growth of vegetation in the northern regions, are attributed to the prolonged action of solar light. Indeed at Christiana, at the summer solstice, the sun remains below the horizon only 5 h. 17 m.; at Froudheim, 3 h. 34 m. At Bodô, the chief town in Nordland, the sun does not descend below the horizon from the 2nd June till the 11th July; at Tromsô, from the 20th May to the 24th July; at Hamerfest, the chief town of Finmark, from the 15th May to the 29th July. On the other hand, the centre of the sun does not appear above the horizon at Bodô from the 14th to the 28th December; at Tromsô, from the 25th November till the 16th January; and at Hamerfest, from the 20th November to the 21st January.

It is not surprising that barley, potatoes, and many other plants and vegetables ripen in the most northern latitudes, seeing they are exposed to a considerable amount of heat during two or three months of the year. In those regions where the sun hardly descends below the horizon in summer, there is no night, only a short twilight; the growing plant therefore enjoys permanently, and without interruption, the heat and light which it requires.

III. TECHNICAL EDUCATION IN ENGLAND, FRANCE, AND GERMANY.

THE City of London Guilds and other corporate bodies seem at length to be convinced of the absolute necessity of adopting some measure for the advancement of technical education in England. As far back as the Paris International Exhibition of 1867 our English masters and workmen awoke to the fact that the leading position which we had formerly occupied as makers of the world's goods was being endangered by the talent and enterprise of foreign nations. The first note of alarm was sounded by Dr. Lyon Playfair, in a letter addressed to Lord Taunton, the Chairman of the Schools Inquiry Commission then sitting. The aim of this communication was to enquire whether England was really losing her high position in those industries which involve the application of scientific knowledge to production, and, if so, whether this retrogression was due to our comparative backwardness in the diffusion of a knowledge of applied science amongst the working classes. The British Commissioners appreciated the warning at its proper value, and, taking advantage of the presence in Paris of some of the most eminent British men of science of the day, they consulted them on the subject, the result being that, with scarcely a single dissentient voice, they affirmed that the lack of technical education on the part of British masters and workmen was slowly, but surely, undermining the position of Great Britain as mistress of the industrial arts.

Speaking generally these salutary warnings have been neglected, although in some few isolated instances they have been duly acted upon. These praiseworthy efforts have for the most part been the work of individuals, and as such have only wrought good in particular localities, anything like combined action being entirely wanting.

Amongst the latest utterances on this vitally important subject, the paper on "Apprenticeship Schools" read by Prof. Silvanus Thompson before the British Association at Sheffield, and just republished in pamphlet form,* and the Address of Prof. Ayrton at the opening of the City and Guilds of London Institute,† are the most striking. The

* Apprenticeship Schools in France. By SILVANUS P. THOMPSON, B.A., D.Sc., F.R.A.S., Professor of Experimental Physics, University College, Bristol. London: Hamilton, Adams, and Co.

† The Improvements Science can effect in our Trades and in the Condition of Our Workmen. By Prof. W. E. AYRTON.

gist of these able contributions to our knowledge of the subject is that our present system of apprenticeship is utterly rotten, and must speedily be replaced, under the penalty of seeing the whole of our trade with foreign nations gradually drift away from us. During the last half century apprenticeship, as it was understood by our forefathers, has ceased to exist except in name. The master of the present day, unlike his predecessors, seeks his own benefit instead of his apprentice's, and looks more to what he can make out of him than what he can teach him. A boy of fourteen enters a workshop willing and anxious to learn his trade; for the first year or so he finds himself in the position of a mere errand-boy, or at any rate the servant instead of the pupil of his superior. As soon as another apprentice can be found to do his drudgery he is set to some particular branch of work, and if the shop be a large one he will very probably be kept at it till the end of his term. He is placed under a workman from whom he learns but slowly, seeing that his teacher, being constantly employed on his own work, has but little time to teach him,—the evil reaching its highest point in places where piecework is the rule. It is no one's duty to teach him, and, as it formed no part of the contract between employer and employed, the journeyman very justly refuses to expend any very great portion of his time in instructing his master's apprentice in the secrets and mysteries of his handicraft. As for the master, the boy receives no help from him, even supposing that he is competent to teach him. He consequently picks up his knowledge of one small branch of his trade in an unintelligent and desultory manner, and leaves the workshop at the end of seven years capable of doing only one thing, and that by rule-of-thumb, just as his shopmates have done before him. How differently things were managed in what may truly be called the good old days of apprenticeship! In those times the master was also a workman, and laboured at his craft. He had learned every branch of it, and understood it so thoroughly as to be able to teach it to others. Capital and steam have together created gigantic factories, and the old domestic workshop—in which each worker formed part of a kind of family—gradually became the exception; the master craftsman became the mere employer, and the apprentice the boy worker.

The connexion between the depression of trade in skilled industries and the question of proper technical education, as well as the hopelessness of attempting to galvanise the old system of apprenticeship into life, is well pointed out by Mr.

George Howell in the "Contemporary Review" for October, 1877.

The question now is, what modern substitute for the old system can be adopted to the wants and wishes of the nineteenth century? Prof. Thompson's investigations happily enable us to lay before the reader the actual results of certain experiments recently made in France with a view to organising a new system of apprenticeship that shall be more in accordance with the social conditions of the present day. These results prove that the systematic instruction of apprentices is possible in several different ways; that apprenticeship schools afford a most satisfactory way of attaining this result; and, lastly, that the new system solves the problem involved in the decay of the old apprenticeship. The problem to be solved, stated briefly, is this:—How to give artizan children the technical training and scientific knowledge which their occupation demands, without detaining them so long at school as to give them a distaste for manual labour. The problem may be solved in four ways, all of which have been tested:—

First. We may apprentice children at an earlier age than at present, making it obligatory that all through their apprenticeship they shall every day have a certain number of hours of schooling in a school attached to the workshop.

Secondly. The children may be kept at school for a longer period, on condition that they shall pass a certain amount of time in a workshop attached to the school.

Thirdly. We may organise a school and workshop side by side, an equal number of hours being devoted to manual labour and study.

Fourthly. We may send the children for half the day to the existing schools, and the other half to work half-time in the workshop or factory.

The first of these plans strongly commends itself to our attention, for the knowledge imparted in the school could be correlated to the work done in the factory, to the manifest benefit of both the employer and the employed. This system has been tried in France for the last thirty years, and the establishment of MM. Chaix and Co., the French Railway Guide printers, may be cited as a type of the whole. MM. Chaix's typographical school—for such it really is—has been in existence for seventeen years, and has

supplied nearly a hundred able workmen to the firm itself, and the few who have left have found exceptionally good situations. The apprentice is bound for four years, the employers guaranteeing him a place when he is out of his time. They are divided into two classes, compositors and printers. Close to the composing- and press-rooms there is a schoolroom, where the apprentices of both classes spend a couple of hours daily, either in improving their knowledge of the three R's or in going through a technical course of typography, including grammar, writing and composition, reading and correcting proofs, the study of the different kinds of type, and so on. They are also taught to read and set up in type Greek and Latin, without any attempt to instruct them grammatically in these languages; and they are taught the rudiments of English and German. Lastly, there is a course on such subjects as the history of typography, or mechanics, physics, and chemistry, as far as they apply to printing machinery and processes. During the three years the apprentice compositors receive from 5*d*. to 2*s*. per day, and the printer apprentices from 7½*d*. to 3*s*. 8*d*. At the end of the term most of the apprentices prefer to remain in the employment of the firm, and can then earn from 3*s*. to 6*s*., according to their ability. Great pains are taken to systematise the teaching. The compositor apprentices are set to work under the direction of a foreman whose chief business is to instruct them, and not to work for his own or his employer's benefit: he is, in fact, a professor of printing, just as Professor Thompson is a professor of Physics.

MM. Chaix's establishment, it must be understood, is only one of over two hundred similar schools in different parts of France, in which a similar system of instruction is given in the manufacture of optical instruments, shirts, jewellery, paper, Italian paste, ribbons, calicoes, plate glass, silks, bookbinding, and a dozen other branches of trade.

A great impetus has been given to this kind of apprenticeship schools by the passage of a law, in 1874, forbidding the industrial employment of children under 12, except they receive two hours schooling per day; nor may children over 12 and under 15 be employed for more than six hours per day, unless they have finished their elementary education, their employers being made personally responsible for carrying out these regulations.

In the school of M. Soufflot, a jeweller, the character of the instruction is purely technical. The success which has attended the "school on the workshop system," as Professor Thompson aptly calls it, must not only be extremely grati-

fying to all who have the cause of technical education at heart, but it must also prove to the attentive observer that, being the most natural, it will eventually become the best system of all.

The second type of school includes those in which systematic instruction in one or more handicrafts is given to boys who are still going on with their elementary instruction. There appears to be one school of this sort in Paris, which is carried on most successfully as far as it goes, only about 12 per cent, however, of the pupils receive manual instruction. They work alternately at carpentering, wood-turning, forging, filing, chipping, and metal-turning for two years; after which they specialise their work. They also receive instruction in modelling and technical drawing, and in the summer they visit the neighbouring factories. On the completion of the preliminary two years they are draughted off into one of the three special workshops in which modelling and carving, carpentry and wood-work, and iron- and metal-work are carried on under the superintendence of master-workmen who have made the teaching of their various crafts a special study. One of the disadvantages of this type of school is, that the instruction given is professedly only preparatory to, and not a substitute for, an ordinary apprenticeship. In its favour it must be conceded that it shortens the long and useless years of apprenticeship, and thus helps the young worker to become a bread-winner.

The third system is where the school and the workshop are placed side by side, so that the hours given to study should be co-ordinated with an equal number of hours of manual instruction. This type of school Prof. Thompson thinks is the *apprenticeship school* of the future. France affords two good examples of this class; one the Paris Municipal School of Apprentices, where several distinct trades are taught; and the Besançon Municipal School of Horology, where clock and watch making alone are taught. Taking the Paris school first, we find that the apprentices are only admitted between the ages of 13 and 16. They must also have a certificate showing that they have completed their elementary education, or else undergo an examination. In comparison with schools of the second type a larger amount of time is devoted to the workshops, which are here much more extensive and complete. The course is a three years' initiation into the handicraft taught, and the majority of the pupils leave the school able workmen. The trades in which direct instruction is given are those of the carpenter, wood-turner, pattern-maker, smith, fitter, and metal-burner.

That the school turns out excellent workers may be judged from the fact that the average age of the pupils who left the school in 1877 was $17\frac{1}{2}$ years, and their average earnings in the places they had obtained was 3*s.* $1\frac{1}{2}$*d.* per day, one boy of 17 getting as much as 5*s.* $4\frac{1}{2}$*d.* per day as a smith. The instruction is entirely gratuitous, and the whole of the necessary tools, machines, books, &c., are supplied by the Municipality. The system pursued in this school appears to be of the very highest order, and should serve as a model for all future schools of the kind. The Besançon School of Horology is managed on similar principles, and is a striking success. The school is managed and supported entirely by the Besançon Municipality. In addition to instruction in every branch of horology, the apprentices receive lessons in their own language, arithmetic, algebra, geometry, physics, chemistry, mechanics, and drawing, in so far as they relate to horology.

The only system remaining for consideration is that of half-time schools; the system has, however, been almost discarded in this country, and has only been partially tried in France. One radical defect in it is, that there is no correlation between the work done in the factory and the information imparted by the schoolmaster; the whole of the pupils, whether they are intended to be mechanics, dyers, or printers, all receive the same kind and quantity of instruction.

So much for the good work that is being done in France, which of all European nations is certainly in the van with regard to lower technical education.

In September last Prof. Thompson visited Germany in compliance with the advice of Mr. Mundella, who, in criticising his paper at the British Association, placed the German technical schools above the French. Prof. Thompson paid visits to the Polytechnicum and Weaving Schools at Chemnitz, these being the special establishments pointed out by Mr. Mundella, and found, as he expected, that although the higher technical training schools in Germany were superior to those elsewhere, they could show nothing in any way equal to the Paris Municipal Apprentice School described above.

Prof. Thompson's investigations have been so thorough, and lead to such practical conclusions, that they should receive the serious consideration of those whose business it will be to organise either national or local systems of technical education.

The movement of the City Companies has resulted in the setting aside annually of £15,000 for the promotion of tech-

nical education, and there has been duly constituted "The City and Guilds of London Institute for the Establishment of Evening Classes for Technical Education, or the Application of Science to Industry." Twelve lectures on "Some of the Practical Applications of Electricity and Magnetism," by Mr. W. E. Ayrton, A.M., Inst. C.E., and twelve lectures on "The First Principles of Chemistry," by Prof. H. E. Armstrong, Ph.D., F.R.S., are now in course of delivery. Subsequent courses of lectures on "The Elementary Principles of Mechanics exemplified in our Clocks and Watches," on "The Applications of the Laws of Heat to the Steam and other Engines, and on "Inorganic Chemistry with especial reference to its Technical Applications," have already been arranged.

We trust this example will be followed in our large manufacturing towns, and that when the best system of imparting technical education has been determined, no red-tapeism will hinder it from being speedily and universally adopted. England will then soon regain her former position. Even the Japanese have, as Mr. Ayrton remarks, set us an example that our ambition should lead us to emulate. There has grown up, in the very midst of a people who a few years ago were almost in a state of slavery, a technical college, with its staff of carefully chosen English professors, with its laboratories, class-rooms, museums, libraries, and workshops, costing for maintenance an annual sum of £12,000. To study at this college neither money nor position is necessary; ability and a desire for knowledge are the only qualifications.

IV. ACCIDENTS RESULTING FROM THE HEAT OF THE COMSTOCK MINES.

IN the "Journal of Science" for March last we gave an abstract of a paper on "The Heat of the Comstock Mines," by Prof. John A. Church. The heat in the lower levels of these mines appears to surpass in intensity that of any other mines in the world. Prof. Church's explanation of the phenomena supposes the existence of a cold, and what may be called a burnt out, layer of rocks, extending 1000 feet below the surface; a zone of hot rock

still in active decomposition, which has been found to exist for a depth of about 1500 feet or more, and doubtless extends thousands of feet farther; and, finally, a mass of cold rock at a great depth, which has not yet begun to decompose. He refers the high temperature not to the internal heat of the earth, nor to the residual heat of the rocks, which were once melted, but to chemical action now maintained in the erupted rocks. This action is not a combustion, but the chemical alteration of the felspathic minerals of the propylite and other rocks, the peculiar bands of hot and cold rocks which Prof. Church describes, being layers of rock in which decomposition has been delayed and hastened.

Prof. G. F. Barker, who has also visited the mines, concludes,* on the other hand, that the heat is a hot-water heat, and that the waters are heated mechanically by those continuous movements of the country so plainly shown both in the mines and at the surface. But Prof. Church replies that although the Comstock discharges four and a half million tons of water yearly, not 1000 feet out of the 12 miles of linear excavation made every year are in ordinarily wet ground.

The object of this present article is not, however, to discuss the cause of the heat of the Comstock Mines, but to draw attention to the large percentage of accidents resulting from this intense heat. At a recent meeting of the American Institute of Mining Engineers, Prof. Church reviewed, at considerable length, these accidents and their relation to deep mining. From the report in the "Scientific American" we learn that during the twenty-two months preceding May, 1879, there were 101 accidents, killing 53 persons and wounding 70 others. These accidents are classified as follows:—(1) Falls of rock, timber, &c.; (2) Tramming; (3) Effects of heat; (4) Falls of men; (5) Explosions; (6) Hoisting apparatus; (7) Overwinding; (8) Miscellaneous.

In several instances miners have been fatally scalded by falling into the hot mine waters, which exhibit temperatures rising to 158° F. The most remarkable casualties, however, are due to the killing effect of labour in the hot and steaming atmosphere. The proportion of fatal casualties is larger in this class than in any other, being 73 per cent; and from the peculiar mental effects of the heat it is highly probable that it may be the real cause of many mishaps, which under other circumstances would be ascribed to culpable blundering.

On the 1900 level of the Gould and Curry mine a drift

* See Letter by G. F. Lesley in Nature, vol. xx., p. 168.

was run along and quite near to the black dike, one of the hot spots of the mine. At a spot where the thermometer marked at times 128° F., Thomas Brown fainted while at work. When taken to the surface and revived he was found to have completely lost his memory. He could not tell his name or where he lived, and had to be dressed and taken home by his friends. The newspaper which recorded the occurrence said that such sudden loss of memory from overheating was quite common in the mines, and suggested that the fact might furnish an explanation of the walking off into fatal winzes and chutes by experienced miners, seemingly with deliberate intention.

A frequent accident in these mines is fainting in the shaft while the cage is rising to the surface. The faintness is always felt immediately upon reaching the cooler air, 100 or 150 feet from the surface, where there is usually a side draught through some adit. This happens so often that a man who has been working in a hot drift is never allowed to go up alone. Long habitude to the heat is no safeguard against this danger, and serious accidents have occurred in this way.

Among minor casualties, Mr. Church mentions one which happened to Mr. Sutro, in the Sutro Tunnel, before it made a connection with the Savage mine. After spending some time in an air temperature of 110° F., Mr. Sutro went to the air-pipe to cool off. He stayed so long that the miners told him to get away from the pipe and let them have air. He did not move, and when they tried to stir him up with the handles of their shovels they found him unable to move. He had lost all volition, and had to be taken out on a car.

The graver results of overheating include insanity and death. The death of a carman on the 1400 level of the Caledonia mine, Gold Hill, March 11, 1878, is a case in point. He had been idle for six months, and that morning he was working his first shift. At an early hour he rushed into the station of the 1400 level, and reported that the wheels of his car were smashed. The station-master returned with him to his car, and found it all right. There was evidently something wrong with the man, and he was taken to a cooling place. Here decided mental aberration was discovered, and the man, firmly lashed to the cage, was hoisted to the surface, where he fainted at once and died in a few minutes. In this case the heat was only about 90° F.

In another case a miner died from cramps, attributed to

heat, but which may have been due to drinking ice-water; and another death is charged to a cold taken while cooling off after being partially overcome with heat. Though contrary to the rules of outside hygiene, the miners resort to copious draughts of ice-water or to exposure to strong cold air currents for recovery from overheating, and usually with impunity. The cold air cooling is considered the safer method; but to gain time Mr. Church commonly chose the ice-water, and never felt any ill effects from it. With several thousand cases a day of rapid cooling off by one or the other of these methods, it is surprising that fatal consequences have been so infrequent.

The next case illustrates the violent effects which excessive heat may have upon a person not accustomed to it:—"On Friday, October 11th, 1878, John McCauley went to work for the first time in the Imperial Mine. He was cautioned against over-exerting himself in the extreme heat of the lower levels. He replied that he thought he was strong enough to stand anything, and paid no attention to the advice. At half-past two in the afternoon he was brought to the surface in an unconscious state, and died the next morning at half-past ten o'clock."

Two other cases very similar to this have occurred in the Imperial within a few years. This mine is excavated in one of the hot spots of the Comstock.

The hot drift on the 1900 level of the Gould and Curry is the scene of the most serious of these casualties due to heat. Five men were sent there in June, 1878, to load a donkey-pump on a car. The work was so exhausting that when the pump caught on a plank they were not able to move it. They seem to have been in a state of mental confusion, but felt that they could not remain longer. Starting up a winze which connects with the 1700 level one man fell on the way, and the others were afraid to stop to help him, but pressed on, reaching the 1700 level in half an hour from the time they left it. They were very confused and nearly speechless, and hardly realised what had occurred. Three men went down to the rescue, and found the fallen man still alive. Clearing the pump they got into the car and signalled to hoist, but on the way up the winze the man they had gone to rescue reeled and fell off. The car was stopped at once, but he was jammed between it and the brattice so fast that the others left him and went for help. They all gave out, two half-way up, and the other just as he reached the 1700 level, where a friendly hand pulled him up. A new rescue party went down and found two men

dead, and the third died soon after. The shift boss reports that "the accident was due solely to the heat, as the air is good enough and pure enough, barring the heat." The winze was not an abandoned one, but in daily use. A heavy volume of steam is reported to rise through it from the 1900 level, the temperature of which, at the time of this accident, is given at 128° F. Mr. Church gathers from the detailed account that the death of the men is possibly attributable to the fact that when the miner fell off the car the latter was stopped in a place that was hotter than the rest of the winze.

It is to be regretted that no adequate studies have been made upon the precise physiological phenomena presented by death under these circumstances. The legal requirements are satisfied when it is proved that the casualties are due to heat.

V. ARE THE CHEMICAL ELEMENTS SIMPLE BODIES?

THE researches which have been made in the various branches of physical science during the last few years have many of them been of unusual importance. Some—the telephone and microphone, for instance—have already resulted in the invention of instruments of great practical utility, while others have opened up new fields for research, and bid fair to effect a complete reversal of the generally-accepted notions concerning Matter. As Prof. W. B. Rogers remarked, in his Presidential Address at the 1879 Meeting of the New York Academy of Sciences—"In all branches of discovery we seem to be catching the clews of far-reaching thought that stretch out where as yet no man's foot has trodden. The recent developments in Chemistry, through the agency of the spectroscope, and the effects of heat in dissociation, have suggested, if they have not proved, that a number of the substances hitherto regarded as elements are hereafter to be regarded as compound. The investigation of the laws of chemical action following out the suggestions made at the beginning of the century by

the great chemist Berthollet, in regard to the influence of man on chemical reaction, seems to promise most important discoveries in chemical statics and the possibility of applying mathematical reasoning and formulæ to chemical activities. The marvellous series of experiments presented recently by Crookes, in which have been exhibited the wholly unexpected phenomena which he has described under the designation of what was first referred to by Faraday as a fourth form of matter, which this illustrious experimentalist called radiant matter, seemed to open up a field of research and speculation until now wholly undreamed of. In truth, the active scientific workers have now been brought by their refined and novel researches to touch the near extremities of innumerable lines of thought and investigation, stretching out into unknown regions, whose exploration is to occupy the activity and reward the labours of a coming generation."

In discussing the question whether or not the study of the spectrum has thrown, or is likely to throw, any light on the ultimate constitution of matter, Dr. Balfour Stewart* recently referred to the fact that Prout first pointed out that the atomic weights of the so-called elements are very nearly all multiples of the half of that of hydrogen, so that the various elements may possibly be looked upon as formed by a grouping together of certain atoms of half the mass of the hydrogen atom. A most remarkable series of experiments was conducted by M. Stas to test this doctrine. He came to the conclusion that the atomic weights of the various elements were not precisely multiples of the half of that of hydrogen, there being greater differences than could possibly be accounted for by errors of experiment. But Dr. Stewart, calling attention to the great difficulty of obtaining substances absolutely free from all impurity, does not admit that Stas's researches settled the point in the negative, and points to the spectrum as a likely means of throwing some light on the question. Now Mr. Lockyer's researches tend to show that at sufficiently high temperatures the so-called chemical elements, or at all events some of them, are compound bodies. These researches were not undertaken with the view of decomposing the elements. Mr. Lockyer was preparing a map of the solar spectrum on a large scale, and the work included a comparison of the Fraünhofer lines with those visible in the spectrum of the vapour of each of the metallic elements

* Nature. "On some Points in the History of Spectrum Analysis: an Address to the Natural Philosophy Classes at Owens College, Manchester, November, 1879."

in the electric arc. (The complete spectrum of the sun, on the scale of the working map, will be half a furlong long: in mapping the metallic lines and purifying the spectra more than 100,000 observations were made, and about 2000 photographs taken.) The final discussion of the complete photographs of the spectra of the metallic elements compared with the spectrum of the sun led to the results which are exciting so much interest at the present time.

There are certain lines in the spectra of each element which appear long and thick; and it has been found that while such a line, for instance, is exceedingly prominent in some one element, other elements appear to possess it, only not nearly so prominently. Mr. Lockyer argued from this, that on the assumption that the elements are truly elementary, the line in the other elements was caused by traces of impurity. He has, however, found that there are coincidences in the lines of metallic spectra of two perfectly distinct kinds. There are coincidences of lines which are not the prominent lines of any one spectrum, and they give no signs of that variability of brightness that might be expected to characterise lines due to impurities: these lines he has called basic lines. In Mr. Lockyer's own words, "The temperature of the sun and the electric arc is high enough to dissociate some of the so-called chemical elements, and give us a glimpse of the spectra of their bases, just as in the case of the various salts of calcium there is a temperature which just allows us to get a glimpse of a line indicating the metal calcium common to them all."

To determine whether these basic lines varied in their behaviour from other lines of spectra taken at random, Mr. Lockyer had recourse to the spectrum of sun-spots. The sun is surrounded by an enormous atmosphere which is supposed to contain the vapours of such metals as iron and magnesium; but whether this be so or not, Mr. Lockyer argued that the atmosphere would certainly be hotter at bottom—nearer the photosphere—than higher up, and that the spectrum of the atmosphere close to the photosphere would differ from that of any higher region, and therefore from the general spectrum of the sun. In observing the spectrum of a sun-spot, or a prominence, the spectrum of an isolated mass of vapours in the hottest region open to inquiry can be determined, and Mr. Lockyer found that this spectrum differed greatly from the general spectrum of the sun. The whole character of the spectrum of iron, for instance, is changed when we pass from the iron lines seen among the Fraünhofer lines to those seen among the spot

and storm-lines. As therefore the spectra of spots and prominences are from the hottest region of the sun, Mr. Lockyer employed them for testing these basic lines. The result has led him to conclude that their appearance in two or more spectra is dependent solely upon high temperature. The basic lines are more prominent in the spectra of spots than in the spectrum of the sun; they are, moreover, more prominent at epochs of sun-spot maximum than during times of minimum. The statement, then, that the spectrum of each element consists only of lines special to that element, is found to be insufficient when the highest temperatures and the greatest dispersions are employed.

A clearer conception of what is meant by the so-called elements being split up at a very high temperature will be gathered from the following illustration, given by Dr. Balfour Stewart:—"If we apply a very powerful source of electricity, we obtain certain peculiar lines from the vapour of calcium. Now if we could catch hold of and segregate—put into a box, as it were—all these minute entities that give us this suspicious line at a high temperature, and, further, if we could keep their high temperature up, I think it is probable that we might obtain something which is not calcium, or, at any rate, something simpler than the molecule of calcium as this appears at lower temperatures. But we are not yet able, and perhaps we may never be able, at an ordinary temperature, to present the chemist with some other substance derived from calcium, which is not calcium."

Much, of course, remains to be done before Mr. Lockyer's views will be universally accepted. The indecomposable character of the bodies which we have hitherto considered elementary has for a long time been considered doubtful. Mr. Lockyer, however, is the first to come boldly forward with experimental proof. What the final result of the discovery will be we cannot foretell. A physical problem, as Dr. Stewart says, begins like a rivulet. At its first introduction it is small and seemingly unimportant; constantly, however, as it winds along it receives accessions from various quarters, until at length it becomes a mighty river that is finally merged in the unfathomable ocean. This course is followed by all such problems. Each begins small, grows broader, and will finally bear us on to the unknown if we trust ourselves to its guidance.

VI. DARWINISM AND ARTICULATE SPEECH.

AT the Meeting of the Academy of Sciences, on the 10th November, 1879, M. Larrey referred to a work written by Dr. Frederic Bateman, of Norwich, on the subject of Darwinism. In the course of his remarks M. Larrey said that Dr. Bateman combats Mr. Darwin's assertion that a difference between man and animals is only one of degree, and not of kind. He has transferred the subject of Evolution to the domain of Psychology, being convinced that hitherto naturalists have concentrated their attention too exclusively on the analogies between the body of man and that of animals, or, in other words, on the purely physical, anatomical, and material characteristics, neglecting the study of the intellectual and metaphysical attributes, which constitute an essential difference between man and animals.

Whilst admitting that man, in his purely physical nature, is closely allied to certain animals, Dr. Bateman repudiates entirely the conclusion drawn from that analogy by Mr. Darwin; for supposing the resemblance of man to an animal bone for bone, nerve for nerve, muscle for muscle, to be proved, what are we to conclude from it if it is also demonstrated that man possesses a distinctive attribute the least trace of which is not found in a brute, an attribute which establishes a gulf between the two? The author affirms that such a distinctive attribute exists in articulate speech. He examines first the doctrine of Darwinism, beginning with an exposition of the principles of Evolution as laid down by Haeckel. He then asserts that no proof exists of the transmutation of species since historic times, and cites the cases of bodies embalmed for three thousand years, and of the birds and animals carved on the ancient monuments of Egypt. He thus confirms what Flourens had already said, viz., "*The species do not alter nor pass from one to the other; the species are fixed.*"

Dr. Bateman then introduces the study of speech as an additional argument against the theory of Evolution, and explains his plan of attack on Mr. Darwin by the three following propositions;—

1. Articulate speech is the distinctive attribute of man, whilst the ape and other animals possess no such power.

2. Articulate speech is a universal attribute of man, and all the races have a language or the capacity of acquiring one.
3. The faculty is immaterial.

Dr. Bateman also discusses and criticises the views of the German neologists on Life-matter and Force, ending with some remarks on the mysteries of life itself.

VII. A REMARKABLE WATER AND GAS GEYSER.

As early as 1833 a salt well, drilled in the valley of the Ohio, threw columns of water and gas at intervals of ten to twelve hours to heights varying from 50 to 100 feet. A number of wells in the oil regions have since then thrown water geysers, but none have attracted so much attention as one situated near Kane.

The columns are composed of mingled water and gas, the latter being readily ignited. After nightfall the spectacle is grand. The antagonistic elements of fire and water are so promiscuously blended that each seems to be fighting for the mastery. At one moment the flame is almost entirely extinguished, only to burst forth at the next instant with increased energy and greater brilliancy. During sunshine the sprays form an artificial rainbow, and in winter the columns become encased in huge transparent ice chimneys.

The cause of the action is thus explained by Mr. C. A. Ashburner, in "Stowell's Petroleum Reporter":—

This well is situated in the valley of Wilson's Run, near the line of the Philadelphia and Erie Railroad, 4 miles south-east from Kane. It was drilled by Messrs. Gruhout and Taylor, in the spring of 1878, to a total depth of 2000 feet. No petroleum was found in paying quantities, and the casing was drawn and the hole abandoned, since which time it has been throwing periodically—every ten to fifteen minutes—a column of water and gas to heights varying from 100 to 150 feet.

During the operation of drilling, fresh "water-veins" were encountered down to a depth of 364 feet, which was the limit of the casing. At a depth of 1415 feet a very heavy "gas vein" was struck. This gas was permitted a

free escape during the time the drilling was continued to 2000 feet.

When the well was abandoned, from failure to find oil, and the casing drawn, the fresh water flowed into the well, and the conflict between the water and gas commenced, rendering the well an object of great interest. The water flows into the well on top of the gas, until the pressure of the confined gas becomes greater than the weight of the superincumbent water, when an explosion takes place, and a column of water and gas is thrown to a great height. This occurs at present at regular intervals of thirteen minutes, and the spouting continues for a minute and a half. On July 31st, 1879, Mr. Sheafer (aid, McKean County) measured two columns, which went to heights respectively of 120 and 128 feet. On the evening of August 2nd Mr. Ashburton measured four columns in succession, and the water was thrown to the following heights:—108, 132, 120, and 138 feet.

VIII. THE TRANSIT OF VENUS IN 1874.*

THE following is a complete transcript of Mr. Hennessey's observatory notes:—"This record was obtained by means of the following agency. My friend, Mr. W. H. Cole, M.A., audibly counted seconds, and named the minutes as completed, from a large chronometer, before which he was comfortably seated, say 6 feet from the equatoreal. Baboo Cally Mohun Ghose, with paper and pencil, took up a position by my side; he mentally followed Mr. Cole's counting, and noted down the instant I made a remark, together with the words I uttered. In the absence of remarks, the Baboo noted the number of each complete minute as it was declared, a reckoning which was checked by inspection of the chronometer and otherwise from time to time, so as to render the adoption of a *wrong* minute practically impossible. Thus all that remained for me to do was to look intently through the telescope of the equatoreal and declare exactly what I beheld. We three individuals were

* "Further Particulars of the Transit of Venus across the Sun, December 9, 1874; observed on the Himalaya Mountains, Mussoorie, at Mary-Villa Station, Lat. 30° 28′ N., Long. 78° 3′ E.; height above sea 6765 feet, with the Royal Society's 5-inch Equatoreal." By J. B. N. HENNESSEY, F.R.S. Abstract of Paper read before the Royal Society, November 20, 1879.

all enclosed within the canvas walling of an observatory tent only 8 feet square, the top of the tent being removed. No sound broke the enforced stillness that prevailed; the sky was brilliantly clear, and all cause for anxiety as to clouding over, or the driving of the equatoreal, or on any other account, was completely absent; while the events in question were plainly seen to progress so gradually and deliberately that there was not the smallest occasion for hurry or confusion of any kind, nor did any occur. Unlike a solar eclipse, when every second is of the utmost importance, there was plenty of time for every purpose. Under these circumstances it was practically impossible that any blunder in recording could be committed.

Observatory Notes.

1874. December. Chronometer Time. d. h. m. s.	1874. December. Greenwich Time. d. h. m. s.	Remarks made by Observer at the time.
		Ingress.
		Beautifully clear morning. No haze. Eyepiece 125. Two coloured glasses giving a neutral or bluish field. Missed the first external contact.
8 19 18 0	8 14 6 35	Venus's edge on sun boils somewhat; it is, however, distinctly visible.
20 0	8 35	Venus's outer edge, *i.e.*, against sky, distinctly visible, because of a narrow edging, or ring of light, around some 30° of that part of it farthest from the sun.
20 50	9 25	The light ring now extends around the whole of that part of Venus's edge against the sky, and is some 3″ in width; it is brightest where farthest from the sun.
22 33	11 8	The light ring against sky is well seen, and looks undeniably like Venus's atmosphere; width some 4″; definition excellent.
23 0	11 35	The light ring against the sky can be plainly traced *in continuation* around Venus on the sun, where, however, it is made visible chiefly by the *movements* occurring in it of minute bright specks, and also because it boils. The entire ring is full of these minute bright specks, which appear and disappear rapidly, dancing about with little flashes. Definition excellent.
24 28	13 3	Light ring against sky wider.
25 10	13 45	,, ,, quite distinct.
26 26	15 1	,, ,, quite bright.
28 34	17 9	Transit of Venus's dark edge across the sun's limb, *i.e.*, first internal contact.

1874. December. Chronometer Time. d. h. m. s.	1874. December. Greenwich Time. d. h. m. s.	Remarks made by Observer at the time.
		Ingress.
29 10	17 45	Transit, by estimate, of outer edge of Venus's light ring (that was against sky) across the sun's limb. No ligament or pear-drop seen, *though expected and carefully watched for.*
22 0 0	8 16 48 36	Brilliantly clear sky; light breeze from south.
32 0	17 20 36	Spectroscope set up, adjusted, and used. Slit placed across Venus's *centre* gave a black band all along the length of solar spectrum. Slit placed tangential to Venus's disk gave a faint, narrow, glimmer of light, slightly brighter than solar spectrum, instead of black band; no displacement of solar lines in glimmer.
		Egress.
		Eyepiece as before, 125. Of the two coloured glasses used for Ingress, one now changed for a red glass; field now red and less lighted than at Ingress. Definition sharp and excellent. No boiling visible around sun's limb; some boiling round Venus's lower limb, and still less around upper limb. Removed the red glass and substituted a blue one, when the boiling round Venus became quite visible. Removed the temporary substitute and restored red glass, as before, and thus watched the further progress of the transit.
8 23 9 0	8 17 57 36	About one-fourth of Venus's diameter from contact. No ligament visible.
11 38	18 0 14	About one-sixth of Venus's diameter from contact. No ligament visible.
12 40	1 16	About one-eighth of Venus's diameter from contact. No ligament visible.
15 8	3 44	Less than one-twelfth of Venus's diameter from contact. No ligament visible.
16 0	4 36	About one-twentieth of Venus's diameter from contact. No ligament visible.
16 57	5 32·6	Second internal contact.
22 0	10 36	Light ring now visible around Venus's limb against sky.
34 0	22 36	No boiling around Venus's or sun's limb.
37 30	26 6	Venus's limb against sky only faintly seen.
40 0	28 36	Light ring has been invisible for some time; was seen for only a short time after second internal contact.
44 14	32 49·6	Second external contact. No ligament visible thereafter or at any time before.

The following is an excerpt from a letter by the late Rev. H. D. James to Mr. Hennessey, who was stationed 19 miles from him to the north-west :—

"Chakatra, N.W.P., December 9, 1874.

"Through your kindness in sending me a diagram of the planet's progress across the sun's disk I had as satisfactory a view of it as was possible. My son was with me. At 6.56 we noticed the sunlight on the snowy range. Ten minutes and twelve seconds after that we saw the rim of the sun rising above the mountain which intervened between us and the horizon. We keep an eye on Mussoorie and Landour, and thought they had but two minutes' advance of us in seeing the sun. Eighteen minutes and ten seconds after our first glimpse of the sunlight Venus began nibbling at the rim of the sun. Between this and her entire entrance on to the disk was twenty-seven minutes and ten seconds; that is, we saw her external contact at 7° 14′ 10″, and her internal contact at 7° 41′ 20″. When she was about half-way on, we both noticed a fringe of white light illuminating that rim of the planet which was yet on the dark sky. When she went off we noticed the same fringe of light, but for a much shorter time, and when only about one-eighth of her had passed the sun's disk. The internal contact for departure took place at 11° 30′ 15″, and the external at 11° 57′ 25, as near as it was possible to say, for there was a sort of flickering, which rendered it difficult to fix on the precise moment of contact.

"The spots on the sun were but insignificant, in magnitude about the same as yesterday, though in position very different."

NOTICES OF BOOKS.

A Handbook of Double Stars, with a Catalogue of twelve hundred Double Stars and extensive Lists of Measures. With Additional Notes, bringing the Measures up to 1879. For the use of Amateurs. By EDWARD CROSSLEY, F.R.A.S., JOSEPH GLEDHILL, F.R.A.S., and JAMES M. WILSON, M.A., F.R.A.S. London: Macmillan and Co. 1879.

IT is said that one-fourth of the stars are multiple stars, and yet their study dates only from the time when the telescope was first used in astronomical research. Thousands of observations have since been made, and much has been written on the subject. There is, however, no convenient handbook for the guidance of students of sidereal astronomy. The present work is written in order that future students may know what stars are of especial interest, at what time observations are especially needful, and what stars have been so frequently and satisfactorily measured that for the present day they need no attention.

The book is divided into four parts. The first part is historical, and descriptive of instruments and methods; the second is mathematical; the third contains lists of measures of the most interesting double and multiple stars, with historical notes on those which are of special interest; the fourth part is bibliographical.

Part I., Chapter II., contains an excellent description of the Equatorial; the Clock, its construction and adjustments; the Observing Chair; and of the best form of Observatory. Chapter III. gives an account of the Equatorials which have been used by double-star observers. Chapter IV. is on the Micrometer; and Chapter V. describes the methods of observing, &c.

The first and second chapters of the second part give a detailed account, with a fully worked example, of determining an orbit and an ephemeris by a purely graphical construction founded on Herschel's and Thiele's methods. An example of the application of analysis to a double-star orbit already approximately known by graphical methods is given in Chapter IV. Chapter V. is on the effects of proper motion and parallactic motion; and Chapter VI. is on the mode of combining observations and determining their weight.

Part III. contains a catalogue of double stars selected as of special interest, with a list of all accessible measures.

The work is illustrated, and is written in such a simple style that the most uninitiated student will have little difficulty in mastering the subject, while it will prove a most valuable work of reference to the experienced astronomer.

PROCEEDINGS OF SOCIETIES

Astronomical Society, *Nov.* 19.—Lord Lindsay, President, in the chair.

Prof. Adams read a paper "On the Ellipticity of Mars and its Effect on the Motion of the Satellites." He said that Prof. Asaph Hall's observations showed that the orbits of both satellites are inclined at small angles to the plane of the planet's equator. It became an interesting question to enquire whether this state of things is a permanent one. Mr. Marth had recently investigated the motion of the nodes of the orbits of the satellites on the orbit of the planet, which will be due to the disturbing action of the Sun, and he came to the conclusion that, if there is no force depending on the internal structure of Mars which modifies the Sun's action, the nodes of the orbits will be in opposition to each other a thousand years hence. But Prof. Adams argued that this motion would be materially affected by the disturbing force due to the ellipticity of the planet, which would cause the nodes of the satellites to retrograde on the plane of the planet's equator. The ellipticity of Mars is too small to be observed, but the motion of the nodes will probably afford a means of determining the ellipticity of the planet within very definite limits.

Mr. Stone read a paper "On the Evidence of a Past Connection between four widely separated Southern Stars, viz., ζ Toucani, ϵ Eridani, ζ^1 Reticuli, and ζ^2 Reticuli."

Mr. Christie read a paper by Prof. C. J. Draper "On a Photograph of the Solar Spectrum, showing dark lines coincident with oxygen lines.

Mr. De La Rue gave an account of a visit to the works of Messrs. Repsold, Hamburg, and described a conical pendulum for the driving-clock of an equatorial which was being made for Professor Winnecke, of Strasburg. The principle was similar to that of the balance-wheel and spring of a watch, for the elasticity of the rod of the pendulum was made use of instead of the force of gravity to resist the centrifugal force caused by the motion of the pendulum. Messrs. Repsold told him that when the driving weight of the clock was changed for another fifteen times as great, the variation and the rate of rotation only amounted to 0·04 per cent.

Dr. Schuster read a paper "On the Polarisation of the Corona."

The President said he had obtained an observation of Deimos at Dunecht. Mr. Christie also obtained observations of Deimos at Greenwich, made on the 4th and 18th of October.

Mr. Common stated that he had on five nights obtained observations and measures of both the satellites of Mars with his 37-inch reflector.

Capt. Noble showed two sketches of the planet Jupiter, showing the great elliptical red marking in the southern hemisphere of the planet.

Chemical Society, *Nov.* 20.—Dr. Gilbert, F.R.S., in the chair.

"A Chemical Study of Vegetable Albinism (Part II., Respiration and Transpiration of Albino Foliage)," by A. H. Church, M.A. The author has proved that white foliage does not possess the power, even in sunshine, of decomposing the carbon dioxide of the air, but adds largely to the normal amount of that gas in the air, thus resembling the petals of flowers and the action of green leaves during darkness. The best results were obtained with the maple (*Acer negundo*), the holly, the ivy, and the *Alocasia macrorhiza*. The author has also studied the comparative loss and gain of albino and green foliage. White holly sprays placed in water gained, in two hours, 0·29 per cent; green holly, under similar conditions, gained 1·55 per cent. When no water was supplied the white holly lost 0·54 per cent; the green, 10·26 per cent. The author promises further work on this interesting subject. He exhibited some dry specimens of albino and ordinary leaves, the albino leaves being thinner and altogether much less substantial in their structure.

Geological Society.—The first meeting of the present Session was held on November 5th; the President, Mr. H. C. Sorby, F.R.S., presiding.

The papers read included one by Robert Mallet, F.R.S., "On the Probable Temperature of the Primordial Ocean of our Globe." According to the latest hypotheses as to the quantity of water on the globe, its pressure, if evenly distributed, would be equal to a barometric pressure of 204·74 atmospheres. Accordingly water, when first it began to condense on the surface of the globe, would condense at a much higher temperature than the present boiling-point, under ordinary circumstances. The first drops of water formed on the cooling surface of the globe may not impossibly have been at the temperature of molten lead. As the water was precipitated, condensation of the remaining vapour took place at a lower temperature. The primordial atmosphere would be more oblate and less penetrable by solar heat than the present, and the difference of temperature between polar and equatorial regions would be greater; so that, in the later geologic times, ice may have formed in the one, while the other was too hot for animal or vegetable life. Thus, formerly the ocean would be a more powerful disintegrant and solvent of rocks, mineral changes

would be more rapid, and meteoric agencies would produce greater effects in a given time.

A discussion ensued, in which the President, Mr. Evans, Prof. Prestwich, Dr. Hicks, Prof. Bonney, and Capt. Galton took part. In replying, Mr. Mallet said he did not suppose any part of the original crust of the globe remained at present visible at the surface. Such geological deductions as were made in his paper were only illustrative, and might be open to question. The epoch at which the phenomena occurred to which his paper referred was long anterior to the existence of either animal or vegetable life upon our globe. What he affirmed as certain was that the method he had indicated, requiring for its data a more extended experimental knowledge of the relations between temperature and pressure in aqueous vapour, and a more exact knowledge of the total volume of water now upon our terraqueous globe, affords the means of determining the temperature of our oceanic water at every period, from that of the primordial ocean to our own day.

A paper "On the Fish-remains found in the Cannel Coal in the Middle Coal-measures of the West Riding of Yorkshire, with the description of some New Species," was contributed by James W. Davis. The remains described by the author were from a bed of Cannel coal about 400 feet from the base of the Middle Coal-measures, and were chiefly obtained from this bed at the Tingley Colliery. The author described the general geological structure of the district. The following are described as new forms:—(1) *Compsacanthus triangularis*, (2) *C. major*, and (3) *Ostracacanthus dilatatus*, the type of a new genus resembling *Byssacanthus*, Agass.

Prof. R. Owen, C.B., F.R.S., read a paper "On the Skull of *Argillornis longipennis*, Owen," in which he described a fragmentary cranium from the London Clay of Sheppey, from which it was procured by W. H. Shrubsole, Esq.

Institution of Civil Engineers.—At the second meeting of the Session, held on the 18th of November, Mr. W. H. Barlow, F.R.S., Vice-President, in the chair, the paper read was on "Tunnel Outlets from Storage Reservoirs," by Mr. C. J. Wood, M. Inst. C.E. The subject was divided into the consideration of culverts in a direct line through an embankment, and tunnels round the end of the embankment as a distinct work. The author advocated the tunnel clear of the embankment, and valve-towers of masonry and iron, as the best and safest, if more costly, plan. The paper contained some remarks on the works for the supply of water to Bradford, Yorkshire, which was obtained from three districts. A High Level Supply was taken from the hills above Oxenhope; an Intermediate Supply from Manywell Springs, and a Low Level Supply from Skipton district. The high-level reservoirs afforded instances of tunnel

outlets. The Stubden Reservoir had been a cause of anxiety, the original culvert of masonry proving leaky. It had been abandoned in favour of a tunnel lined with cast-iron plates backed with concrete, terminating in a cast-iron valve-tower, erected from the design and under the superintendence of Mr. A. R. Binnie, M. Inst. C.E. The embankment was cut down, the original culvert taken out, and the embankment was then made up solid in thin layers, the new tunnel being used to run water off during the reconstruction of the work. No leakage of any description had since been noticed, the valve-tower and tunnel being water-tight. The Leeming Compensation Reservoir for mill-owners had been originally made with a circular culvert having a slip joint and iron shield where it crossed the puddle-trench. This culvert was seriously fractured by the subsidence of the bank, which was mostly on a clay foundation, and it was considered unsafe to fill the reservoir. On the suggestion of Mr. Rawlinson, C.B., M. Inst. C.E., and of Mr. Binnie, on the latter taking charge of the Bradford Corporation Water-Works, the old culvert was abandoned, and a new tunnel was substituted, similar to the one at Stubden. The original culvert was afterwards filled on the inside with concrete, and the reservoir was successfully filled with water, no leakage or disturbance having occurred up to the present time. In the case of the Leeshaw Reservoir, which was in course of construction, on Mr. Rawlinson's inspection it was resolved to abandon the culvert already cracked, and to substitute a tunnel outlet and valve-tower of cast-iron, similar to those at Leeming and Stubden: this had been successfully done, the reservoir filled, and the work had since proved most satisfactory. In two of the above cases the culverts were so damaged as to be a strong argument in favour of tunnel outlets distinct from the embankment.

Physical Society.—The first meeting of this Society for the Session 1879-80 was held on Saturday, November 8th, Prof. W. G. Adams in the chair.

The first paper read was "On an Analogy between the Conductivity for Heat and the Induction Balance Effect of Copper-tin Alloys," by W. Chandler Roberts, F.R.S. Mr. Roberts traced a remarkable resemblance between a curve representing the induction balance effect of the copper-tin alloys published by him in June last, and the curve of Calvert and Johnston for the conductivity of heat, and, on the other hand, he showed that the induction curve does not agree with Matthiessen's curve for the electric conductivity of the same alloys. In the course of his paper Mr. Roberts expressed a hope that Prof. Hughes's beautiful instrument will enable us to determine whether the relation between conductivity for heat and electricity is really as exact as it has hitherto been supposed to be.

Capt. Armstrong exhibited a standard Daniell cell formed of a porcelain vessel with a porous partition dividing it into two compartments. In one the zinc plate was immersed in a solution of sulphate of zinc; in the other the copper plate in a solution of sulphate of copper. To use the cell as a standard, it was only necessary to connect the two liquids by a cotton string moistened with water. This arrangement prevented mixing of the liquids, as the string could be withdrawn after use. The resistance was high, but it was a constant standard of electromotive force.

Prof. Guthrie mentioned that Prof. Pirani, of Melbourne, in a letter to him, had signalled the fact that when a dilute acid was being electrolysed, the positive electrode, if made of iron, became incandescent below the surface of the liquid. Prof. Guthrie had found this to be true not only for iron, but for other metals, and that it could hardly be due to oxidation, because it took place not only at the cathode or positive electrode, where oxygen was evolved, but also at the anode, where hydrogen was evolved. The incandescence appeared to him to be due rather to resistance. The author exhibited his experimental results, which he did not doubt had already been obtained by Prof. Pirani himself. The positive electrode when immersed in the electrolyte was seen to get red-hot and to wobble about. As the liquid heated the red glow became fainter. The negative electrode, on the other hand, emitted a bright light, accompanied by a sputtering noise. The light was tinged with the characteristic colour of thc flame of the metal of which it was composed; in the case of a copper electrode, for example, it was greenish. These effects were shown by Prof. Guthrie with iron, copper, and platinum electrodes, in dilute sulphuric and dilute nitric acids.

November 22, 1879.—Prof. W. G. Adams in the chair.

Prof. Guthrie exhibited a new Image Retention Photometer, and demonstrated its action to the meeting.

Dr. Shettle, of Reading, read a paper on the "Influence of Heat upon certain forms of Induction Coils, considered more especially in relation to the Inductive Power which the Blood exercises on the Various Structures of the Body. The author found that when a copper and a zinc wire were insulated from each other by parchment-paper and paraffined silk, and wound in close proximity to each other, an (induced) current was indicated on a galvanometer whose terminals were connected to the neighbouring ends of the zinc and copper wires respectively, the other ends being left free. When the latter were connected across the deflection was *nil*. On raising the temperature of the two wires, by causing hot water to flow inside the coil into which they were wound, the deflection was largely increased. These experiments led Dr. Shettle to imagine that there is a similar action in the animal body. The heart is made up of nerves and muscular fibres winding spirally, and some of these wind round

each other so as to form a spiral cord, round which the blood capillaries also wind. Dr. Shettle compares these nerve and muscle bundles to the coils of zinc and copper wire in his experiments, and infers that electric currents may be induced in them as in the wires. The flow of the warm magnetic blood would also tend to produce currents in them. Dr. Shettle, further, drew attention to the fact that animals live and move in a magnetic field, and that electricity must be generated in them by their movements, internal and external.

Mr. Emmott exhibited Crossley's Form of Microphone, which consists of four short rods of carbon jointed loosely into four blocks of carbon, so as to form a square. It is used as a transmitter for telephones, and Mr. Crossley regularly transmits the services of a church with it to several hearers. Its speaking, singing, and whistling powers were successfully demonstrated to the meeting.

Royal Society, *Nov.* 20.—Dr. Spottiswoode, President, in the chair.

"Experimental Researches on the Electric Discharge with the Chloride of Silver Battery," by Warren De La Rue, M.A., D.C.L., F.R.S., and Hugo W. Müller, Ph.D., F.R.S., Part III. In the first part of this paper the authors describe a series of experiments to determine the potential necessary to produce a discharge in a tube, exhausted gradually more and more while using a constant number of cells in all the experiments. The tube employed was 33 inches long and 2 inches in diameter, the distance between the ring and straight wire terminals being 29·75 inches; the battery consisted of 11,000 cells. The next part of the paper deals with the potential necessary to produce a discharge between discs 1·5 inch in diameter, at a constant distance and at various pressures; the remainder being chiefly occupied with the study of the phenomena of the electric arc under various conditions of distance, pressure, and potential. The experiments were made in a bell-jar, containing the terminals, which could be gradually exhausted after having been filled with air or other gas. A remarkable phenomenon was observed on making connection between the terminals and the battery by means of the discharging key, namely, that within certain limits of pressure in the bell-jar a sudden expansion of the gas took place, and that as soon as the connection was broken the gas then as suddenly returned nearly, but not quite, to its original volume in consequence of a slight increase of temperature. The effect was exactly like that which would have been produced if an empty bladder had been suspended between the terminals, and suddenly inflated and as suddenly emptied. This is thought to be produced by a projection of the molecules by electrification causing them to press outwards against the walls of the containing vessel, this pressure being distinct from

the motion caused by heat. The appearances presented by the arc in air, hydrogen, and carbonic acid are very beautiful, and are copiously illustrated by copies, in mezzotint, of photographs and drawings. The authors conclude that for all gases there is a minimum pressure which offers the least resistance to the passage of an electric discharge. After the minimum has been reached, the resistance to a discharge rapidly increases as the pressure of the medium decreases. That the potential necessary to produce a discharge between parallel flat surfaces at a constant distance and various pressures, or at a constant pressure and various distances, may be represented by hyperbolic curves. The resistance of the discharge between parallel flat surfaces being as the number of molecules intervening between them. That the electric arc and the stratified discharge in vacuum tubes are modifications of the same phenomenon.

"Preliminary Note on Magnetic Circuits in Dynamo- and Magneto-Electric Machines," by Lord Elphinstone and Charles W. Vincent, F.R.S.E., F.C.S., F.I.C. The experiments which form the subject of this note were made in connection with an investigation as to the best form for the construction of a dynamo-electric machine, intended to furnish currents of high intensity in great quantity. The principles deduced applies equally, however, to magneto-electric machines. The authors show that electro-magnets do not lose their magnetism, as commonly supposed, immediately on cessation of the voltaic current, but that if the poles are "closed" by approximation of other magnets or armatures, the magnetism is retained for an indefinite time. A magnet 58lbs. weight could be lifted by its armature, and sparks obtained from the ends of the wires of its helix a week after magnetisation. The authors have constructed a dynamo-electric machine, in which the "closed magnetic circuit" is utilised, which they propose to describe in a future paper.

"Preliminary Report to the Committee on Solar Physics on the evidence in favour of the Existence of certain Short Periods common to Solar and Terrestrial Phenomena," by Balfour Stewart, F.R.S., and William Dodgson. In a previous Report to this Committee the authors exhibited a method of detecting the unknown inequalities of a series of observations, and gave some evidence that the temperature-range and the declination-range at Kew Observatory are both subject to the same short-period inequalities, the particular periods investigated being those around twenty-four days. In the present paper they investigate, after the method described in their first Report, the following ranges:—

I. Diurnal Ranges of Temperature of Air at—α Kew (already given); β Toronto; γ Utrecht.

II. Diurnal Ranges of Magnetic Declination, including Disturbances at—α Kew; β Prague.

III. Sun-spot Inequalities of the period of twenty-four days, as far as this may be obtained from existing records.

The various results of the present communication are summed up as follows:—

1. The temperature-ranges at Kew, Utrecht, and Toronto exhibit certain common periods (around twenty-four days).
2. Of the curves embodying those periods that of Kew is most like the mean, and that of Toronto least so.
3. Similar phases occur at Toronto eight days before they occur at Kew, and occur at Kew one day before they occur at Utrecht.
4. Correcting for these differences of phase, the individual inequalities of Kew, Utrecht, and Toronto are very like the mean of the three, and like each other.
5. Of these the Kew inequalities are most like the mean, and the Toronto least so.
6. The declination-ranges at Kew and Prague exhibit certain common periods which we may regard as the same, or very nearly the same, as the meteorological periods above indicated.
7. Similar magnetic phases occur at Kew about one day before they occur at Prague.
8. Correcting for this difference of phase, the individual inequalities at Kew and Prague are very like the mean of the two, and like each other.
9. There is also a less striking likeness between the various magnetical and the corresponding meteorological inequalities.
10. Provisional sun-spot records appear to show certain solar inequalities very like the magnetic and meteorological inequalites in point of period.

In conclusion, the authors express the hope that steps will be taken to ascertain what information exists in the world available for these researches, and also to bring such information together for the purpose of a further and more complete discussion of the laws now indicated.

"On the Action of Nuclei in producing the Sudden Solidification of Supersaturated Solutions of Glauber's Salt," by Charles Tomlinson, F.R.S. The author states the general conclusions at which he has arrived since resuming the study of this subject two years ago. In his former papers a nucleus is defined as a body that has a stronger adhesion for the salt, or for the liquid of a solution, than subsists between the salt and the liquid. The action of nuclei the author believes to be simply mechanical, and to be expressed by the familiar word adhesion.

"On the Normal Paraffins," Part III., by C. Schorlemmer, F.R.S. Mr. Schorlemmer is inclined to believe that petroleum consists chiefly of an inextricable mixture of isomeric and homo-

logous paraffins, in which, however, the normal hydrocarbons preponderate.

"On the Solubility of Solids in Gases," by J. B. Hannay, F.R.S.E., F.C.S., and James Hogarth. The authors show the phenomenon of a solid with no measurable gaseous pressure, dissolving in a gas, and not being affected by the passage of its menstruum through the critical point to the liquid state, showing it to be a true case of gaseous solution of a solid.

"The Geometric Mean, in Vital and Social Statistics," by Francis Galton, F.R.S. Mr. Galton's purpose is to show that an assumption which lies at the basis of the well-known law of "Frequency of Error" (commonly expressed by the formula $y=e^{-h^2x^2}$), is incorrect in many groups of vital and social phenomena, although that law has been applied to them by statisticians with partial success and corresponding convenience. Next he points out the correct hypothesis upon which a Law of Error suitable to these cases ought to be calculated. Mr. Galton also communicated a memoir by Mr. Donald McAlister, who, at his suggestion, has mathematically investigated the subject.

Royal Geographical Society, *Nov.* 24.—The Earl of Northbrook, President, in the chair.

"The Arctic Campaign of 1879 in the Barents Sea," by Capt. A. H. Markham, R.N. The year 1879, the author said, would always be regarded as a very remarkable one in the annals of Arctic exploration, for during the last few months two important geographical problems had been successfully solved: The Swedish ship Vega, under the leadership of that persevering and energetic explorer, Nordenskjold, had completed her voyage from the Atlantic to the Pacific, by rounding the northern outskirts of the old world—a voyage which, even if it should not prove important in a commercial sense, would always rank as one of the greatest geographical feats of the present century; secondly, a small sailing schooner had reached the hitherto inaccessible shores of Franz-Josef Land. The vessel which accomplished this feat was the Dutch exploring schooner Willem Barents. Three hundred years ago the Dutch flag was a formidable rival of the English in the van of Arctic discovery, and English discoverers were delighted that Holland had again entered the lists and obtained a great success. The importance of the achievement of the Willem Barents could not be too highly rated, for the achievement tided over the only difficulty which, in the opinion of a great many Arctic authorities, presented itself with respect to the route for exploration. Early in the present year the author accepted an invitation from Sir Henry Gore Booth to accompany him on a sporting trip to Novaya Zemlya, with the understanding that they should afterwards examine the ice in the Barents Sea and other localities,

during what he considered to be the navigable season—namely, during the month of September. For the cruise he had hired the little Norwegian cutter Isbjörn, already rendered historical by her exploits under Payer and Count Wilczek. The Isbjörn was a vessel of 48 tons burden, 55 feet long, with a beam of 17 feet. She was built in 1870, and made her first voyage in the following year with Weyprecht and Payer. The crew consisted of a skipper, mate, two harpooners, and five seamen. They met the first ice on the fourth of June at a distance of 40 miles from the part of Novaya Zemlya coast called Goose Land. The Matochkin Shar was then impassable, and they therefore cruised along the west coast of Novaya Zemlya until they were stopped by the ice off Cape Nassau on July 15. They succeeded in passing through the Matochkin Shar on July 31, and found the Kara Sea full of heavy ice. Returning they met the Dutch exploring vessel Willem Barents on August 18. Here Captain Markham bore testimony to the great kindness they received from the officers and crew of that vessel. Everything they had was placed at their disposal. They then again shaped a course northward, along the Novaya Zemlya coast, and this time succeeded in passing Cape Nassau, and in reaching Cape Mauritius—the extreme north-western point of that land—on September 6. Finally they pushed northward on a meridian midway between Novaya Zemlya and Spitzbergen, and on the 12th they met the ice in latitude 78° north, and longitude 47° east. They pushed on and penetrated through loose streams of ice to 78° 24′, about 80 miles from Franz-Josef Land, and six miles further than that attained by the Dutch expedition last year. Captain Markham had the honour of first carrying the British flag to the northward of Novaya Zemlya. His conclusion was, looking at all the voyages in that direction, that a steamer would have no difficulty in reaching the south coast of Franz-Josef Land during the last week in August, or during the first two weeks in September. If a vessel were once able to establish herself in winter quarters on the west coast of Zichy Land the results would certainly be satisfactory. So long as the land continued to trend northward with a western aspect either a ship or a sledge would follow it without great difficulty, even if it went as far as the Pole. What was wanted in Arctic exploration was perseverance. He trusted that next season a vessel would proceed to Franz-Josef Land prepared to winter, and if it had to return it should be sent out again the following year, and from that starting point attack the work of exploration. With regard to the results of the 1879 expedition, although geographical knowledge had not been greatly increased, yet the observations that had been made with regard to physical geography would prove of the utmost value in future explorations. The Franz-Josef Land route, in his opinion, was the one that would lead to the greatest discoveries in future. The specimens of rocks and

fossils brought home were sent to Mr. Etheridge for examination, the birds and eggs to Mr. Fielden, while the plants were sent to Dr. Hooker and Mr. Oliver. A discussion followed in which Count Bylandt, Sir Leopold M'Clintock, Sir George Nares, and Admiral Hamilton took part.

Paris.

Academy of Sciences, *November* 3.—The memoirs presented at this meeting included the following:—

"Result of the Researches made for the purpose of Discovering the Origin of Re-invasions of *Phylloxera*."—M. Faucon. —The author contends for the superiority of immersion to insecticides. The method he indicates is as follows:—On heavy lands, which hold water well, the immersion should be continued for 55 consecutive days; on moderately light soil, for 65 days; and on very light land, for 75 days. If the immersion is intended to destroy the eggs of *Phylloxera*, it must be carried on without the least interruption for 90 days on medium, and for 120 days on light, lands.

"A New Stellar Spectroscope."—M. L. Thollon.—M. Thollon has arrived at the conclusion that, in order not to repeat indefinitely experiments already made, three things are necessary, viz.:—(1.) That the place of observation should be as high as possible, and in a very favourable climate. (2.) That the greatest possible quantity of light should be concentrated on the slit of the spectroscope by means of an object-glass or a mirror with a broad surface. (3.) That a spectroscope should be used in which the loss of light is reduced as much as possible. It is to the solution of the latter problem that M. Thollon has specially directed his attention. He calculates the loss of light in an ordinary spectroscope to at least 57 per cent. The instrument he has invented contains two compound direct-vision prisms of special form, one in the collimator, the other in the telescope tube. The larger of the simple (or component) prisms has an angle of 100°, and contains a mixture of sulphide of carbon and ether. Two rectangular prisms of crown glass, one on either side, have faces parallel to each other and to the bisecting line of the angle of 100°. With this spectroscope M. Thollon has succeeded in economising light, and, to a certain degree, in making up for the loss that cannot be avoided.

NOTES

BIOLOGY.

THE effects of the bite of the skunk are exciting some discussion. In a Monograph of the North-American Mustelidæ, by Dr. Elliot Coues (see "Quarterly Journal of Science," viii., p. 138), we read that a wound from the teeth of this animal, when in its normal state of health, rarely fails to bring on hydrophobia. The "Forest and Stream" and the "Medical and Surgical Reporter" have more recently taken up the subject. Among the authorities they quote is Dr. Cushing, of Trinidad, Colorado, who gives it as his opinion that the natural bite of the skunk, when not suffering from rabies, never fails to bring on hydrophobia. Dr. W. L. South, who has had great experience in Texas and New Mexico, asserts that the bite sooner or later results in death. It is curious, however, that no similar cases have been recorded in Pennsylvania, where the skunk is very common.

The "Journal de Pharmacie et de Chimie" for October, 1879, contains a paper on the "Actions of Life without Air; their Influence on the Chemical Phenomena of Respiration," by M. Pasteur. The author is led to believe that in the animal economy there take place phenomena of the same order as in fermentation. Oxygen acts not only by effecting combustions, but gives to the cellules an activity whence they derive the power of action beyond the influence of free oxygen, in the manner of the ferment-cells. Direct combustions are of little importance.

At a Congress of Naturalists and Physicians, on the 23rd of September, Prof. Jäger, of Stuttgart, gave a demonstration on his method of examining odours (neural analysis) and on the odours themselves. The professor also spoke on the influence of the temper. Amidst boisterous merriment he appealed to all mothers present if children, when in a good humour, did not give off a pleasant odour, but a disagreeable one when cross or fretful. The lecturer ended amidst hisses.

MM. J. Macé and W. Nicati, in their researches on colour-blindness (described to the Academy of Sciences on Oct. 27th), have sought to obtain comparative measurements between the quantities of light perceived in the different parts of the spectrum by the colour-blind on the one hand, and by the normal eye on the other. A red glass which scarcely lessens the vision of the normal eye diminishes remarkably that of a red colour-blind eye.

MICROSCOPY.

Dr. Pelletan, in the "Journal de Micrographie," 3me année (No. 3, p. 139), complains of the little scientific value of the majority of microscopic objects prepared for sale. Preparations of diatoms are the most satisfactory as they are the easiest to make, and J. D. Möller's "Typenplatte" is especially commended by Dr. Pelletan; certain vegetable preparations, sections of dense substances, animal, vegetable, and mineral, are noticed as being very instructive. But the histological preparations, whether of man or other vertebrates, or of the invertebrates, are stated to be precisely those of least value. The usual insect preparations meet with but little favour from the writer; they present, he says, a magnificent appearance, but the integument is all that has been preserved, and the little that remains of the internal organs is represented by a uniform transparent mass, in which the microscopist finds nothing to study. Some of the English preparations mounted without pressure are very successful, particularly those of spiders, some of which show the internal organs very fairly. Complaint is made that the preparers of animal anatomical subjects have not sufficient knowledge to understand what it is necessary to make visible, and what is the characteristic detail to be rendered evident, in order to make the preparation instructive. They imagine it to be sufficient to take a piece of tissue, injected, or otherwise, harden it, make longitudinal and transverse sections, then steep it in some dye, mount it in a beautiful cell, and by these means obtain a slide useful for something. For example, in certain "commercial" histological preparations the writer notices dissociated muscular fibre, a torn nerve filament, a slice of conjunctive tissue, and the nerve terminations on a muscular fibre. What is learned? The muscular fibres have not been stretched, the sarcolemma is not evident, nor the nodes, nor the least detail of the striæ, discs, and transparent spaces; as to the other portions all is confusion, and nothing clearly displayed. The complaint is certainly true, but, it is to be feared, little will be done, as well made animal preparations are costly, and the purchasers few. Pretty objects are those which pay best.

Dr. John Matthews, F.R.M.S., Vice-President of the Quekett Club, has contrived a machine for making sections of such substances as bone, hard wood, ivory, nut, and other materials which are too hard to be cut with the section knife, and not of a nature to require the lapidary's wheel. The carriage holding the saw runs smoothly yet firmly between friction rollers, and derives its reciprocating motion from a crank, which can be turned either by hand, or when a higher speed is required, driven by a treadle and pulley. The saw is provided with

adjustments to secure its parallelism and proper tension; the thickness of the section is regulated by a screw of fifty threads to the inch, reading to thousandths by means of a micrometer head; the feed is either automatic by means of a cup, in which a suitable quantity of shot can be placed, acting by gravitation on a lever; or, as Dr. Matthews prefers, is capable of being regulated by hand. Owing to the steady motion of the saw when in proper adjustment sections of suitable tissues can be cut as thin as the thousandth of an inch; the surfaces show no trace of the saw cut, and are almost polished; very little after treatment is needed to remove the few scratches left by the saw, and if required for mounting in balsam it can be done at once, taking the usual precautions to prevent penetration, and the consequent obliteration of structure. The machine has been constructed with his usual care by Mr. J. W. Bailey.

Some interesting experiments on "Brownian Movements" have been communicated to the Royal Microscopical Society, by Wm. M. Ord, M.D. The paper, which does not admit of profitable abbreviation, will be found in vol. ii. of the Society's Transactions, p. 656.

The Royal Institution Christmas Lectures, adapted to a juvenile auditory, will this year be given by Professor Tyndall, the subject being "Water and Air." As full a report as possible will be given in the "Journal of Science."

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London: Printed by E. J. Davey, Boy Court, Ludgate Hill, E.C.